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OF THE

ROYAL SOCIETY

OF

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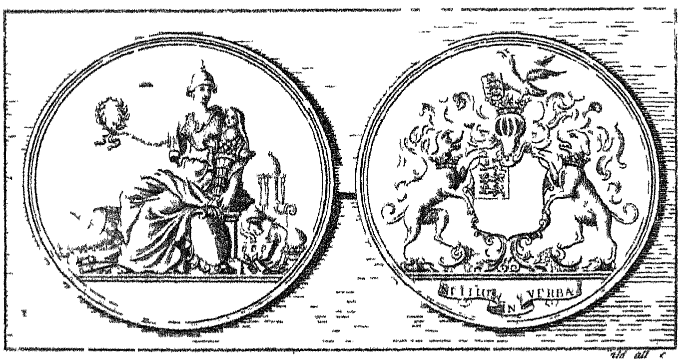
PART I.



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MDCCLXXIX.

A D V E R T I S E M E N T.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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PHILOSOPHICAL TRANSACTIONS.

I. *A Letter from Sir* William Hamilton, *K. B. F. R. S. to Sir* John Pringle, *Bart. P. R. S. giving an Account of certain Traces of Volcanos on the Banks of the* Rhine.

From on board a Yacht on the Rhine,
near Mayence, Sept. 29, 1777.

SIR,

Read Dec. 11, 1777.

AS I do not recollect ever to have heard of, or seen, any account of ancient volcanos on the banks of this river, I have the pleasure of sending you a few imperfect remarks, which I have

juſt made during a five-days moſt delightful paſſage up the Rhine from Bonn to Mayence. The firſt certain token of volcanos having exiſted in this country was evident to me in the court of the palace of the Elector-palatine at Duſſeldorff, which is at this moment new paving with a lava exactly like that of Etna and Veſuvius. Upon enquiry I was told, that it came from a quarry belonging to the ſame Elector at Unkel, between Bonn and Coblenz. When I arrived at the gates of Cologne, I was ſtruck with the ſight of numberleſs baſaltic columns inſerted in the walls of the town; and I remarked, that columns of the ſame ſort were univerſally uſed as poſts in the ſtreets, and at every door; they are chiefly pentagonal, but ſome are hexagonal, and a few have only four ſides; they are very like the baſaltes of the Giants Cauſeway, but without their regular articulations. I was informed, that they came likewiſe from the Unkel quarry; and that the town of Cologne is in poſſeſſion of an ancient right to as much ſtone from that quarry as may be wanted for its own uſe. I perceived likewiſe, that the walls of moſt of the ancient buildings in the town of Cologne were of a tuffa exactly reſembling that of Naples and its environs. This ſpecies of ſtone, as I was informed, abounded on the banks of the Rhine, between Bonn and Coblenz: theſe circumſtances

made me keep a ſharp look-out, and, on my approach to Bonn, was ſtruck with the volcanic forms of the Seven-bergen, or Seven Mountains, about two leagues from the town, on the other ſide of the Rhine. In the walls and ſtreets of Bonn are many of the above mentioned columns of baſaltes, and the pavement of the Town is of lava. The ſtone in general uſe for building here, is a very compact one, a hard volcanic tuffa like that of Pianura near Naples, and of the ſort called Piperno in Italy; it is ſomething like free-ſtone, but, upon near inſpection, is mixed with fragments of lava and other volcanic ſubſtances.

The day after my arrival at Bonn I viſited Wolckenberg, Tackenfelts, and Stromberg, three of the Sevenbergen, and found the two firſt entirely compoſed of tuffa, and the laſt of tuffa and lava: I dare ſay, by the ſhape and appearance of the reſt of theſe mountains, I ſhould have found them all equally compoſed of the ſame volcanic ſubſtances, had my time allowed me to have examined them. The craters on the mountains I viſited are diſcernable, though much altered, and filled up by time and the rubbiſh thrown from the quarries that are conſtantly worked on their tops. On each ſide of the Rhine, moſt of the way from Bonn to Coblenz, particularly between Prohl and Andernach, I perceived

high rocks of lava or tuffa. Where the volcanos had not operated, the mountains and rocks are of ſlate. At Erpel, in a mountain cloſe to the river, and oppoſite the convent ſituated on an iſland about three leagues from Bonn, there are ſome traces of baſaltic columns, the quarry ſeeming to have been nearly exhauſted. I have often thought (and this exhauſted quarry brings it to my mind again) that the reaſon why there are ſcarcely any remains of lavas that have taken the columnal form on Veſuvius and the volcanos near Naples is, that they have been carried off for the uſe of paving the great Roman roads. The Appian way is moſtly compoſed of lava of a pentagonal and hexagonal form, and ſeems evidently made of pieces of ſuch baſaltic columns. Theſe lavas being ready cut by nature, would naturally be carried off firſt, as the cutting of ſolid rocks of lava for ſuch purpoſes is attended with very great expence.

At Unkel, above a league further on towards Coblenz, juſt oppoſite the town on the other ſide of the Rhine, is the great quarry belonging to the Elector Palatine, which affords a moſt pleaſing and uncommon ſight: it is entirely compoſed of the moſt regular detached baſaltic columns, and though millions of theſe columns have been extracted, as the towns of Cologne and Bonn teſtify, yet the quarry is very rich. They lie moſtly in an horizon-

tal direction, but some are perpendicular, and others inclining towards the Rhine, which, being very low, shews many of them in the bed of the river itself; they rise from thence into the mountain (where is the present quarry) above 100 feet. They are, as I mentioned before, chiefly pentagonal; the smallest are in general the most distinct and regular, about six inches diameter; the largest of the columns that I measured in this quarry (or indeed that I had remarked any where) was about three feet long, and about one foot and a half diameter. The other lavas in this neighbourhood are of the same substance, and some incline to the same forms, but none so regular. I have not the least doubt but that all basaltes, wheresoever they exist, have originated from subterraneous fire, and are true lavas.

I hope some one, who has more leisure, will examine this curious country particularly. It is wondeful to me, that such visible tokens of great volcanic productions, in a country so well inhabited, should not have attracted the attention of naturalists more than they seem to have done.

I must not forget to mention another curious circumstance: at Andernach, between Bonn and Coblenz, I saw vast heaps of tuffa ready cut, lying on the banks of the Rhine, and some Dutch vessels loading it; upon

enquiry I found, that a confiderable trade of this material is carried on between this town and Holland, where they grind down this fort of ftone by wind-mills into a powder, which they ufe as a puzzolane for all their buildings under water. This alfo correfponds with an idea mentioned in one of my former letters to the Royal Society, that the tuffas of Naples were compofed of a puzzolane, prepared by volcanic fire deep in the bowels of the earth, and, mixing with water at the time of its explofion, formed a fort of natural mortar or cement. The Dutch reduce it again to its priftine ftate of puzzolane.

I flatter myfelf you will excufe my fending you fuch crude and hafty remarks, as my time will not allow me to examine further: I only mean to point out this curious country for further inveftigation. What I have juft feen confirms me in the opinion, that volcanic operations are much greater agents of nature than is generally imagined.

I am, &c.

II. *Of the Heat, &c. of Animals and Vegetables. By Mr.* John Hunter, *F. R. S.*

Read June 19, and Nov. 13, 1777.

IN the courſe of a variety of experiments on animals and vegetables, I have frequently obſerved that the reſult of experiments in the one has explained the œconomy of the other, and pointed out ſome principle common to both; I have therefore collected ſome experiments which relate to the heat and cold of thoſe ſubſtances. Having found variations in the degree of heat and cold in the ſame experiment, for which I could not account; I ſuſpected that this might ariſe from ſome imperfection in the conſtruction of the thermometer. I mentioned to Mr. RAMSDEN my objection to the common conſtruction of that inſtrument, and my ideas of one more perfect in its nature, and better adapted to the experiments in which I was engaged. He accordingly made me ſome very ſmall thermometers, ſix or ſeven inches long, not above $\frac{2}{12}$ths of an inch thick in the ſtem; having the external diameter of the ball very little larger than that of the

the ſtem, on which was marked the freezing point. The ſtem was embraced by a ſmall ivory ſcale ſo as to ſlide upon it eaſily, and retain any poſition. Upon the hollow ſurface of this ſcale were marked the degrees which were ſeen through the ſtem. By theſe means the ſize of the thermometer was very much reduced, and it could be applied to ſoft bodies with much more eaſe and certainty, and in many caſes in which the former ones could not be conveniently applied: I therefore repeated with it ſuch of my former experiments as were not originally ſatisfactory, and found the degrees of heat very different, not only from what I generally imagined, but alſo from what I had found in my former experiments with the thermometers of the common conſtruction.

I have obſerved in a former paper[(a)], and find it ſupported by every experiment I have made on the heat and cold of animals, that the more perfect have the greateſt power of retaining a certain degree of heat, which may be called their ſtandard heat, and allow of much leſs variation than the more imperfect animals: however, it will appear from the firſt, ſecond, and third experiments, that many, if not all of them, are not capable of keeping conſtantly to one ſtandard; but vary from their ſtandard

(a) Vide Philoſophical Tranſactions for the year 1775, vol. LXV. part II. p. 446.

heat,

heat, either by external applications, or diſeaſe. However, theſe variations are much greater below the ſtandard heat than above it; the perfect animals having a greater power of reſiſting heat than cold, ſo that they are commonly near their ultimate heat. Indeed we do not want any other proof of this variation than our own feelings: we are all ſenſible of heat and of cold, which ſenſations could not be produced without an alteration really taking place in the parts affected; which alteration in the parts could not take place, if they did not become actually warmer or colder. I have often cooled my hands to ſuch a degree, that I have put them into pump-water, immediately pumped, to warm them; therefore, my hands were really colder than the pump-water.

Real increaſe of heat muſt alter the texture or poſition of the parts, ſo as to produce the ſenſation called heat: and as this heat is diminiſhed, the texture or poſition of the parts is altered in a contrary way; which, when carried to a certain degree, becomes the cauſe of the ſenſation of cold. Now theſe effects could not take place in either caſe without a real increaſe or decreaſe of heat in the part; heat, therefore, in its different degrees, muſt be preſent. When heat is applied to the ſkin, it becomes hot, in ſome degree, according to the application; and this may be carried ſo far as actually to burn the living

 parts:

parts: on the contrary, in a cold atmoſphere, a man's hand ſhall become ſo cold as to loſe the ſenſation of cold altogether, and change it for that of pain. Real heat and cold can be carried ſo far, as even to alter the ſtructure of the parts upon which the actions of life depend.

As animals are ſubject to variations in their degrees of heat and cold from external applications, they are of courſe, in this reſpect, affected in ſome meaſure like inanimate matter; and therefore, as parts are elongated or recede from the common maſs, theſe effects more readily take place: for inſtance, all projecting parts and extremities, more eſpecially toes, fingers, noſe, ears, combs of fowls, particularly of the cock, are more readily cooled, and are therefore moſt ſubject to be affected by cold. Animals are not only ſubject to increaſe and decreaſe of heat, ſimilar to inanimate matter; but the tranſition from the one to the other (as far as they allow themſelves to go) is nearly as quick. However, I ſhall not confine myſelf to ſenſation alone, for it is in ſome degree ruled by habit: the habit of uniformity in the degree of the one or the other, will be the cauſe of a conſiderable increaſe of ſenſation from the ſmalleſt variation; while the habit of variation in the degree of heat and cold, will, in a proportional degree, prevent the ſenſation ariſing from either: but we ſhall be guided by actual experiment.

 The

The parts above mentioned (*viz.* projecting parts and extremities) are ſuch as will admit of the greateſt change in their degrees of heat and cold, without materially affecting the animal. I find that they will raiſe or ſink the thermometer, in ſome degree, according to the external heat or cold applied; although not in a proportional degree to this application, as would be the caſe in inanimate matter. Nor are the living parts cooled or heated in the ſame degrees, which appears from the application of the thermometer to the ſkin; for the cuticle may be conſidered as a dead covering, capable of taking greater degrees of heat or cold, than the living parts underneath can do; and it might be ſuſpected, that the whole of the variation was in the covering. To remove this doubt I made the following experiments.

EXP. I. I ſunk the ball of my thermometer under my tongue, which lay perfectly covered by all the ſurrounding parts, kept it there ſome minutes, and found that it roſe to 97°; having continued it ſome time longer there, I found it roſe no higher. I then took ſeveral pieces of ice, about the ſize of walnuts, and put them in the ſame ſituation, allowing them to melt in part, but not wholly, that the application of cold might be better kept up, occaſionally ſpitting out the water ariſing from the ſolution: this I continued for ten minutes, and found, on

introducing my thermometer, that it fell to 77°; ſo that the mouth at this part had loſt 20° of heat. It gradually roſe to 97° again; but the thermometer in this experiment did not ſink ſo low as it would have done in the hand, if a piece of ice had been held in it ſo long. Perhaps one reaſon may be aſſigned: the ſurface under the tongue being ſurrounded with warm parts, renders it next to an impoſſibility to cool it to any greater degree: but I ſuſpect ſtill another reaſon, *viz.* parts which have been in a habit of conſiderably varying in this reſpect, as the hand, will allow of greater latitude, being as it were inſenſibly drawn into cold, nor ſo ſuſceptible of it, as has been already obſerved.

As a further proof, that the more perfect animals are capable of varying their heat, in ſome degree, according to the external heat applied, I ſhall adduce the following experiments made on the human ſubject.

The mouth being a part ſo frequently in contact with the external atmoſphere in the action of breathing, whatever is put into it will be ſuppoſed to be influenced by that atmoſphere; this will always render an experiment made in the mouth, relative to heat and cold, in ſome degree doubtful. I imagined that the urethra would anſwer better, becauſe it is an internal cavity, and can be only influenced by heat and cold applied to the

external

external ſkin of the parts. I imagined alſo, that whatever effects heat or cold might have, when applied, would ſooner take place in the urethra than in any other part of the body, as it is a projecting part; and therefore, if living animal matter was in any degree ſubject to the common laws of matter in this reſpect, the urethra would be readily affected: for this purpoſe I got a perſon, who allowed me to make ſuch experiments as I thought neceſſary.

EXP. II. I introduced the ball of my thermometer into the urethra about an inch; after it had remained there a minute, the quickſilver roſe only to 92°; at two inches, it roſe to 93°; at four inches, the quickſilver roſe to 94°; and when the ball had got as far as the bulb of the urethra, where it is ſurrounded by warm parts, the quickſilver roſe to 97°.

EXP. III. Theſe parts being immerſed in water heated only to 65° for one minute, and the thermometer introduced about an inch and a half into the urethra, the quickſilver roſe to 79°: this was repeated ſeveral times with the ſame ſucceſs. To find if there was any difference in the quickneſs of the tranſition of heat and cold in living and dead parts, and alſo if the latitudes to which each would go were alſo different, I made the following experiments. As this (*viz.* the urethra) ſtill

appeared to me to be the very beſt part of any animal body for experiments of this kind, I had recourſe to it; and as all comparative experiments ſhould be as ſimilar to one another as poſſible, excepting in thoſe points where the difference (if there is any) makes the eſſential part of the experiment, I procured a dead penis.

EXP. IV. The heat of the penis of a living perſon, an inch and a half in the urethra, was 92° exactly. I firſt heated the dead one to the ſame degree, and then had the living one immerſed in water at 50°, at the ſame time immerſing the dead one in the ſame water; when, introducing the thermometer at different times, I obſerved their comparative quickneſs in cooling from 92°. The dead one cooled faſter; but only by two or three degrees. The living came down to 58°, and the dead to 55°. After having continued the thermometer there ſome time longer, it fell no lower. I repeated the ſame experiment ſeveral times, with the ſame ſucceſs; although ſometimes there was a ſmall difference in the degrees of heat from thoſe of others, the heat of the water alſo differing; but the difference in the reſult was nearly in proportion, in all the three different trials, therefore the ſame concluſions are to be drawn from them. In theſe laſt experiments we find very little difference between the cooling of a part of a dead body, and that of the living; but we cannot ſuppoſe that this can take

place

place through the whole body, as in this caſe a living man ſhould always be of the ſame degree of heat with the atmoſphere in which he lives. The man not chooſing to be cooled lower than 53° or 54°, put it out of my power to ſee if the powers of generating heat were exerted in a higher degree, when the heat was brought ſo low as to threaten deſtruction; but from ſome experiments on mice, which will be related hereafter, it will appear, that the animal powers are called upon to exert themſelves in this, when neceſſary.

From the experiments related I found, that parts of an animal were capable of becoming much colder than the common or natural heat: I therefore made farther experiments, with a view to ſee whether the ſame parts were capable of becoming much hotter than the ſtandard heat of animals. The experiments were made in the ſame manner as the former, only the water was now hotter than the natural heat of the animal.

EXP. V. The natural heat of the parts being 92°, they were now immerſed in water heated to 113° for two minutes, and the thermometer being introduced as before, the quickſilver roſe to $100°\frac{1}{2}$. This experiment I alſo repeated ſeveral times, but could not raiſe the heat of the penis beyond $100°\frac{1}{2}$: this was probably owing to the perſon not being able at this time to bear the application of water warmer than 113°. As theſe were only ſingle

expe-

experiments, I chose to make a comparative one with the dead part.

EXP. VI. Both the living and dead part being immersed in water, gradually made warmer and warmer from 100° to 118°, and continued in this heat for some minutes, the dead part raised the thermometer to 114°, while the living could not raise it higher than $102°\frac{1}{4}$. It was observed, by the person on whom the experiment was made, that, after the parts had been in the water about a minute, the water did not feel hot; but, on its being agitated, it felt so hot that he could hardly bear it. Upon applying the thermometer to the sides of the living gland, the quicksilver immediately fell from 118° to about 104°, while it did not fall above a degree when put close to the dead; so that the living gland produced a cold space of water around it[(b)].

EXP. VII. The heat of the rectum in the same man was $98°\frac{1}{2}$ exactly.

In the second, third, fourth, fifth, and sixth experiments, we had an internal cavity, which is both very vascular and sensible, evidently influenced by external heat and cold, though only applied to the skin of the part;

(*b*) This might furnish an useful hint respecting bathing in water, whether colder or warmer than the heat of the body: for if intended to be either colder or hotter, it will soon be of the same temperature with that of the body; therefore in a large bath, the patient should move from place to place: and in a small one, there should be a constant succession of water of the intended heat.

while,

while, in the ſeventh experiment, another part of the ſame body, where external heat and cold can make little or no impreſſion, was of the ſtandard heat. Although we ſhall find hereafter, from experiment, that the rectum is not the warmeſt part of an animal; yet, in order to determine how far the heat could be increaſed by ſtimulating the conſtitution to a degree ſufficient to quicken the pulſe, I repeated the ſeventh experiment after the man had eaten a hearty ſupper, and drank a bottle of wine, which increaſed the pulſe from 73° to 87°, and yet the thermometer only roſe to 98°½.

Having formerly made experiments upon dormice in the ſleeping ſeaſon, with a view to ſee if there was any alteration in the animal œconomy at that time, I find amongſt theſe experiments the following which appear to be to our preſent purpoſe: but, that I might be more certain of the accuracy of my former experiments, I repeated them with my new thermometer.

EXP. VIII. In a room, in which the air was at between 50° and 60° of temperature, a ſmall opening was made in the belly of a dormouſe, of a ſufficient ſize to admit the ball of my thermometer, which, being introduced into the belly at about the middle of that cavity, roſe to 80°, and no higher.

EXP. IX. The moufe was put into a cold atmofphere of 15° above 0, and left there for fifteen minutes; after which, the thermometer being introduced a fecond time, it rofe to 85°.

EXP. X. The moufe was again put into a cold atmofphere for fifteen minutes more; and the thermometer being then introduced, the quickfilver rofe to 72° only, but gradually came up to 83°, 84°, and 85°.

EXP. XI. It was put a third time into the cold atmofphere, and allowed to ftay there for thirty minutes; the lower part of the moufe was at the bottom of the difh, and almoft frozen; the whole of the animal was a little numbed, and a good deal weakened. When the thermometer was introduced, it varied according to the different parts of the belly; in the pelvis, near the parts moft expofed to the cold, it was as low as 62°; in the middle, among the inteftines, about 70°; but near the diaphragm it rofe to 80°, 82°, 84°, and 85°; fo that in the middle of the body the heat had decreafed 10°. Finding a variation in different parts of the fame cavity in the fame animal, I repeated the fame experiments upon another dormoufe.

EXP. XII. I took a healthy dormoufe, which had been afleep in a room in which there was a fire (the atmofphere at 64°); I put the thermometer into its belly, nearly at the middle,

middle, between the thorax and pubis, and the quick-ſilver roſe to 74° or 75°; when I turned the ball towards the diaphragm, it roſe to 80°; and when I applied it to the liver, it roſe to 81°½.

EXP. XIII. The mouſe was put into an atmoſphere at 20°, and left there half an hour; when taken out, it was very lively, much more ſo than when put in. I introduced the thermometer into the lower part of the belly, and it roſe to 91°; and upon turning it up to the liver, to 93°.

EXP. XIV. The animal was put back into the cold atmoſphere at 30° for an hour, when the thermometer was again introduced into the belly; at the liver it roſe to 93°; in the pelvis, to 92°: it was ſtill very lively.

EXP. XV. It was again put back into the cold atmoſphere at 19°, and left there an hour; the thermometer at the diaphragm was 87°; in the pelvis, 83°; but the animal was now leſs lively.

EXP. XVI. It was put into its cage, and two hours after the thermometer, placed at the diaphragm, was at 93°.

From theſe experiments we have actual heat increaſed and decreaſed by the application of external cold; and likewiſe the heat varied according to the powers of life, as well in the ſame parts, as alſo in the different parts, of the ſame animal; for at firſt the natural heat of the

 animal

animal was much below the common ſtandard, and, by the application of cold, and the powers of reſiſtance to the cold being thus increaſed, the heat was conſiderably augmented; but when the animal was weakened by thoſe exertions, it fell off with reſpect to the power of producing heat, and this in proportion to the diſtance from the heart.

Why the heat of this animal ſhould be ſo low as 80° in an atmoſphere of between 50° and 60°, is not eaſily accounted for, except upon the principle of ſleep. But I ſhould very much ſuſpect, that the ſimple principle of ſleep is out of the queſtion, as ſleep is an effect that takes place in all degrees of heat and cold. In thoſe animals where the voluntary actions are ſuſpended, it appears to be an effect ariſing from a certain degree of cold acting as a ſedative, under which the animal faculties are proportionably weakened, but ſtill retain the power of carrying on all the functions of life under ſuch circumſtances; but beyond this degree cold ſeems to act as a ſtimulant, and the animal powers are rouſed to action for ſelf-preſervation. It is more than probable, that moſt animals are under this predicament; and that every order has its degree of cold, in which the voluntary actions can be ſuſpended.

When man is aſleep, he is colder than when awake; and I find, in general, that the difference is about one de-

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gree and a half, ſometimes leſs. But this difference in the degree of cold between ſleeping and waking is not a cauſe of ſleep, but an effect; for many diſeaſes produce a much greater degree of cold in the animal, without giving the leaſt tendency to ſleep; therefore the inactivity of animals from cold is different from ſleep. Beſides, all the operations of perfect life are going on in the time of natural ſleep, at leaſt in the perfect animals, ſuch as digeſtion, ſenſations, &c.; but none of theſe operations are performed in the latter tribe.

To ſee how far the reſult of theſe experiments upon dormice was peculiar to them, I repeated the ſame expements upon common mice. I procured two; one ſtrong and vigorous, the other weakened by faſting.

EXP. XVII. The common atmoſphere being at 60°, I introduced the thermometer into the abdomen of the ſtrong mouſe: the ball being at the diaphragm, the quickſilver was raiſed to 99°, but at the pelvis only to 96°$\frac{3}{4}$.

Here there was a real difference of about 9° in two animals of the ſame ſize, in ſome degree of the ſame genus, and at the ſame ſeaſon of the year, and the atmoſphere of nearly the ſame temperature.

EXP. XVIII. The ſame mouſe was put into a cold atmoſphere of 13°, for an hour, and then the thermometer

was

was introduced as before; the quickſilver at the diaphragm was raiſed to 83°, in the pelvis only to 78°.

Here the real heat of the animal was diminiſhed 16° at the diaphragm, and 18° in the pelvis.

EXP. XIX. In order to determine whether an animal that is weakened, has the ſame powers, with reſpect to preſerving heat and cold, as one that is vigorous and ſtrong, I introduced the ball of the thermometer into the belly of the weak mouſe; the ball being at the diaphragm, the quickſilver roſe to 97°; in the pelvis to 95°: the mouſe being put into the cold atmoſphere as the other, and the thermometer again introduced, the quickſilver ſtood at 79° at the diaphragm, and at 74° in the pelvis.

In this experiment the heat at the diaphragm was diminiſhed 18°, in the pelvis 21°.

Here was a diminution of heat in the ſecond greater than in the firſt, we may ſuppoſe proportional to the decreaſed power of the animal ariſing from want of food.

To determine how far different parts of other animals than thoſe mentioned were of different degrees of heat; I made the following experiments upon a healthy dog.

EXP. XX. The ball of the thermometer was introduced two inches within the rectum, the quickſilver roſe to $100°\frac{1}{2}$ exactly. The cheſt of the dog was opened, and a wound

a wound made into the right ventricle of the heart, and the ball immediately introduced; the quickſilver roſe to 101° exactly. A wound was next made ſome way into the ſubſtance of the liver; and the ball being introduced, the quickſilver roſe to 100°$\frac{3}{4}$. It was next introduced into the cavity of the ſtomach, where it ſtood exactly at 101°. All theſe experiments were made in a few minutes.

EXP. XXI. The ſame experiments were made upon oxen; the quickſilver roſe exactly to 99°$\frac{1}{2}$.

EXP. XXII. The ſame were alſo made upon a rabbit, and the quickſilver roſe to 99°$\frac{1}{2}$.

From the experiments on mice, and thoſe upon the dog, it plainly appears, that every part of an animal is not of the ſame degree of heat; and hence we may reaſonably infer, that the heat of the vital parts of man is greater than what it is found to be either in the mouth, the rectum, or the urethra.

To determine how far my idea, that animals could have their heat varied in proportion to their imperfections, is juſt, I made the following experiments upon fowls, which I conſider to be one remove below what are commonly called quadrupeds.

EXP. XXIII. I introduced the ball of the thermometer ſucceſſively into the *inteſtinum rectum* of ſeveral hens, and

and found that the quickfilver rofe as high as 103°, 103°½, and in one of them to 104°.

EXP. XXIV. I made the fame experiments on feveral cocks, and the refult was the fame.

EXP. XXV. To determine if the heat of the hen was increafed when fhe was prepared for incubation, I repeated the twenty-third experiment upon feveral fitting or clocking hens; in one the quickfilver rofe to 104°; in the others, to 103½, 103°, as in the twenty-third experiment.

EXP. XXVI. Under the hen, who raifed the quickfilver to 104°, I placed the ball of the thermometer, and found the heat there as great as in the rectum.

EXP. XXVII. I took fome of the eggs from under the fame hen, where the chick was about three parts formed, broke a hole in the fhell, &c. and introduced the ball of the thermometer, and found that the quickfilver rofe to 99°½. In fome that were addled, I found their heat not fo high by two degrees; fo that the life in the living egg affifted in fome degree to fupport its own heat.

It may be afked, whether thofe three or four degrees of heat, which are found in the fowl more than in the quadruped, are for the purpofe of incubation? We found that the heat of the eggs, which was caufed and fupported by this heat, was not above the ftandard of the quadrupeds; and

and that it muſt probably have been leſs, if the heat of the hen had not been ſo great.

Finding from the above experiments, that fowls were ſome degrees warmer than that claſs commonly called quadrupeds (although certainly not ſo perfect animals) I choſe to continue the experiments upon the ſame principles, and made the following upon thoſe of a ſtill inferior order. The next remove from the fowl are thoſe commonly called amphibious.

EXP. XXVIII. I took a healthy viper, and introduced the thermometer into its ſtomach and anus; the quickſilver roſe from 58° (the heat of the atmoſphere in which it was) to 68°; ſo that in a common atmoſphere it is 10° warmer.

EXP. XXIX. The viper was put into a pan, and the pan into a cold mixture of about 10°; after being there about ten minutes, its heat was reduced to 37°. It was allowed to ſtay ten minutes longer, the mixture being at 13°, and its heat was reduced to 35°. It was allowed to ſtay ten minutes more, the mixture at 20°, its heat at 31°, and it did not become lower; its tail was beginning to freeze; and it was now very weak. It may be remarked, that it became cold much ſlower than many of the following animals.

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The frog being, in its ſtructure, more ſimilar to the viper than to either fowl or fiſh, I made the following experiments on that animal.

EXP. XXX. I introduced the ball of the thermometer into its ſtomach, and the quickſilver ſtood at 44°. I then put it into a cold mixture, and the quickſilver ſunk to 31°; the animal appeared almoſt dead, but recovered very ſoon: beyond this point it was not poſſible to leſſen the heat, without deſtroying the animal. But its decreaſe of heat was quicker than in the viper, although the mixture was nearly the ſame.

The next order of animals were fiſh.

EXP. XXXI. I aſcertained the heat of water in a pond, where there were carp, and found it 65°½. I then took a carp out of the ſame water, and introduced the thermometer into the ſtomach; the quickſilver roſe to 69°; ſo that the difference between the water and the fiſh was only 3°½.

EXP. XXXII. In an eel, the heat in the ſtomach, which at firſt was at 37°, ſunk, after it had been ſome time in the cold mixture, to 31°. The animal at that time appeared dead, but was alive the next day.

EXP. XXXIII. In a ſnail, whoſe heat was at 44°, it ſunk, after it had been put into the cold mixture, to 31°, and then the animal froze.

EXP. XXXIV. Several leaches having been put into a bottle, and that bottle immerfed in the cold mixture, the ball of the thermometer being placed in the middle of them, the quickfilver funk to 31°; and by continuing the immerfion for a fufficient time to deftroy life, the quickfilver rofe to 32°, and then the leaches froze. In all thefe experiments none of the animals returned to life when they became thawed.

Finding that thefe imperfect claffes of animals are capable of varying their heat to that ftandard which can freeze the folids or fluids when dead, and not much farther before death enfues, I wifhed to determine to what degree of heat the animal could be brought.

EXP. XXXV. A healthy viper was put into an atmofphere of 108°, and allowed to ftay feven minutes, when the heat of the animal in the ftomach and anus was found to be 92°½, beyond which it would not rife in the above heat. The fame experiment was made upon frogs with nearly the fame fuccefs.

EXP. XXXVI. An eel very weak, its heat at 44°, which was nearly that of the atmofphere, was put into water at 65°, for fifteen minutes; and, upon examination, it was of the fame degree of heat with the water.

EXP. XXXVII. A tench, whofe heat was 41°, was put into water at 65°, and left there ten minutes; the

ball of the thermometer being introduced both into the ſtomach and rectum, the quickſilver roſe to 55°. Theſe experiments were repeated with nearly the ſame ſucceſs.

To determine whether life had any power of reſiſting heat and cold in theſe claſſes of animals, I made comparative trials between living and dead ones.

EXP. XXXVIII. I took a living and a dead tench, and a living and a dead eel, and put them into warm water; they all received heat equally faſt; and when they were put into the cold, both the living and the dead received it equally.

I long ſuſpected, that the principle of life was not wholly confined to animals, or animal ſubſtance endowed with viſible organization and ſpontaneous motion; but I conceived, that the ſame principle exiſted in animal ſubſtances, devoid of apparent organization and motion, where the power of preſervation ſimply was required.

I was led to this notion twenty years ago, when I was making drawings of the growth of the chick in the proceſs of incubation. I then obſerved, that whenever an egg was hatched, the yolk (which is not diminiſhed in the time of incubation) was always perfectly ſweet to the very laſt; and that part of the albumen, which is not expended on the growth of the animal, ſome days before hatching,

was alſo perfectly ſweet; although both were kept in a heat of 103°, in the hen's egg for three weeks, and in the duck's for four; but I obſerved, that if an egg was not hatched, that egg became putrid in nearly the ſame time with any other dead animal matter.

To determine how far eggs would ſtand other teſts of a living principle, I made the following experiments.

EXP. XXXIX. I put an egg into cold at about 0, and froze it, then allowed it to thaw; from this proceſs I conceived, that the preſerving powers of the egg muſt be loſt. I then put this egg into the cold mixture, and with it one newly laid; and the difference in freezing was ſeven minutes and a half, the freſh one taking ſo much longer time in freezing.

EXP. XL. A new laid egg was put into a cold atmoſphere, fluctuating between 17° and 15°; it took above half an hour to freeze; but, when thawed and put into an atmoſphere at 25°, it froze in half the time. This experiment was repeated ſeveral times, with nearly the ſame ſucceſs.

To determine the comparative heat between a living and a dead egg, and alſo to determine whether a living egg be ſubject to the ſame laws with the more imperfect animals, I made the following experiments.

EXP. XLI. A fresh egg, and one which had been frozen and thawed, were put into the cold mixture at 15°; the thawed one soon came to 32°, and began to swell and congeal; the fresh one sunk to 29°½, and in twenty-five minutes after the dead one, it rose to 32°, and began to swell and freeze.

The result of this experiment upon the fresh egg was similar to the above experiments upon the frog, eel, snail, &c. where life allowed the heat to be diminished 2° or 3° below the freezing point, and then resisted all further decrease; but the powers of life were expended by this exertion, and then the parts froze like any other dead animal matter.

From these experiments in general it must appear, that a fresh egg has the power of resisting heat, cold, and putrefaction, equal to many of the more imperfect animals; and it is more than possible, that this power arises from the same principle in both.

From some of these experiments it appears, that the more imperfect animals are capable of having their heat and cold varied very considerably, not according to the extent of the heat or cold of the surrounding medium in which they can live, but according to the degree of cold which is capable of altering the parts in a dead state, below which the living power will not go far;

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for whenever the ſurrounding cold brings them to that degree, the power of generating heat takes place till life is gone, then the animal freezes, and is immediately capable of admitting any degree of cold.

From theſe circumſtances of thoſe imperfect animals (upon which I made my experiments) varying their heat ſo readily, we may conclude, that heat is not ſo very eſſential to life in them as in the more perfect; although it be eſſential to many of the operations, or what may be called the ſecondary actions of life, ſuch as digeſting food[(b)], and the propagation of their ſpecies, which requires the greateſt power an animal can exert, more eſpecially the laſt; and, as moſt of the more perfect of theſe imperfect animals are commonly employed in the firſt, we may ſuppoſe their heat to be ſuch as this action of life requires, although in them it be never eſſentially neceſſary to be ſo high as to produce propagation[(c)].

Therefore

(*b*) How far this idea holds good with fiſh I am not certain.

(*c*) How far the animal heat is lowered in the more perfect animals, when theſe ſecondary actions are not neceſſary, as in the bat, hedge-hog, bear, &c. I have not been able to determine, not having opportunities of examining theſe animals in their involuntary ſtate. Dormice are in a mixed ſtate between the voluntary and involuntary, and we find the heat diminiſhed when the actions are not vigorous; and from a general review of this whole ſubject it would appear, that a certain degree of heat in the animal is neceſſary for digeſtion, and that neceſſary heat will be according to the nature of the animal A frog will digeſt food when its heat is at 60°, but not when at 35° or 40°; and it is

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Therefore, whenever theſe imperfect animals are in a cold ſo low as to weaken their powers, and diſable them from performing the firſt of theſe ſecondary actions, they become in ſome meaſure involuntary, and remain in a torpid ſtate during the degree of cold which will always occur in ſome part of the winter in ſuch countries as they inhabit; and the food of ſuch animals in general not being produced in the cold ſeaſon, affords another reaſon for their torpidity.

From the circumſtances of their heat being allowed to ſink to the freezing point, or ſomewhat lower, and then becoming ſtationary; and of the animal not being able to ſupport life in a much greater degree of cold for a conſiderable time, we ſee a reaſon why thoſe animals always endeavour to procure ſuch places of abode in the winter as ſeldom arrive at that point. Thus we have toads burrowing, frogs living under large ſtones, ſnails protected under the ſhelter of ſtones and in holes, fiſh hav-

very probable that, when the heat of the bear, hedge-hog, dormouſe, bat, &c. is reduced to 70°, 75°, or 80°, they loſe their power of digeſtion; or rather that the body, in ſuch a degree of cold, has no call upon the ſtomach. That animals, in a certain degree of heat, muſt always have food, is further illuſtrated by the inſtance of bees. The conſtruction of a bee is very ſimilar to a fly, a waſp, &c. A fly and a waſp can allow their heat to diminiſh as in the fiſh, ſnake, &c. without loſing life, but a bee cannot; therefore a bee is obliged to keep up its heat as high as what we may call its digeſtive heat, but not its propagating; for which purpoſe they provide againſt ſuch cold as would deprive them even of their digeſtive heat, if they had not food to preſerve it.

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ing recourſe to deep water, all which places are generally above the freezing point in our hardeſt froſts: however, our froſts are ſometimes ſo ſevere as to kill many whoſe habitations are not very ſecure.

When the froſt is more intenſe and of longer ſtanding than common, or in countries where the winters are always ſevere, there is generally ſnow, and the water freezes: the advantage ariſing from theſe two circumſtances are great; the ſnow ſerving as a blanket to the earth, and the ice to the water(e).

(e) Snow and ice are perhaps the worſt conductors of heat of any ſubſtance yet known. In the firſt place, they never allow their own heat to riſe above the freezing point, ſo that no heat can paſs through ice or ſnow when at 32°, by which means they become an abſolute barrier to all heat that is at or above that degree; ſo that the heat of the earth, or whatever ſubſtance they cover, is retained: but they are conductors of heat below 32°. Perhaps that power decreaſes in proportion as the heat decreaſes under that part.

In the winter 1776 a froſt came on, the ſurface of the ground was frozen; but a conſiderable fall of ſnow alſo came on, and continued ſeveral weeks; the atmoſphere at this time was often at 15°, but it was not allowed to affect the ſurface of the earth conſiderably, ſo that the ſurface of the ground thawed, and the earth retained the heat of 34°, in which beans and peas grow.

The ſame thing took place in water, in a pond where the water was frozen on the ſurface to a conſiderable thickneſs; a large quantity of ſnow fell and covered the ice; the heat of the water was preſerved and thawed the ice, and the ſnow at its under ſurface was found mixed with the water.

The heat of the water under the ſnow was at 35°, in which the fiſh lived very well.

It would be worthy of the attention of the philoſopher, to inveſtigate the cauſe of the heat of the earth, upon what principle it is preſerved, &c.

As all the experiments I ever made upon the freezing of animals, with a view to ſee if it were poſſible to reſtore the actions of life when thawed, were made upon whole ones, and as I never ſaw life return by thawing[(f)]; I wiſhed to ſee how far parts were ſimilar to the whole in this reſpect; eſpecially as we have it aſſerted, and with ſome authority, that parts of a man may be frozen, and afterwards recover: for this purpoſe I made the following experiments upon an animal of the ſame order as ourſelves.

In January 1777, I mixed ſalt and ice till the cold was about 0; on the ſide of the veſſel was a hole, through which I introduced the ear of a rabbit. To carry off the heat as faſt as poſſible, it was held between two flat pieces of iron that went farther into the mixture. That part of the ear projecting into the veſſel became ſtiff, and when cut did not bleed; and the part cut off by a pair of ſciſſars, flew from between the blades like a hard chip.

The ear remained in the mixture nearly an hour: when taken out it ſoon thawed, and began to bleed; it became very flaccid, ſo as to double upon itſelf, having loſt its natural elaſticity. When out of the mixture nearly an hour, it became warm, and this warmth in-

(f) Vide Phil. Tranſ. for the year 1775, vol. LXV. part. II. p. 446.

creaſed

creafed to a confiderable degree; while the other ear continued in its ufual cold, and alfo began to thicken. The day following the frozen ear was ftill warm; and two days after it ftill retained its heat and thicknefs, which continued for many days after.

About a week after this, the mixture being the fame as the former, I introduced both ears of the fame rabbit through the hole, and froze them both: the found one, however, froze firft, probably from its being confiderably colder at the beginning. When withdrawn, they foon thawed, and foon both became warm, and the frefh ear thickened as the other had done before.

Feb. 23, 1777, I repeated thefe experiments. I froze the ear of a white rabbit till it became as hard as a board. It was longer in thawing than in the former experiment, and much longer before it became warm; however, in about two hours it became a little warm, and the day following it was very warm and thickened.

In the fpring 1776, I obferved that the cocks I had in the country had their combs fmooth with an even edge, and not fo broad as formerly, appearing as if near one half of them had been cut off. Having inquired into the caufe of this, my fervant told me, that it had been common in that winter during the hard froft. He obferved, that they had become in part dead, and at laft dropped off:

alſo, that the comb of another cock had dropped intirely off, which I did not ſee, as by accident he burnt himſelf to death. I naturally imputed this effect to thoſe combs having been frozen in the time of the ſevere froſt; and having, conſequently, loſt the life of that part by this operation. I endeavoured to try the ſolidity of this reaſoning by experiment.

I attempted to freeze the comb of a very large young cock (which was of a conſiderable breadth) but could only freeze the ſerrated edges (which proceſſes were full half an inch long); the comb itſelf being very thick and warm reſiſted the cold. The frozen parts became white and hard; and, when I cut off a little bit, it did not bleed, nor did the animal ſhew any ſigns of pain. I next introduced into the cold mixture one of his wattles, which was very broad and thin; it froze very readily: upon thawing both the comb and wattle, they became warm, but were of a purple colour, having loſt that tranſparency which the other parts of the comb and the other wattle had. The wound in the comb now bled freely.

Both comb and wattle recovered perfectly in about a month. The natural colour returned firſt neareſt to the ſound parts, increaſing gradually till the whole was become perfectly ſound.

There was a very material difference in the effect between thofe fowls, the ferrated edges of whofe combs I fufpected to have been frozen in the winter of $176\frac{5}{6}$, for they muft have dropped off. The only way in which I can account for this difference is, that in thofe fowls the parts were kept fo long frozen, that the unfrozen or active parts had time to inflame, and had brought about a feparation of the frozen parts, treating them exactly as dead, fimilar to a mortified part; and that before they thawed, the feparation was fo far compleated as to deprive them of farther fupport.

As it is confidently afferted, that fifh are often frozen and come to life again, and as I had never fucceeded in any of my experiments of this kind upon whole fifh; I made fome partial experiments upon this clafs of animals, being led to it by having found a material difference in my experiments upon whole individuals and only parts of the more perfect order of animals.

I froze the tail of a tench (as high as the anus) which became as hard as a board; when it thawed, that part was whiter than common; and when it moved, the whole tail moved as one piece, and the termination of the frozen part appeared like the joint on which it moved.

On the fame day I froze the tails of two gold fifh till they became as folid as a piece of wood. They were put

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into cold water to thaw: they appeared at firft, for fome days, to be very well; but that part of the tail which had been frozen had not the natural colour, and the fin of the tail became ragged. About three weeks after a furr came all over the frozen part; the tail became lighter, fo that the fifh was fufpended in the water perpendicularly, and they had almoft loft the power of motion; at laft they died. The water in which they were kept was New River water, fhifted every day, and about ten gallons in quantity.

I made fimilar experiments upon an order of animals ftill inferior, *viz.* common earth worms.

I firft froze the whole of an earth worm as a ftandard; when thawed it was perfectly dead.

I then froze the anterior half of another earth worm; but the whole died.

I next froze the pofterior half of an earth worm; the anterior half lived, and feparated itfelf from the dead part.

As I had formerly in making my experiments upon animals, relative to heat and cold, made fimilar ones on vegetables, and had generally found a great fimilarity between them in thefe refpects, I was led to purfue the fubject upon the fame plan; but I was ftill farther induced to continue my experiments upon vegetables, as

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I imagined I ſaw a material difference between them in their power of ſupporting cold.

From obſervations and the foregoing experiments it plainly appears, that the living principle will not allow the heat of ſuch animals to ſink much lower than the freezing point, although the ſurrounding atmoſphere be much colder, and that in ſuch a ſtate they cannot ſupport life long; but it may be obſerved, that moſt vegetables of every country can ſuſtain the cold of their climate. In very cold regions, as in the more Northern parts of America, where the thermometer is often 50° below 0, where peoples feet are known to freeze and their noſes to drop off if great care be not taken, yet the ſpruce-fir, birch, juniper, &c. are not affected.

Yet that vegetables can be affected by cold, daily experience evinces; for the vegetables of every country are affected if the ſeaſon be more than ordinarily cold for that country, and ſome more than others; for in the cold climates abovementioned, the life of the vegetable is often obliged to give way to the cold of the country: a tree ſhall die by the cold, then freeze and ſplit into a great number of pieces, and in ſo doing produce conſiderable noiſe, giving loud cracks which are often heard at a great diſtance.

In this country the ſame thing ſometimes happens to exotics from warmer climates: a remarkable inſtance of this

this kind happened this winter in his Majesty's garden at Kew. The *Erica arborea* or Tree-heath, a native of Spain and Portugal, which had kept its health extremely well against a garden-wall for four or five years, though covered with a mat, was killed by the cold, and then being frozen split into innumerable pieces (g). But the question is, is every tree dead that is frozen? I can only say, that in all the experiments I ever made upon trees and shrubs, whether in the growing or active state, or in the passive, that whole or part which was frozen, was dead when thawed.

The winter $177\frac{5}{6}$ afforded a very favourable opportunity for making experiments relative to cold, which I carefully availed myself of. However, previous to that winter, I had made many experiments upon vegetables respecting their temperature comparatively with that of the atmosphere, and when they were in their different states of activity: I therefore examined them in different seasons, with a

(g) This must be owing to the sap in the tree freezing, and occupying a larger space when frozen than in a fluid state, similar to water; and that there is a sufficient quantity of sap in a tree newly killed is proved by the vast quantity which flows out upon wounding a tree. But what appeared most remarkable to me was, that in a walnut-tree, on which I made many of my experiments, I observed that more sap issued out in the winter than in the summer. In the summer, a hole being bored, scarcely any came out; but in the winter it flowed out abundantly.

view

view to ſee what power vegetables have. I ſhall relate theſe experiments in the order in which they were made.

They were begun in the ſpring, the actions of life upon which growth depends being then upon the increaſe; and they were continued till thoſe actions were upon the decline, and alſo when all actions were at an end, but whilſt the paſſive powers of life were ſtill retained.

The firſt were made on a walnut tree, nine feet high in the ſtem, and ſeven feet in circumference in the middle.

A hole was bored into it on the North ſide, five feet above the ſurface of the ground, eleven inches deep towards the centre of the tree, but obliquely upwards, to allow any ſap, which might ooze through the wounded ſurface, to run out.

I then fitted to this part a box about eight inches wide and five deep, and faſtened it to the tree: the bottom of the box opened like a door with a hinge. I ſtuffed the box with wool, excepting the middle, oppoſite to the hole in the tree: for this part I had a plug of wool to ſtuff in, which, when the door was ſhut, incloſed the whole. The intention of this was to keep off as much as poſſible all immediate external influence either of heat or cold.

 The

The ſame thermometer with which I made my former experiments, ſeven inches and a half long, was ſunk into a long feather of a peacock's tail, with a ſlit upon one ſide to ſhow the degrees; by this means the ball of the thermometer could be introduced into the bottom of the hole.

EXP. I. March 29th, I began my experiments at ſix in the morning, the atmoſphere at 57°½, the thermometer in the tree at 55°; when it was withdrawn the quickſilver ſunk to 53°, but ſoon roſe to 57°½[(b)].

This experiment was repeated three times with the ſame ſucceſs. Here the tree was cooler than the atmoſphere; when one ſhould rather have expected to have found it warmer, ſince it could not be ſuppoſed to have as yet loſt its former day's heat.

EXP. II. April 4th, half paſt five in the evening, the tree at 56°, the atmoſphere at 62°; the tree therefore ſtill cooler than the atmoſphere.

EXP. III. April 5th, wind in the North, a coldiſh day, ſix o'clock in the evening, the thermometer in the tree was at 55°, the atmoſphere at 47°; the tree warmer than the atmoſphere.

(*b*) The ſinking of the quickſilver upon being withdrawn I imputed to the evaporating of the moiſture of the fluid upon the ball.

EXP.

EXP. IV. April 7th, a cold day, wind in the North, cloudy, at three o'clock in the afternoon, the thermometer in the tree was at 42°, the atmoſphere at 42° alſo.

EXP. V. April 9th, a cold day, with ſnow, hail, and wind, in the North-eaſt; at ſix in the evening the thermometer in the tree at 45°, the atmoſphere at 39°.

Here the tree was warmer than the atmoſphere, juſt as might have been expected. If theſe experiments prove any thing, it is that there is no ſtandard; and probably theſe variations aroſe from ſome circumſtance which had no immediate connection with the internal powers of the tree; but it may alſo be ſuppoſed to have ariſen from a power in the tree to produce or diminiſh heat, as ſome of them were in oppoſition to the atmoſphere.

After having endeavoured to find out the comparative heat between vegetables and the atmoſphere, when the vegetables were in action; I next made my experiments upon them when they were in the paſſive life.

As the difference was very little when in their moſt active ſtate, I could expect but very little when the powers of the plant were at reſt.

From experiment upon the more imperfect claſſes of animals it plainly appears, that although they do not reſiſt the effects of extreme cold till they are brought to the freezing point, they then appear to have the

power of refifting it, and of not allowing their cold to be brought much lower.

To fee how far vegetables are fimilar to thofe animals in this refpect, I made feveral experiments: I however fufpected them not to be fimilar, becaufe fuch animals will die in a cold in which vegetables do live; I therefore fuppofed that there is fome other principle.

I did not confine thefe experiments to the walnut tree, but made fimilar ones on feveral trees of different kinds, as pines, yews, poplars, &c. to fee what was the difference in different kinds of trees. The difference proved not to be great, not above a degree or two: however, this difference, although fmall, fhews a principle in life, all other things being equal; for as the fame experiments were made on a dead tree, which ftood with its roots in the ground, fimilar to the living ones, they became more conclufive.

In October I began the experiments upon the walnut tree, when its powers of action were upon the decline, and when it was going into its paffive life.

EXP. VI. October 18th, at half paft fix in the morning, the atmofphere at $51°\frac{1}{2}$, the thermometer in the tree was at $55°\frac{1}{2}$; but, on withdrawing and expofing i for a few minutes in the common atmofphere, it fell to $50°\frac{1}{2}$.

EXP.

EXP. VII. October 21ſt, ſeven o'clock in the morning, the atmoſphere at 41°, the tree at 47°.

EXP. VIII. October 21ſt, in the evening at five o'clock, the atmoſphere at 51°$\frac{1}{2}$, the tree at 57°.

EXP. IX. October 22d, at ſeven in the morning, the atmoſphere at 42°, the tree at 48°.

EXP. X. October 22d, one o'clock after noon, the atmoſphere at 51°, the tree at 53°.

EXP. XI. October 23d, in the evening of a wet day, the atmoſphere at 46°, the tree at 48°.

EXP. XII. October 28, a dry day, the atmoſphere at 45°, the tree at 46°.

EXP. XIII. October 29th, a fine day, the atmoſphere at 45°, the tree at 49°.

EXP. XIV. November 2d, wind Eaſt, the atmoſphere at 43°, the tree at 43°.

EXP. XV. November 5th, wet day, the atmoſphere at 43°, the tree at 45°.

EXP. XVI. Nov. 10th, atmoſphere at 49°, the tree at 55°.

EXP. XVII. November 18th, atmoſphere at 42°, the tree at 44°.

EXP. XVIII. November 20th, fine day, the atmoſphere at 40°, the tree at 42°.

EXP. XIX. December 2d, the atmofphere at 54°, the tree at 54°.

In all thefe experiments, which were made at very different times in the day, *viz.* in the morning, at noon, and in the evening, the tree was in fome degree warmer than the atmofphere, excepting in one, when their temperatures were equal. For the fake of brevity I have drawn up my other experiments (which were made on different trees) into four tables, as they were made at four different degrees of heat of the atmofphere, including thofe made in the time of the very hard froft in the winter of $177\frac{5}{6}$. They were as follows.

1ft.

Atmofphere.	Names.	Height. Ft. In.	Diameter. Ft. In.	Heat. °
29 deg.	Carol. poplar,	2	2	$29\frac{1}{2}$
	Engl. poplar,	4	$2\frac{1}{4}$	$29\frac{1}{2}$
	Orien. plane,	3	$1\frac{1}{4}$	30
	Occid. plane,	3.6	2	30
	Carol. plane,	1	$1\frac{3}{4}$	30
	Birch,	3.6	$2\frac{1}{2}$	$29\frac{1}{2}$
	Scotch fir,	3.6	4	$28\frac{1}{2}$
	Cedar libanon,	2.2	$4\frac{1}{2}$	$28\frac{1}{2}$
	Arbutus,	2.6	$3\frac{1}{2}$	30
	Arbor vitæ,	2.8	$3\frac{1}{2}$	29
	Diffid. cyprus,	3	$2\frac{1}{2}$	30
	Lacker varnifh,	3.6	2	30
	Walnut tree,	5	2.4	31

The

The old hole in the walnut tree being full of ſap was frozen up, but a freſh one was made.

2d.

Atmoſphere.	Names.	Height. Ft. In.	Diameter. In.	Heat. °
27 deg.	Spruce fir,	4	$2\frac{1}{2}$	32
	Scotch fir,	$1.5\frac{1}{2}$	$1\frac{1}{2}$	28
	Silver fir,	3.11	$2\frac{1}{2}$	30
	Weymouth fir,	4.6	$2\frac{1}{2}$	30
	Yew,	3.7	3	30
	Holly,	2.6	2	30
	Plumb tree,	4.6	3	$31\frac{1}{2}$
	Dead cedar,	3.11	3	29
	Ground under ſnow,	3 deep	—	34

3d.

Atmoſphere.	Names.	Heat.
24 deg.	Spruce fir,	23°
	Scotch fir,	23
	Silver fir,	23
	Weymouth fir,	23
	Yew,	22
	Holly,	23
	Dead cedar,	24

The ſame trees we mentioned when the thermometer was at 29°, in new holes made at the ſame height, and left ſome time pegged up till the heat produced by the gimlet was gone off; but in which, as they were moiſt

from the ſap, the heat could be very little, eſpecially as the gimlet was not in the leaſt heated by the operation.

4th.

16 deg.	Car. poplar,	17°
	Eng. poplar,	17
	Ori. plane,	17
	Occ. plane,	17
	Carol. plane,	17
	Birch,	17
	Scotch fir,	16½

It will be neceſſary to obſerve, that the ſap of the walnut tree, which flowed out in great quantity, froze at 32°. I did not try to freeze the ſap of the others.

Now, ſince the ſap of a tree, when taken out, freezes at 32°; alſo, ſince the ſap of the tree, when taken out of its proper canals, freezes when the heat of the tree is at 31°; and ſince the heat of the tree can be ſo low as 17° without freezing; by what power are the juices of the tree, when in their proper canals, kept fluid in ſuch a cold? Is it the principle of vegetation? Or is the ſap incloſed in ſuch a way as that the proceſs of freezing cannot take place, which we find to be the caſe when water is confined in globular veſſels? If ſo, its confinement muſt be very different from the confinement of the moiſture in dead vegetables; but the circumſtance of

vege-

vegetables dying with the cold, and then freezing, appears to anſwer the laſt queſtion. Theſe, however, are queſtions which at preſent I ſhall not endeavour to ſolve.

I have made ſeveral experiments upon the ſeeds of vegetables ſimilar to thoſe on the eggs of animals; but, as inſerting them would draw out this paper to too great a length, I will reſerve them for another.

III. *The Force of fired Gun-powder, and the initial Velocities of Cannon Balls, determined by Experiments; from which is also deduced the Relation of the initial Velocity to the Weight of the Shot and the Quantity of Powder.* By Mr. Charles Hutton, *of the Military Academy at* Woolwich. *Communicated by* Samuel Horsley, *LL.D. Sec. R. S.*

Read Jan. 8, 1778.

THESE experiments I made at Woolwich in the summer of the year 1775, assisted by several able officers of the royal artillery at that place, and other ingenious gentlemen. The object of them was the determination of the actual velocities with which balls are impelled from given pieces of cannon, when fired with given charges of powder. These experiments were made according to the method invented by Mr. ROBINS, and described in his treatise, intitled, New Principles of Gunnery, of which an account was printed in the Philosophical Transactions for the year 1743. Before the discoveries of that ingenious gentleman very little progress had been made in the true theory of military projectiles. His book, however, contained such important discoveries, that it was soon

ſoon tranſlated into ſeveral of the languages on the continent, and the famous Mr. L. EULER honoured it with a very extenſive commentary in his tranſlation of it into the German language. That part of it hath always been particularly admired which relates to the experimental method of aſcertaining the actual velocities of ſhot, and in imitation of which were made the experiments related in this paper. Experiments in the manner of Mr. ROBINS were generally repeated by his commentators and others, with univerſal ſatisfaction, the method being ſo juſt in theory, ſo ſimple in practice, and altogether ſo ingenious, that it immediately gave the fulleſt conviction of its excellence, and of the abilities of its author. The uſe which that gentleman made of this invention was, to obtain the actual velocities of bullets experimentally, in order to compare them with thoſe which he computed *a priori* from his new theory, and thereby to verify the principles on which it is founded. The ſucceſs was fully anſwerable to his expectations, and left no doubt of the truth of his theory, when applied to ſuch pieces and bullets as he had uſed: but theſe were very ſmall, being only muſket balls of about one ounce weight; for, on account of the great ſize of the machinery neceſſary for ſuch experiments, Mr. ROBINS and other ingenious gentlemen had not ventured to extend

their practice beyond bullets of that kind, and satisfied themselves with earnestly wishing for experiments to be made in a similar manner with balls of a larger sort. By the experiments in this paper I have endeavoured, in some degree, to supply this defect, having made them with small cannon balls of above twenty times the size, or from one pound to near three pounds weight. These are the only experiments that I know of which have been made with cannon balls for this purpose, although the conclusions to be deduced from such are of the greatest importance to those parts of natural philosophy which are dependent on the effects of fired gunpowder; nor do I know of any other practical method of ascertaining the initial velocities of military projectiles within any tolerable degree of the truth. The knowledge of this velocity is of the utmost consequence in gunnery: by means of it, together with the law of the resistance of the medium, every thing is determinable relative to that business; for, besides its being an excellent method of trying the strength of different sorts of powder, it gives us the law relative to the different quantities of powder, to the different weights of shot, and to the different lengths and sizes of guns. Besides these, there does not seem to be any thing wanting to determine any inquiry that can be made concerning the flight and ranges of shot, except the effects arising from the resistance of the medium.

Of

Of the nature of the experiment, and of the machinery used in it.

The intention of the experiment is to diſcover the actual velocity with which a ball iſſues from a piece, in the uſual practice of artillery. This velocity is very great; from one thouſand to two thouſand feet in a ſecond of time. For conveniently eſtimating ſo great a velocity, the firſt thing neceſſary is to reduce it, in ſome known proportion, to a ſmall one. This we may conceive to be effected thus: ſuppoſe the ball, with a great velocity, to ſtrike ſome very heavy body, as a large block of wood, from which it will not rebound, ſo that they may proceed forward together after the ſtroke. By this means it is obvious, that the original velocity of the ball may be reduced in any proportion, or to any ſlow velocity which may conveniently be meaſured, by making the body ſtruck to be ſufficiently large; for it is well known, that the common velocity, with which the ball and block of wood would move forward after the ſtroke, bears to the original velocity of the ball only, the ſame ratio which the weight of the ball hath to that of the ball and block together. Thus then velocities of one thouſand feet in a ſecond are eaſily reduced to thoſe

thoſe of two or three feet only; which ſmall velocity being meaſured by any convenient means, let the number denoting it be increaſed in the proportion of the weight of the ball to the weight of the ball and block together, and the original velocity of the ball itſelf will thereby be obtained. In theſe experiments, this reduced velocity is rendered very eaſy to be meaſured by a very ſimple and curious contrivance, which is this: the block of wood, which is ſtruck by the ball, is not left at liberty to move ſtraight forward in the direction of the motion of the ball, but it is ſuſpended, as the weight or bob of a pendulum, by a ſtrong iron ſtem, having a horizontal axis at top, on the ends of which it vibrates freely when ſtruck by the ball. The conſequence of this ſimple contrivance is evident: This large balliſtic pendulum, after being ſtruck by the ball, will be penetrated by it to a ſmall depth, and it will then ſwing round its axis and deſcribe an arch, which will be greater or leſs according to the force of the blow ſtruck; and from the ſize of the arch deſcribed by the vibrating pendulum, the velocity of any point of the pendulum itſelf can be eaſily computed; for a body acquires the ſame velocity by falling from the ſame height, whether it deſcend perpendicularly down, or otherwiſe; therefore, the length of the arch deſcribed, and of its radius, being

given, its verfed fine becomes known, which is the height perpendicularly defcended by the correfponding point of the pendulum. The height defcended being thus known, the velocity acquired in falling through that height becomes known from the common rules for the defcent of bodies by the force of gravity; and this is the velocity of that point of the pendulum: this velocity of any known point whatever is then to be reduced to the velocity at the center of ofcillation, by the proportion of their radii or diftances from the axis of motion; and the velocity of this center, thus obtained, is to be efteemed the velocity of the whole pendulum itfelf; which being now given, that of the ball before the ftroke becomes known from the given weights of the ball and pendulum. Thus then the menfuration of the very great velocity of the ball is reduced to the obfervation of the magnitude of the arch defcribed by the pendulum, in confequence of the blow ftruck. This arch may be meafured after various ways: in the following experiments it was afcertained by meafuring the length of its chord by means of a piece of tape or fmall ribband, the one end of which was faftened to the bottom of the pendulum, and the reft of it made to flide through a fmall machine contrived for the purpofe, which will be hereafter defcribed; for thus the length of the tape drawn out,

out, was equal to the length of the chord of the arch defcribed by the bottom of the pendulum.

This defcription may convey a general idea of the nature and principle of the experiment; but befides the center of ofcillation and the weights of the ball and pendulum, the effect of the blow depends alfo on the place of the center of gravity and the point of impact: it will, therefore, be now neceffary to give a more particular defcription of the machine, and of the methods of finding the abovementioned requifites, and then inveftigate our general rule for determining the velocity of the ball, in all cafes, from them and the chord of the arch of vibration.

Of the particular defcription of the machine, and of the determination of the centers of gravity and ofcillation.

Tab. I. Fig. I. is a reprefentation of the machine ufed in the firft three courfes of experiments; and fig. 2. of that which was ufed in the other two. I fhall here defcribe the former of thefe, and afterwards take notice of the few particulars in which the other differs from it when I come to treat of the ufe of the latter.

The firft pendulum confifted of a block of found and dry elm, being nearly a cube of twenty inches long, which

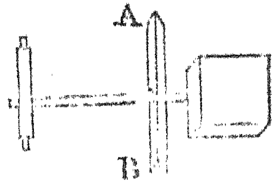
which was faſtened to a ſtrong iron ſtem on the back part of it by ſcrew-bolts, having a thick iron axis at the top, whoſe ends were turned truly cylindrical, to roll pretty freely in ſockets made to receive them; the whole being ſupported by a four-legged ſtand of very ſtrong timber, which was firmly fixed in the ground. A is the face of the cube into which the balls were fired; by means of the blow it is made to ſwing round the axis BC, and the chord of the arch thereby deſcribed is meaſured by the tape DEF faſtened to the bottom of the wood at D, and ſliding with ſome ſlight friction through a little machine of braſs, fixed at E for that purpoſe, the tape being marked with inches and tenths, for the more eaſily meaſuring of the chord or part of it drawn through by the pendulum. The whole length of this pendulum, from the middle of the axis to the ribband at D, was $102\frac{1}{2}$ inches. The weight and the other dimenſions were taken each day when the experiments were made, and then regiſtered; and the manner of diſcovering the places of the centers of gravity and oſcillation was as follows:

To find the center of oſcillation, the pendulum was hung up, and made to vibrate in ſmall arcs, and the time of making two or three hundred vibrations was obſerved by a half-ſecond pendulum. Having thus obtained the

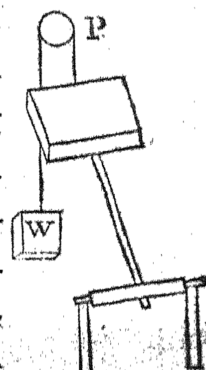
 time

time anfwering to a certain number of vibrations, the finding of the center of ofcillation is eafy: for if v denote the number of vibrations made in s feconds, then it is well known, that as $vv : ss :: 39.2 : \frac{39.2\,ss}{vv}$ = the diftance in inches from the axis of motion to the center of ofcillation; and by this rule the place of that center was found for each day.

The center of gravity was afcertained by one or both of the two following methods. Firft, a triangular prifm of iron AB, being placed upon the ground with an edge upwards, the pendulum was laid



acrofs it, and moved forward or backward on the ftem or block as the cafe required, till the two ends exactly balanced each other; then, as it lay, the diftance was meafured from the middle of the axis to the part which refted on the edge of the prifm, or the center of gravity of the pendulum. The other method was as in the latter of the two annexed figures, where the ends of the axis being fupported on fixed uprights, and a chord faftened to the lower end of the pendulum, was paffed over a pulley at P, different weights W were faftened to the other end of it, till the pendulum was



brought

brought to a horizontal poſition. Then, taking alſo the whole weight of the pendulum, and its length from the axis to the bottom where the chord was fixed, the place of the center of gravity is found by this proportion, as p the weight of the pendulum : w the appended weight :: d the whole length from the axis to the bottom : $\frac{dw}{p}$ = the diſtance from the axis to the center of gravity. Either of theſe two methods gave the place of the center of gravity ſufficiently exact; but the coincidence of the reſults of both of them was ſtill more ſatisfactory.

Of the rule for computing the Velocity of the ball.

Having deſcribed the methods of obtaining the neceſſary dimenſions, I proceed now to the inveſtigation of the theorem by which the velocity of the ball is to be computed. The ſeveral weights and meaſures being found let then

b denote the weight of the ball,
p the whole weight of the pendulum,
g the diſtance of the center of gravity below the axis
h the diſtance to the center of oſcillation,
k the diſtance to the point ſtruck by the ball,
z the velocity of this point ſtruck after the blow,

v the original velocity of the ball,

c the chord of the arch meaſured by the tape, and

r its radius, or the diſtance from the axis to the bottom of the pendulum.

Then the effect of the blow ſtruck by the ball is as $\frac{gb}{kk}$; or, as $kk : gb :: p : \frac{gbp}{kk}$ = the weight of a body, which, being placed at the point ſtruck, would acquire the ſame velocity from the blow as the pendulum does at the ſame point. Here then are two bodies, b and $\frac{gbp}{kk}$, the former of which, with the velocity v, ſtrikes the latter at reſt, ſo that after the blow they both proceed uniformly forward together with the velocity z: in which caſe it is well known, that $b : b + \frac{gbp}{kk} :: z : v$; and therefore the velocity z is $= \frac{bkkv}{bkk+gbp}$. But becauſe of the acceſſion of the ball to the pendulum, the place of the center of oſcillation will be changed; and from the known property of that point we find $\frac{bkk+ghp}{bk+gp}$ = to its diſtance from the axis. Call this diſtance of the center of oſcillation, of the maſs compounded of the ball and pendulum, H. Then, ſince z is the velocity of the point whoſe diſtance is k, we have this proportion, as $k : \text{H} :: z : \frac{z\text{H}}{k} = \frac{bkv}{bk+gp}$ = the velocity of this compound center of oſcillation.

Again,

Again, ſince $\frac{cc}{2r}$ is the verſed ſine of the deſcribed arc c, its radius being r; therefore as $r : \text{II} :: \frac{cc}{2r} : \frac{cc}{2rr} \times \frac{bkk+ghb}{bk+gp} =$ the verſed ſine to the radius II, or the verſed ſine of the arc deſcribed by the center of oſcillation, which call V; then is V the perpendicular height deſcended by this center, and the velocity it acquires by the deſcent through this ſpace is thus eaſily found, *viz.* as $\sqrt{16\frac{1}{12}} : \sqrt{\text{V}} :: 32\frac{1}{6} : \frac{8.02c}{r\sqrt{2}} \times \sqrt{\frac{bkk+gbp}{bk+gp}} =$ the velocity of the center of oſcillation deduced from the chord of the arc which is actually deſcribed.

Having thus obtained two different expreſſions for the velocity of this center, independent of each other, let an equation be made of them, and it will expreſs the relation of the ſeveral quantities in the queſtion; thus then we have $\frac{bkv}{bk+gp} = \frac{8.02c}{r\sqrt{2}}\sqrt{\frac{bkk+gbp}{bk+gp}}$ from which we obtain $v = \frac{8.02c}{bkr\sqrt{2}}\sqrt{\overline{bk+gp} \times \overline{bkk+gbp}}$ the true expreſſion for the original velocity of the ball the moment before it ſtruck the pendulum.

COROLLARY. But this theorem may be reduced to a form much more ſimple and fit for uſe, and yet be ſufficiently near the truth. Thus, let the root of the compound factor $\sqrt{\overline{bk+gp} \times \overline{bkk+gbp}}$ be extracted, and it will be equal to $\sqrt{b \times pg + bk \times \frac{b+k}{2h}}$ within the 100000th part of

of the truth in fuch cafes as generally happen. But fince $bk \times \frac{b+k}{2b}$ is ufually but about the 300th or 400th part of pg, and that bk differs from $bk \times \frac{b+k}{2b}$ but by about the 80th or 100th part of itfelf, therefore $pg + bk$ is within about the 20000th or 30000th part of $pg + bk \times \frac{b+k}{2b}$. Confequently v is $= 8.02\,c\sqrt{\tfrac{1}{2}b} \times \frac{p+bk}{bkr}$ very nearly. Or, farther, if g be written for k in the laft term bk, then finally v is $= 8.02\,cg\sqrt{\tfrac{1}{2}b} \times \frac{p+b}{bkr}$, or $v = 5.672\,cg\sqrt{b} \times \frac{p+b}{bkr}$, which is an eafy theorem to be ufed on all occafions; and being within about the 3000th part of the truth, it is fufficiently exact for all practical purpofes whatever. Where it muft be obferved, that c, g, k, r, may be taken in any meafures, either feet or inches, &c. provided they be but all of the fame kind; but b muft be in feet, becaufe the theorem is adapted to feet.

SCHOLIUM. As the balls remain in the pendulum during the time of making one whole fet of experiments, by the addition of their weight to it, both its weight and the centers of gravity and ofcillation will be changed by the addition of each ball which is lodged in the wood, and therefore p, g, and h, muft be corrected after every fhot in the theorem for determining the velocity v. Now the fucceeding value of p is always $p+b$;

or

or p muſt be corrected by the continual addition of b: and g is corrected by taking always $g+\frac{k-g}{p+b}b$, or $g+\frac{k-g}{p}b$ nearly for each ſucceſſive value of g; or g is corrected by adding always $\frac{k-g}{p}b$ to the next preceding value of g: and laſtly, b is corrected by taking for its new values ſucceſſively $\frac{pgb+bbk}{pg+bk}$, or by adding always $\frac{k-b}{pg+bk}bk$, or $\frac{k-b}{p}b$ nearly, to the preceding value of b; ſo that the three corrections are made by adding always,

b to the value of p,

$\frac{k-g}{p} \times b$ to the value of g,

$\frac{k-b}{p} \times b$ to the value of b.

Before we proceed to the experiments it may not be improper to take notice of three ſeeming cauſes of error, which have not been brought to account in our theorem for determining the velocity of the ſhot; and to examine here whether their effects can ſenſibly affect the concluſion. Theſe are the penetration of the ball into the wood of the pendulum, the reſiſtance of the air to the back of it, and the friction on the axis: by each of theſe three cauſes the motion of the pendulum ſeems to be retarded. The principle on which our rule is founded ſuppoſes the momentum of the ball to be communicated to the pendulum in an inſtant; but this is not accurately

the caſe, becauſe that this force is communicated during the ſmall time in which the ball makes the penetration; but as this is generally effected before the pendulum has moved one-tenth of an inch out of its vertical poſition, and uſually amounts to ſcarcely more than the 200th part of a ſecond, its effect will be quite imperceptible, and therefore it may ſafely be neglected in theſe experiments. As to the ſecond retarding force, or the reſiſtance of the air to the back of the pendulum, it is manifeſt that it will be quite inſenſible, when it is conſidered that its velocity is not more than three feet in a ſecond, that its ſurface is but about twenty inches ſquare, and that its weight is four or five hundred pounds. Neither can the effect of the laſt cauſe, or the friction on the axis, ever amount to a quantity conſiderable enough to be brought into account in theſe experiments: for, beſides that care was taken to render this friction as ſmall as poſſible, the effect of the little part which does remain is nearly balanced by the effect it has on the diſtance of the center of oſcillation; for as this center was determined from the actual vibrations of the pendulum, the friction on the axis would a little retard its motion, and cauſe its vibrations to be ſlower, and the conſequent diſtance of this center to be greater; ſo that the other parts of our theorem being multiplied by $\sqrt{b}$, or the root of this diſtance, which is

as the time of a vibration, it is evident that the friction in the one case operates against that in the other; and that the difference of the two is the real efficacious cause of resistance, and which therefore is either equal to nothing, or very nearly so.

These general causes of error in the principles of the experiments are therefore safely omitted in the theorem: and our only care must be to guard against accidental errors in the actual execution of the business.

Of the experiments.

The gun, with which the experiments were made, was of brass; the diameter of the bore or cylinder at the muzzle was $2\frac{4}{25}$, or 2.16 inches; but its diameter next the breech was a small matter less, being there only $2\frac{2}{25}$, or 2.08 inches; so that the greatest cast-iron ball it would admit was just $19\frac{1}{2}$ ounces avoirdupois, or $1\frac{1}{4}$ pound wanting half an ounce; but sometimes leaden balls were used, which weighed above $1\frac{3}{4}$ pound, and sometimes long or cylindrical shot which weighed near three pounds; the length of the bore was $42\frac{3}{5}$, or 42.6 inches, so that it was nearly $20\frac{1}{2}$ calibers long.

The powder used was of the sort which is commonly made for government; the quantity was two, four, or

eight ounces to a charge, which was always put into a light flannel bag, and rammed more or lefs, as expreffed in each day's experiments, but without ever ufing any wad before it.

The diftance of the gun from the pendulum was 29 or 30 feet; which diftance was found by firing the piece, with eight ounces of powder without a ball, at different diftances, till the force of the elaftic fluid was found not to move the pendulum.

The penetrations of the balls into the wood were attempted to be taken, but were foon neglected on account of their uncertainty, becaufe of fo many balls ftriking in or near the fame part of the wood. The depth of the penetration feemed to be near about three inches in folid wood when two ounces of powder was ufed.

The firft courfe of experiments was on the 13th of May, 1775, it being a clear, dry day. The weights and meafures then taken were thus, *viz.*

$p = 328$ pounds, the weight of the pendulum,

$g = 72$ inches, the diftance of the center of gravity,

$h = 88$ inches $= 7\frac{1}{3}$ feet, the diftance of the center of ofcillation,

$r = 102\frac{1}{2}$ inches, the diftance to the bottom or tape.

The value of $h = 88$ was determined from the number of forty vibrations being made in a minute; for as

$40^2 : 60^2 :: 4 : 9 :: 39.2 : 88$. The number of ſhot was eight, and the circumſtances and reſults as exhibited in the following table.

Number.	Weight of powder.	Diam. of the ball.	Height of the charge.	Struck below the axis, k.	Weight of the ball.	Weight of the ball, b.	Values of p.	Values of g.	Chord of the arc, c.	Veloc. *per* ſecond.
	Oz.	In.	Inches.	Inches.	Oz.	Pounds	Pounds	Inches.	Inches.	Feet.
1	2	1.98		92.5	$17\frac{1}{2}$	1.094	328.0	72.0	13.0	458
2	2	1.98		92.5	$17\frac{1}{2}$	1.094	329.1	72.1	17.8	631
3	2	1.98	3.15	91.6	$17\frac{1}{2}$	1.094	330.2	72.2	18.1	650
4	2	1.97	3.15	91.	$17\frac{1}{4}$	1.078	331.3	72.3	17.6	646
5	2	1.97	3.15	90.5	$17\frac{1}{4}$	1.078	332.3	72.3	16.3	604
6	2	1.96	3.15	92.4	17	1.063	333.4	72.4	16.2	598
7	4	1.97	4.5	92.	$17\frac{1}{4}$	1.078	334.4	72.5	24.0	881
8	4	1.96	4.5	90.5	17	1.063	335.5	72.5	25.0	950

By computing the velocities from our theorem inveſtigated in the corollary, they come out as they are here regiſtered in the laſt column of the table, and they are all pretty regular excepting the firſt one, which is about one-fourth part leſs than the reſt with the ſame weight of powder, and which irregularity muſt have been cauſed by ſome unperceived accident. The values of p and g were each corrected by their reſpective theorems; but the value of b was kept the ſame ($7\frac{1}{2}$ feet) through-

out, becaufe that its correction was fo fmall as not to make a difference of above a foot or two at moft in the velocity: and for the fame reafon this correction is neglected, as quite unneceffary, in the reft of the experiments of the other days following.

The mean velocity of the fecond, third, fourth, fifth, and fixth numbers is 626, and of the feventh and eighth it is 915; that is, the velocity with two ounces of powder was 626 feet *per* fecond, and that with four ounces was 915 feet; and thefe two velocities are in the ratio of 1 to 1.46. But the mean weight of the balls in the former cafe was $17\frac{1}{3}$ ounces, and in the latter it was $17\frac{1}{8}$ ounces; and the ratio of the quantities of powder was that of 1 to 2. But the direct fub-duplicate ratio of the powder, compounded with the inverfe fub-duplicate ratio of the weights of the fhot, forms the ratio of 1 to 1.42, which is nearly equal to the ratio (1 to 1.46) of the velocities; that is, in this inftance the velocities are very nearly as the fquare roots of the quantities of powder directly, and the fquare roots of the weights of the balls inverfely. The powder was forced up with only one ftroke of the rammer.

The fecond courfe was performed on the 3d of June, 1775, which was a clear, dry day, but windy. Some of the

the experiments of this day are doubtful, as indeed is evident from their irregularity, on account of the wind blowing the tape, which was not very properly ſecured by the little brazen machine through which it was made to ſlide.

The powder was taken from the bottom of a barrel, and the charges rammed a little cloſer than thoſe of the former day; and ſo tight did the ſhots fit towards the breech, that many ſtrokes of the rammer were neceſſary to drive them home.

The fourth and fifth ſhots were of a long form, which may be called ſpherico-cylindrical, as they were cylinders terminated by hemiſpherical ends, ſo that their ſection through the axis was of this form ▭, and the length of the axis was near double the diameter of the ſhot.

The fourth ſhot, or firſt of the long ſort, ſtruck ſide-ways, making a hole of the ſhape of the above ſection, only its length or axis was not horizontal but vertical, thus ▯.

The laſt ſhot lay obliquely in the wood; it appeared to have ſtruck with its end foremoſt, or nearly ſo, as the oblique poſition in which it lay ſeemed to be cauſed by its ſtriking againſt a former ſhot lodged in the wood, with the hance of its end, ſo as to flatten it in that part.

Of the pendulum, the weight, length, and centers of gravity and of oscillation were the fame as when taken the former day before the experiments were made; the former balls having been extracted, and the holes filled up with wood.

Number.	Weight of powder.	Diam. of the ball.	Height of the charge.	Struck below the axis, k.	Weight of the ball.	Weight of the ball, b.	Values of p.	Values of g.	Chord of the arc, c.	Veloc. per second.
	Oz.	In.	Inches.	Inches.	Oz.	Pounds	Pounds	Inches.	Inches	Feet.
1	2	2.08	2.85	$88\frac{1}{2}$	$19\frac{1}{2}$	1.219	328.0	72.0	24.3	800
2	2	2.08	2.85	89	$19\frac{1}{2}$	1.219	329 2	72.1	30.5	1003
3	2	2.08	2.85	$93\frac{1}{2}$	$19\frac{1}{2}$	1.219	330.4	72.1	30.0	943
4	2	2 08	3 35	$92\frac{1}{2}$	$46\frac{1}{2}$	2.906	331.6	72.2	57 0	767
5	2	2.08	3.35	93	$46\frac{1}{2}$	2.906	334.5	72.4	54.0	731

Here the firft fhot is again fo much fmaller than the two following ones, that fome irregularity muft have attended it, on which account we cannot make any ufe of it. The mean between the fecond and third is 973; and between the fourth and fifth the mean is 749; that is, the velocity of the $19\frac{1}{2}$ ounce ball is 973, and that of the $46\frac{1}{2}$ ounce fhot 749 feet *per* fecond, which two numbers are in the ratio of 1.3 to 1. But the reciprocal fubduplicate ratio of the weights ($19\frac{1}{2}$ and $46\frac{1}{2}$) is the ratio of 1.54 to 1: therefore, in this inftance, the velocity of the heavier fhot is a little lefs than would arife from the

inverfe

inverſe ratio of the ſquare roots of the weights of the ſhot. But the accurate ratio cannot certainly be drawn from theſe numbers, on account of the doubtfulneſs of ſome of them, as was before obſerved.

It is very remarkable, that in the experiments of this day, the mean velocity with two ounces of powder is 973, whereas it was no more than 626 in the former day with the ſame quantity of powder, notwithſtanding the balls were heavier with the greater velocity in the proportion of 19 to 17 nearly. This remarkable difference muſt be chiefly owing to the windage in the firſt courſe: and from hence we may perceive the great advantage to be gained by the uſe of balls approaching in proportion nearer to the diameter of the bore of the gun than what is preſcribed in the preſent eſtabliſhment. Poſſibly, however, ſome part of this difference might be owing to ſome ſmall inequality in the powder, as that which was uſed this day was taken from the bottom of a barrel. Perhaps alſo ſome part of the effect may be owing to the greater degree of ramming which the powder had in this courſe.

The third courſe was made on the 12th of June, 1775, it being a clear, dry, and calm day. The powder in the experiments of this day was rammed in the ſame degree

degree as in the laſt one. It was alſo nearly the ſame in the ſucceeding days, as may be perceived by inſpecting the fourth column of each courſe, which, denoting the height of the charge, ſhews the degree of compactneſs with which the powder was lodged in the piece. The dimenſions, as taken this day, were thus:

p = 324 pounds, the weight of the pendulum.

g = 71.4 inches, the diſtance of the center of gravity.

h = 88 inches = $7\frac{1}{3}$ feet, the diſtance of the center of oſcillation.

r = $102\frac{1}{2}$ inches, the whole length to the tape.

Number.	Weight of powder.	Diam. of the ball.	Height of the charge.	Struck below the axis, k.	Weight of the ball.	Weight of the ball, b.	Values of p.	Values of g.	Chord of the arc, c.	Veloc. *per* ſecond.
	Oz.	Inches.	Inches.	Inches.	Oz.	Pounds	Pounds	Inches.	Inches.	Feet
1	2	2.080	2.85	94	$19\frac{1}{2}$	1.219	324.0	71.4	23.0	700
2	2	2.036	2.85	94	$18\frac{1}{4}$	1.141	325.2	71.5	24.5	799
3	2	2.045	2.85	$93\frac{1}{2}$	$18\frac{1}{2}$	1.156	326.4	71.6	22.0	715
4	4	2.062	4.	$92\frac{1}{4}$	19	1.188	327.5	71.7	27.3	880
5	4	2.036	4.	$93\frac{1}{2}$	$18\frac{1}{4}$	1.141	328.7	71.7	35.0	1163
6	4	2.045	4.	$93\frac{1}{2}$	$18\frac{1}{2}$	1.156	329.9	71.8	33.0	1087

Here the common mean weight of the ball is $18\frac{5}{6}$ ounces, the mean velocity with two ounces of powder is 738, and that with four ounces of powder is 1043 feet *per* ſecond. The ratio of theſe two velocities is that of

1 to

1 to 1.414; that is, accurately the ratio of the ſquare roots of the quantities of powder.

Of the experiments made with the other pendulum, which is repreſented in fig. 2.

The firſt pendulum was gradually more and more rent and ſhattered by the firing of ſo many balls into it, till at the end of the laſt courſe of experiments it had become quite uſeleſs. Another was then fitted up, and with it were performed the two following courſes.

This ſecond pendulum conſiſted of a cubical block of ſound elm, of near two feet long, fixed to the iron ſtem, but not exactly in the manner of the former; for in this the ſtem was placed vertically over the center of the top-end, to which point it continued whole, but there divided in two, each paſſing to right and left over the top down the ſides, and returning along the bottom, and being at proper intervals faſtened to the wood with iron pins. A thick ſheet of lead was faſtened over each of the two upright faces into which the ſhot were to be fired, both to guard them from ſplintering very much, and to add to the weight of the pendulum. The whole was then firmly ſecured by two very thick iron bands or hoops, paſſed horizontally quite around the wood, and

 firmly

firmly fixed to it, the one next the upper end, and the other near the lower, so as strongly to resist the endeavours of the shot to split the wood.

The whole weight of the pendulum, thus fitted up, was 552 pounds; its whole length, from the middle of the axis to the tape at the bottom, was 101 inches; the distance to the center of gravity was 78 inches; and the distance to the center of oscillation was 88 inches equal to $7\frac{1}{3}$ feet, which was exactly the same as that of the former pendulum, their numbers of vibrations being alike in the same time.

Instead of suspending this pendulum, after the manner of the former, by the ends of its axis in grooves turned to fit them, they were only placed on flat, level pieces of wood, on which this pendulum vibrated much freer than the other did; but a small nail was driven into the supporting wood, just behind each end of the axis, to prevent the stroke of the shot from throwing it off the stand.

To this pendulum was adapted a better machine for the tape to slide through than the former one was, the inconvenience of which had often been experienced by its catching and entangling the tape, so as to interrupt its free motion, and once indeed to break it. This new one, however, is at once very simple, and perfectly free from every

every inconvenience, giving juſt the neceſſary degree of friction to the tape, without ever ſtopping its motion; ſo that of the real quantity drawn out by the vibration of the pendulum there could not poſſibly be the leaſt doubt. This ſimple contrivance conſiſted barely of about ſix or eight inches of the liſt of woollen cloth faſtened upon the arch of a ſmall piece of wood, which was ſhaped into the form of the ſegment of a circle thus , the tape being made to paſs through between the curved ſide and the liſt, which was moderately ſtretched and faſtened by its two ends to thoſe of the little arch.

Upon the whole, the machinery was all ſo perfect, and every circumſtance attending the experiments of the two enſuing days ſo carefully obſerved, that I can with great ſafety rely on the concluſions reſulting from them. And as thoſe of the one day were made with leaden balls, and thoſe of the other with iron ones, which differ greatly in weight, every other circumſtance being the ſame, they afford very good means for diſcovering the law of the different weights of ſhot, while the variations in the powder from two to four and eight ounces furniſh us with the rule for the different quantities of it.

The fourth courſe was on the 20th of July, a fine clear day. The powder was a mixture of ſeveral of the ſorts made for government, and the balls were of lead. The quantities of powder were two, four, and eight ounces alternately; and the dimenſions at firſt were thus:

$p = 552$ pounds, the whole weight of the pendulum.

$r = 101$ inches, its whole length.

$g = 78$ inches, the diſtance of the center of gravity.

$h = 88$ inches $= 7\frac{1}{3}$ feet, that of the center of oſcillation.

Number.	Weight of powder.	Diam. of the ball.	Height of the charge.	Struck below the axis, k.	Weight of the ball.	Weight of the ball, b.	Values of p.	Values of g.	Chord of the arc c.	Veloc. per second.
	Oz.	Inches.	Inches.	Inches.	Oz.	Pounds	Pounds	Inches.	Inches.	Feet
1	2	2.021	2.85	90.	$28\frac{1}{4}$	1.766	552.0	78.0	14.8	612
2	4	2.021	4.4	87.	$28\frac{1}{4}$	1.766	553.8	78.0	20.5	879
3	8	2.032	7.1	87.	$28\frac{3}{4}$	1.797	555.5	78.1	27.5	1164
4	2	2.026	2.85	90.	$28\frac{1}{2}$	1.781	557.3	78.1	15.0	622
5	4	2.026	4.4	88.	$28\frac{1}{2}$	1.781	559.1	78.1	20.5	871
6	8	2.032	7.1	92.	$28\frac{3}{4}$	1.797	560.9	78.2	28.5	1154
7	2	2.021	2.85	89.8	$28\frac{1}{4}$	1.766	562.7	78.2	14.3	605
8	4	2.026	4.4	91.3	$28\frac{1}{2}$	1.781	564.5	78.2	21.0	870
9	8	2.026	7.1	87.	$28\frac{1}{2}$	1.781	566.2	78.3	26.8	1169

Let us now collect together the ſeveral velocities belonging to the ſame quantity of powder, in order to take their means, thus:

Veloc.

	Veloc. with 2 ounces.	Veloc. with 4 ounces.	Veloc. with 8 ounces.
	612	879	1164
	622	871	1154
	605	870	1169
	3)1839	3)2620	3)3487
The means,	613	873	1162

The uniformity of theſe velocities is very ſtriking, and the means with two, four, and eight ounces of powder are 613, 873, and 1162, which are in the ratio of 1, 1.424, and 1.9; theſe numbers are nearly in the ratio of the ſquare roots of the quantities (2, 4, and 8) of powder, the numbers in this latter ratio being 1, 1.414, and 2, where the ſmall difference lies chiefly in the laſt number. A ſmall part of this defect in the greateſt velocity is to be attributed to the mean weight of the balls uſed with it being greater than in the others; for the mean weight of the balls uſed with eight ounces of powder is $28\frac{2}{3}$ ounces, while that with the two and four ounces is only $28\frac{1}{3}$; the reciprocal ſub-duplicate ratio of theſe is that of 1 to 1.006, in which proportion, increaſing 1.9 the number for the greater velocity, it becomes 1.91, which ſtill falls ſhort of 2 by .09, which is about the $\frac{1}{22}$d part too ſmall for the ſub-duplicate ratio of the

the powder. This defect of a 22d part is owing to three evident causes, *viz.* 1. The less length of cylinder through which the ball was impelled; for by inspecting the fourth column, denoting the height of the charge, it appears, that the balls lay three or four inches nearer to the muzzle of the piece with the eight ounce charge than with the others. 2. The greater quantity of elastic fluid which escaped in this case than in the others by the windage; this happens from its moving with a greater velocity, in consequence of which a greater quantity escâpes by the vent and windage than with the smaller velocities. 3. The third cause is the greater quantity of powder blown out unfired in this case than in that of the less velocities; for the ball which was impelled with the greater velocity would be sooner out of the piece than the others, and the more so as it had a less length of the bore to move through; and if powder fire in time, which cannot be denied, although indeed that time is manifestly very short, a greater quantity of it must remain unfired when the ball with the greater velocity issues from the piece, than when that which has the less velocity goes out, and still the more so as the bulk of powder which was at first to be inflamed in the one case so much exceeded that in the others. The effect, however, will arise chiefly from the first and last

of

of thefe three caufes, as that of the fecond will amount to very little; becaufe that the effect arifing from the greater velocity with which the fluid efcapes at the vent and windage, is partly balanced by the fhorter time in which it acts.

From the above reflections we may alfo perceive, how fmall the quantity of powder is which is blown out unfired in any of thefe cafes, and the amazing quicknefs with which it fires in all cafes: for although the time in which the ball paffed through the barrel, when impelled by the eight ounces of powder, was not greatly different from the half only of the time in which it was impelled by the two ounces, it is evident that in half the time there was nearly four times the quantity of powder fired.

The fifth or laft courfe was on the 21ft of September, 1775, fine clear weather, but a little windy.

The machinery and the balls were of iron, but powder the fame as in the laft courfe, and the dimenfions as follows:

$p = 553$ pounds, the weight of the pendulum.

$r = 101$ inches, its length.

$g = 78\frac{1}{8}$ inches, the diftance of the center of gravity.

$h = 84.775$ inches $= 7.065$ feet, that of the center of ofcillation, the pendulum making 68 vibrations in 100 feconds.

Number

Number.	Weight of powder.	Diam. of the ball.	Height of the charge.	Struck below the axis, k.	Weight of the ball.		Weight of the ball, b.	Values of p.	Values of g.	[illegible]	[illegible]
	Oz.	Inches	Inches	Inches.	Oz.	Dr.	Pounds	Pounds	Inches	Inch.	Feet.
1	2	2.062	3.	88.3	19	0	1.188	553.0	78.1	11.4	702
2	4	2.062	4.3	88.3	19	0	1.188	554.2	78.1	17.3	1068
3	8	2.062	6.7	91.0	19	0	1.188	555.5	78.2	23.6	1419
4	2	2.070	3.	90.7	19	$3\frac{1}{2}$	1.201	556.8	78.2	11.4	682
5	4	2.080	4.3	90.7	19	$8\frac{1}{2}$	1.221	558.1	78.2	17.3	1020
6	8	2.064	6.7	90.7	19	$0\frac{1}{4}$	1.190	559.4	78.2	22.3	1352
7	2	2.060	3.	91.	18	15	1.184	560.6	78.3	11.4	695
8	4	2.058	4.3	90.	18	14	1.180	561.9	78.3	15.3	948
9	8	2.049	6.7	90.	18	$9\frac{3}{4}$	1.163	563.1	78.3	22.9	1443
10	2	2.047	3.	88.3	18	9	1.160	564.3	78.3	10.9	703
11	4	2.037	4.3	88.3	18	$4\frac{1}{3}$	1.142	565.5	78.4	14.8	973
12	8	2.036	6.7	88.3	18	$3\frac{3}{4}$	1.140	566.6	78.4	20.6	1360
13	2	2.034	3.	92.	18	3	1.137	567.8	78.4	11.4	725
14	4	2.034	4.3	92.	18	3	1.137	569.0	78.4	15.0	957
15	8	2.031	6.7	94.3	18	$1\frac{1}{4}$	1.131	570.1	78.5	22.5	1412

Let us now take the means among thofe of the fame quantity of powder, thus:

Veloc.

	Veloc. with 2 ounces.	Veloc. with 4 ounces.	Veloc. with 8 ounces.
	702	1068	1419
	682	1020	1352
	695	948	1443
	703	973	1360
	725	957	1412
	5)3507	5)4966	5)6986
The means,	701	993	1397

And thefe mean velocities with two, four, and eight ounces of powder, are as the numbers 1, 1.416, and 1.993; but the fub-duplicate ratio of the weights (two, four, and eight) of powder gives the numbers 1, 1.414, and 2, to which the others are fufficiently near. It is obvious, however, that the greateft difference lies in the laft number which anfwers to the greateft velocity, and which is again in defect. It will ftill be a little more in defect if we make the allowance for the weights of the balls; for the mean weight of the balls with the two and four ounces is $18\frac{3}{4}$ ounces, but of the eight ounces it is $18\frac{3}{5}$; diminifhing therefore the number 1.993 in the reciprocal fub-duplicate ratio of $18\frac{3}{5}$ to $18\frac{3}{4}$, it becomes 1.985, which falls fhort of the number 2 by .015 or the 133d part of itfelf; which defect is to be attri-

attributed to the ſame cauſes as it was in the laſt courſe of experiments before explained.

Let us now compare the correſponding velocities in this courſe and the laſt.

In this courſe they are	701, 993, 1397;
In the laſt they were	613, 873, 1162.

Now the ratio of the firſt two numbers, or the velocities with two ounces of powder, is that of 1 to 1.1436; the ratio of the next two, is that of 1 to 1.1375; and the ratio of the laſt is that of 1 to 1.2022. But the mean weight of the ſhot was, for two and four ounces of powder $28\frac{1}{3}$ ounces in the laſt courſe, and $18\frac{3}{4}$ ounces in this; and for eight ounces of powder, it was $28\frac{1}{5}$ in the laſt, and $18\frac{3}{5}$ in this: taking now the reciprocal ſubduplicate ratios of theſe weights of ſhot, we obtain the ratio of 1 to 1.224 for that of the balls which were fired with two ounces and four ounces of powder, and the ratio of 1 to 1.241 for the balls which were fired with eight ounces. But the real ratios above found are not greatly different from theſe. And the variation of the actual velocities from this law of the weights of ſhot incline the ſame way in this courſe, as they appeared to do in the ſecond courſe of theſe experiments.

We

We may now collect into one view the principal inferences that have resulted from these experiments.

1. And first, it is made evident by them, that powder fires almost instantaneously, seeing that almost the whole of the charge fires though the time be much diminished.

2. The velocities communicated to balls, or shot of the same weight, with different quantities of powder, are nearly in the sub-duplicate ratio of those quantities. A very small variation, in defect, taking place when the quantities of powder become great.

3. And when shot of different weights are fired with the same quantity of powder, the velocities communicated to them are nearly in the reciprocal sub-duplicate ratio of their weights.

4. So that, universally, shot which are of different weights, and impelled by the firing of different quantities of powder, acquire velocities which are directly as the square roots of the quantities of powder, and inversely as the square roots of the weights of the shot, nearly.

5. It would therefore be a great improvement in artillery to make use of shot of a long form, or of heavier matter; for thus the momentum of a shot, when fired

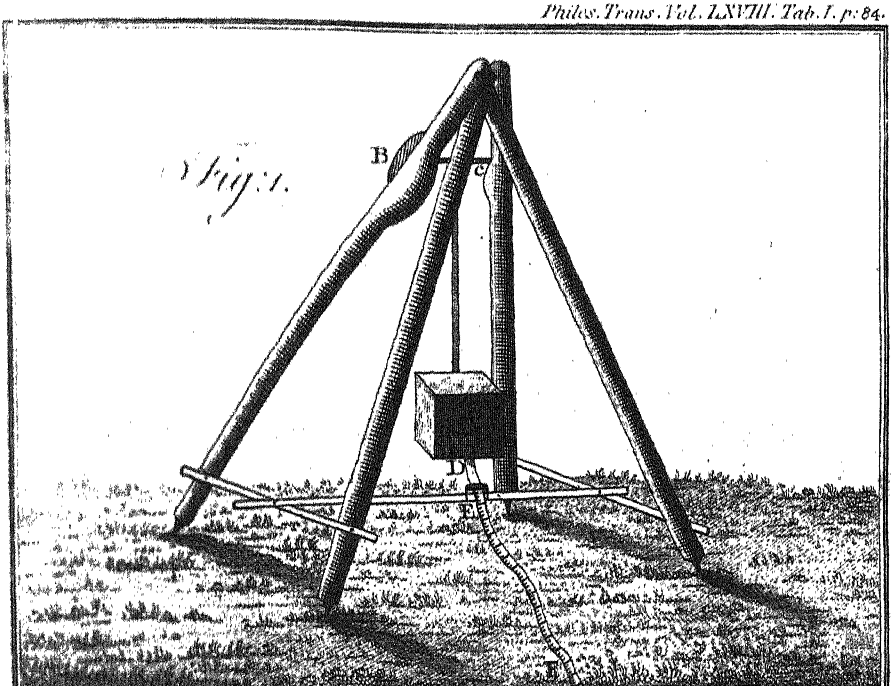
with the fame weight of powder, would be increafed in the ratio of the fquare root of the weight of the fhot.

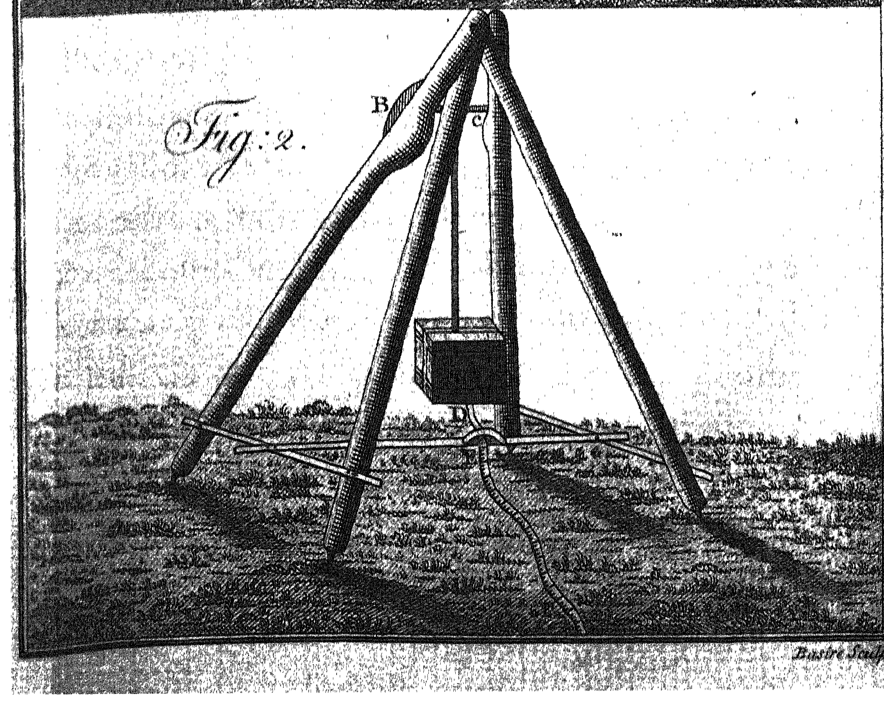
6. It would alfo be an improvement to diminifh the windage; for by fo doing, one-third or more of the quantity of powder might be faved.

7. When the improvements mentioned in the laft two articles are confidered as both taking place, it is evident that about half the quantity of powder might be faved, which is a very confiderable object. But important as this faving may be, it feems to be ftill exceeded by that of the article of the guns; for thus a fmall gun may be made to have the effect and execution of one of two or three times its fize in the prefent mode, by difcharging a fhot of two or three times the weight of its natural ball or round fhot. And thus a fmall fhip might difcharge fhot as heavy as thofe of the greateft now made ufe of.

Finally, as the above experiments exhibit the regulations with regard to the weights of powder and balls, when fired from the fame piece of ordnance, &c.; fo by making fimilar experiments with a gun, varied in its length, by cutting off from it a certain part before each courfe of experiments, the effects and general rules for the different lengths of guns may be certainly determined by them. In fhort, the principles on which thefe

expe-





Basire Sculp.

experiments were made, are ſo fruitful in conſequences, that, in conjunction with the effects reſulting from the reſiſtance of the medium, they ſeem to be ſufficient for anſwering all the enquiries of the ſpeculative philoſopher, as well as thoſe of the practical artilleriſt.

IV. *A new Case in Squinting, by* Erasmus Darwin, *M. D. F. R. S.; communicated by* Thomas Aſtle, *Esq. F. R. S.*

Litchfield, March 10, 1777.

Read Jan. 15, 1777.

THE following caſe in ſquinting, as a ſimilar one has not been recorded or explained by others, may perhaps merit your attention from its novelty.

About ſix years ago I was deſired to ſee a child of the reverend Dr. SANDFORD, in Shropſhire, to determine if any method could be deviſed to cure him of ſquinting. The child was then about five years old, and exceedingly tractable and ſenſible, which enabled me to make the following obſervations upon him with great accuracy and frequent repetition.

1. He viewed every object which was preſented to him with but one eye at a time.

2. If the object was preſented on his right-ſide, he viewed it with his left eye; and if it was preſented on his left-ſide, he viewed it with his right eye.

3. He turned the pupil of that eye, which was on the ſame ſide with the object, in ſuch a direction that the image

image of the object might fall on that part of the bottom of the eye where the optic nerve enters it.

4. When an object was held directly before him, he turned his head a little to one ſide, and obſerved it with but one eye, *viz.* with that moſt diſtant from the object, turning away the other in the manner above deſcribed; and when he became tired with obſerving it with that eye, he turned his head the contrary way, and obſerved it with the other eye alone, with equal facility; but never turned the axes of both eyes on it at the ſame time.

5. He ſaw letters, which were written on bits of paper, ſo as to name them with equal eaſe, and at equal diſtances, with one eye as with the other.

6. There was no perceptible difference in the diameters of the iriſes, nor in the contractibility of them, after having covered his eyes from the light. Theſe obſervations were carefully made by writing ſingle letters on ſhreds of paper, and laying wagers with the child that he could not read them when they were preſented at certain diſtances and directions.

From theſe circumſtances it appeared, that there was no defect in either eye, which is the common cauſe of ſquinting, ſo well obſerved by M. BUFFON and Dr. REID; and hence, that the diſeaſe was ſimply a depraved habit of moving his eyes, and might probably be occaſioned by

by the form of a cap or head-drefs, which might have been too prominent on the fides of his face, like bluffs ufed on coach-horfes; and might thence, in early infancy, have made it more convenient for the child to view objects placed obliquely with the oppofite eye, till by habit the *mufculi adductores* were become ftronger, and more ready for motion than their antagonifts.

A paper gnomon was made, and fixed to a cap; and when this artificial nofe was placed over his real nofe, fo as to project an inch between his eyes, the child, rather than turn his head fo far to look at oblique objects, immediately began to view them with that eye which was next to them. But the death of Dr. SANDFORD, which happened foon after, occafioned the removal of his family; and the grief and cares of Mrs. SANDFORD prevented this, and the other methods propofed, from being put in execution.

About a month ago I had again an opportunity of feeing mafter D. SANDFORD, and obferved all the circumftances of his mode of vifion to be exactly as they were fix years before, except that they feemed eftablifhed by longer habit; fo that I could not by any means induce him to bend the axes of both his eyes on the fame object, not even for a moment.

A gnomon

A gnomon of thin braſs was made to ſtand over his noſe, with a half circle of the ſame metal to go round his temples; theſe were covered with black ſilk, and by means of a buckle behind his head, and a croſs-piece over the crown of his head, this gnomon was managed ſo as to be worn without any inconvenience, and projected before his noſe about two inches and an half. By the uſe of this gnomon he ſoon found it leſs inconvenient to view all oblique objects with the eye next to them, inſtead of the eye oppoſite to them.

After this habit was weakened by a week's uſe of the gnomon, two bits of wood, about the ſize of a gooſe-quill, were blackened all but a quarter of an inch at their ſummits; theſe were frequently preſented for him to look at, one being held on one ſide the extremity of his black gnomon, and the other on the other ſide of it. As he viewed theſe they were gradually brought forwards beyond the gnomon, and then one was concealed behind the other: by theſe means, in another week, he could bend both his eyes on the ſame object for half a minute together.

By the practice of this exerciſe before a glaſs, almoſt every hour in the day, he became in another week able to read for a minute together with his eyes both directed on the ſame objects; and I have no doubt, if he has pa-

tience enough to perſevere in theſe efforts, but he will in the courſe of ſome months overcome this unſightly habit.

I ſhall conclude the account of this caſe by adding, that all the other ſquinting people I have had occaſion to attend to, have had one eye much leſs perfect than the other, according to the obſervations of Mr. BUFFON and Dr. REID. Theſe patients, where the diſeaſed eye is not too bad, are certainly curable by covering the beſt eye many hours in a day; as, by a more frequent uſe of the weak eye, it not only acquires a habit of turning to the objects which the patient wiſhes to ſee, but gains at the ſame time a more diſtinct viſion; and the better eye at the ſame time ſeems to loſe ſomewhat in both theſe reſpects, which alſo facilitates the cure.

This evinces the abſurdity of the practice of prohibiting thoſe who have weak eyes from uſing them; ſince the eye, as well as every other part of the body, acquires ſtrength from that degree of exerciſe which is not accompanied with pain or fatigue; and I am induced to believe, that the moſt general cauſe of ſquinting in children originates from the cuſtom of covering the weak eye, which has been diſeaſed by any accidental cauſe, before the habit of obſerving objects with both eyes was perfectly eſtabliſhed.

The facility with which master SANDFORD received the images of oblique objects on the insensible part of the retina of one eye, whilst he viewed them with the other, induced me to observe the size of this insensible spot, and to endeavour to ascertain the cause of it.

There was formerly a dispute among philosophers, whether the choroid coat of the eye or the retina was the immediate organ of vision, which has lately been revived in some measure in Dr. PRIESTLEY's valuable History of Light and Colours; and it was then thought by one party in this dispute, that the defect of the choroid coat, where the optic nerve enters the eye, was the cause of this want of vision in that part.

But the following observation shews beyond a doubt the fallacy of this supposition: the diameter of the optic nerve, at its entrance into the eye, is about one-sixth of an inch, and the perforation of the choroid coat, through which it passes, must of necessity be of the same diameter: now the dark spot, which is seen in objects opposed to the center of the optic nerve, if it was occasioned by the deficiency of the choroid coat, should, at nine inches distance from the eye, be fifty-four times the diameter of this aperture, or nine inches in diameter; whereas I find, by experiment, that a paper of one inch in diameter could not be totally concealed at nine inches distance

from my eye; and M. LE CAT by accurate obſervations found, that the inſenſible part of his eye was but between the thirtieth and fortieth part of an inch in diameter. This experiment is ſo eaſily made, that it can be attended with no fallacy; and at the ſame time that it ſhews that the inſenſible ſpot, where the optic nerve enters the eye, is not owing to the deficiency of the choroid coat, intirely ſubverts the opinion of the choroid coat being the organ of viſion; for viſion exiſts where the choroid coat is not.

Nor is the inſenſibility of the center of the optic nerve owing to the ingreſs of the arteries along with it into the eye; for a large branch of this artery runs along the bottom of the eye, where viſion is moſt diſtinct, and becauſe all this artery is covered with the expanſe of the retina on the external ſide of it. Mr. SAVAGE made an experiment for another purpoſe, which however ſhews, that the optic artery, where it is branched under or through the retina, does not much diſturb the power of viſion. It is this: if you look on a white wall on a luminous day, with the Sun ſhining on the wall only by its reflected light, you will diſcern the parts of the wall become darker and lighter at every pulſation of the optic artery. This darker and lighter appearance is like net-work, and not uniform like the wall itſelf; but the

the whole, though rather darker while the diaſtole of the artery compreſſes the retina, is yet diſtinctly viſible.

The following circumſtance ſeems to give riſe to the inſenſibility of the central part of the optic nerve at its ingreſs into the eye, which I have obſerved in ſeveral calves' eyes. The point of a pair of ſciſſars was introduced behind the ciliary circle, and the whole of the cornea, aqueous humour, iris, and cryſtalline, being removed, the retina was beautifully ſeen through the vitreous humour ſomewhat magnified. On expoſing this to the ſun-ſhine, and inſpecting it with nicety, a white filament, about the tenth of an inch in length, ariſing from the center of the optic nerve, was ſeen aſcending ſtraight upwards into the vitreous humour, like a thin white worm. The uſe of this may be to ſupply the vitreous humour or cryſtalline with nouriſhment, whether it be a nerve or an empty blood-veſſel; but this is certain, that its riſing ſo high above the ſurface of the retina muſt render it incapable of viſion: whence there is juſt reaſon to conclude, that this conformation muſt be the true cauſe of the inſenſibility of this part of the eye.

I do not affirm, that the human eye, either during infancy or in our riper years, is ſimilar in conformation to that of a calf, nor have we ſufficient opportunities to ob-

ferve them; but I fufpect this veffel may, after the growth of the animal, be totally obliterated; and that, in fome few inftances, the optic nerve may even in this part become fenfible to light. One inftance I am certain I have feen, as it was in a man capable of the moft patient and accurate obfervation, who on numberlefs repeated trials, at different times, in my prefence, could never lofe fight of the fmalleft object with either of his eyes.

Supplement to the cafe in fquinting.

IT fince occurred to me, that the unufual mode of fquinting defcribed in the above paper muft have arifen from fome original difference in the fenfibility of fome parts of the eye, which might have rendered it more eafy for mafter SANDFORD, when a child, to obferve objects with one eye only, and that with the eye moft diftant from objects prefented obliquely to him.

Two circular papers, each of four inches diameter, were ftuck againft the wall, their centers being exactly at eight inches diftance from each other. On clofing one eye, and viewing the central fpot of one of thefe papers with the eye furtheft from it, and then retreating twenty-

twenty-ſix inches from it, the other paper became inviſible. This experiment was made on five people of various ages, from ten years old to forty; and the paper diſappeared to them all at about this diſtance, or an inch or two more or leſs: but to maſter SANDFORD the paper diſappeared at about thirteen inches diſtance from the wall. Theſe papers were afterwards removed to twelve inches, and then to four inches interval between them; and by the niceſt obſervations on repeated trials I found, that the paper, equally with one eye as with the other, uniformly diſappeared to him at about half the diſtance it did to five others.

Another curious circumſtance is, that as large a paper diſappeared to him at half the diſtance as it did to others at the whole diſtance; and hence the inſenſible part of the center of the optic nerve in his eyes is, as near as can be eſtimated, four times the area of the inſenſible part of the eyes of other people, at the ſame time that the angle made between the ingreſs of the optic nerve and the bottom of the eye is twice as great as in others.

It is eaſy to conceive that, in early infancy, when any object which the child wiſhed to inſpect was preſented obliquely to him, that on this firſt indiſtinct view of it, before either eye could be turned towards it, it would appear much more brilliant and diſtinct to the contrary eye,

eye, than to that neareſt the object, as ſo great a part of it would now fall on the large inſenſible part of that eye. This muſt naturally induce him to view it with the oppoſite eye, to which it already appeared more brilliant and diſtinct: and this to him would be ſo much eaſier to accompliſh, as the inſenſible part of the neglected eye was great enough to receive as large a part of an object as is uſually viewed at once with accuracy, and hence would not confuſe the viſion of the other.

I muſt beg leave to add, that by wearing the artificial noſe he has greatly corrected the habit of viewing objects with the eye furtheſt from them; and has more and more acquired the voluntary power of directing both his eyes to the ſame object, particularly if the object be not more than four or five feet from him; and will, I believe, by reſolute perſeverance, intirely correct this unſightly deformity. Nothing but the curioſity and novelty of the ſubject can excuſe the length of this paper.

V. *A Cure of a Muſcular Contraction by Electricity.* By Miles Partington, *in a Letter to* William Henly, *F. R. S.*

Great Ruſſel-ſtreet, June 13, 1777.

DEAR SIR,

Read Jan. 15, 1777.

IT is ſome time ſince you informed me that you had mentioned to Sir JOHN PRINGLE Miſs LINGFIELD's cure by electricity; that it excited his attention; and that it was his opinion, that the communication of it to the Royal Society would be deemed important and uſeful. I hope you will not blame my delay in the compliance with your requeſt. I have waited for no other purpoſe than to obtain the lateſt account of the permanency of thoſe good effects, which ſhe had then but recently experienced from our electrical experiments upon her. Of theſe advantages we have both had repeated confirmation; and I may now, I believe, with ſtrict propriety, from the notes I made for my own ſatiſfaction, ſubmit the following particulars of them to the inſpection of whomſoever your judgement ſhall direct, or to appropriate them to any other purpoſe you pleaſe. As you

 were

were prefent when I firft waited on this unhappy young lady, you will recollect the condition in which we found her. Her head was drawn down over her right fhoulder; the back part of it was twifted fo far round, that her face turned obliquely towards the oppofite fide, by which deformity fhe was difabled from feeing her feet, or the fteps as fhe came down ftairs. The *fterno-maftoideus* mufcle was in a ftate of contraction and rigidity. She had no material pain on this fide of her neck; but, owing to the extreme tenfion of the teguments of the left fide, fhe had a pain continually, and often it was very violent, particularly in fudden changes of the weather. Her pulfe was weak, quick, and irregular. She was fubject to a great irritability, had frequently a little fever, which came on of an evening, and left her before morning; her fpirits were generally exceedingly oppreffed, and at times fhe was flightly paralytic.

She dated the origin of her diforder at fomething more than two years from that period. She was fuddenly feized, going out of a warm room into the cold air, with a pain upon the back of her head, which admitted of fmall abatement for fome months, contracting gradually the mufcles to the melancholy deformity we then beheld; and notwithftanding every prudent means had been ufed to fubdue it, and fhe ftrictly adhered to every article

prescribed to her by the faculty, she was sensible of little variation since, and that rather on the unfavourable side.

I urged her to make a trial of Electricity. She was willing while she was in London to try the experiment; and, though the weather was remarkably tempestuous, she came to me the first tolerable day, and was electrified the first time February 18, 1777.

I sat her in an insulated chair, and, connecting it by a chain to the prime conductor of a large electrical machine, I drew strong sparks from the parts affected for about four minutes, which brought on a very profuse perspiration (a circumstance she had been unaccustomed to) which seemed to relax the *mastoideus* muscle to a considerable degree; but, as the sparks gave her a good deal of pain, I desisted from drawing them, and only subjected her a few minutes longer to the admission of the fluid, which passed off without interruption from the pores of her skin and adjacent parts. The next time she came to me was the 24th of the same month: as she had been in the afternoon of the first day's experiment a good deal disordered, I changed the mode of conducting, and sat her in a common dining-chair, while I dropped, for five minutes, by the means of a large discharging rod with a glass handle, very strong sparks upon the *mastoideus* muscle, from its double origin at the *sternum* and *clavi-*

cula to its inſertion at the back of the head. She bore this better than before, and the ſame good effect followed in a greater degree, and without any of the ſubſequent inconveniences. I ſaw her the third time on the 27th: ſhe aſſured me ſhe had eſcaped her feveriſh ſymptoms on an evening, and that her ſpirits were raiſed by the proſpect of getting well; that, ſince the laſt time I electrified her, ſhe had more freedom in the motion of her head than ſhe had ever experienced ſince the firſt attack of her diſorder. I perſiſted in electrifying her after the ſame manner, March 3d, 5th, 6th, 7th, and 9th; from each time ſhe gained ſome advantage, and her feveriſh tendency and nervous irritability went off entirely.

The weather now ſetting in very unfavourable, and fearful of loſing the advantages we had happily reaped from our early efforts, I requeſted the favour of you, as her next-door neighbour, to electrify her every evening while ſhe was in town, and ſhe might, if any alteration took place, ſee me occaſionally. Fortunately for her, you accepted the propoſal, and to your judgement and caution in the conduct of it for the next fortnight (three evenings only excepted) you brought about the happy event; and have received her teſtimony of gratitude for relieving her from a condition under

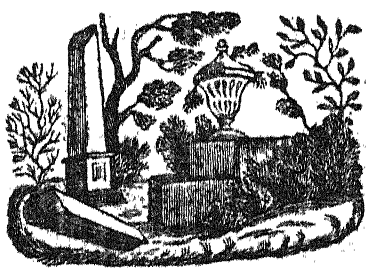
 which

which life could not be deſirable, to a comfortable aſſociation with her family and friends.

I am, &c.

THE method I purſued was, to place the lady upon a ſtool with glaſs legs, and to draw ſtrong ſparks, for at leaſt ten minutes, from the muſcles on both ſides of her neck. Beſides this, I generally gave her two ſhocks from a bottle containing 15 ſquare inches of coated ſurface fully charged, through her neck and one of her arms, croſſing the neck in different directions. This treatment ſhe ſubmitted to with a proper reſolution; and it gave me ſincere pleaſure to find it attended with the deſired ſucceſs.

W. HENLY.



VI. *An Account of a large Stone near* Cape Town. *In a Letter from Mr.* Anderſon *to Sir* John Pringle, *Bart. P. R. S.; with a Letter from Sir* William Hamilton, *K. B. F. R. S. to Sir* John Pringle, *on having ſeen pieces of the ſaid Stone.*

Cape of Good Hope,
Nov. 24, 1776.

SIR,

Read Jan. 15, 1777.

THE honour you did me laſt winter when in London, by approving of the notes I had taken concerning the poiſonous effects of ſome fiſhes which had been eaten by part of the Reſolution's crew in her laſt voyage, has made me take the liberty to write to you on another ſubject, which, though perhaps leſs intereſting, is yet curious enough to deſerve ſome attention.

What I mean is, a ſtone of an extraordinary ſize in this country, which Mr. MASSON, whoſe papers relative to this place were read before the Royal Society, may have mentioned; but it could not be in ſuch a manner as he wiſhed, as it was at his deſire that I went to ſee it: and though neither my time nor abilities were ſufficient to obſerve every particular worth notice, I hope my ſincere

cere intention of communicating any thing uſeful or curious will compenſate for theſe deficiencies.

The ſtone is ſo remarkable, that it is called by the people here the Tower of Babel, and by ſome the Pearl Diamond. It either takes the laſt name from a place near which it is ſituated, or it gives name to the tract of cultivated land called the Pearl. It lies upon the top of a ridge of low hills, beyond a large plain, at the diſtance of about thirty miles from the Cape Town, beyond which, at a little diſtance, is a range of hills of a much greater height. It is of an oblong ſhape, and lies North and South. The South end is higheſt; the Eaſt and Weſt ſides are ſteep and high; but the top is rounded, and ſlopes away gradually to the North end, ſo that you can aſcend it by that way, and enjoy a moſt extenſive proſpect of the whole country. I could not preciſely determine its circumference, but it took us above half an hour to walk round it; and by making every allowance for the rugged way, and ſtopping a little, I think the moſt moderate computation muſt make it exceed half a mile. The ſame difficulty occurred with reſpect to knowing its height; but I think that, at the South-end, it is nearly equal to half its length: or, were I to compare it to an object you are acquainted with, I ſhould ſay it equalled the dome of St. Paul's Church.

I am

I am uncertain whether it ought to be confidered as the top of the hill, or a detached ftone, becaufe there is no pofitive proof of either, unlefs we were to dig about its bafe; but it would certainly imprefs every beholder, at firft fight, with the idea of its being one ftone, not only from its figure, but becaufe it is really one folid uniform mafs from top to bottom, without any interruption; which is contrary to the general character of the high hills of this country, they being commonly divided, or compofed of different ftrata, at leaft if we may judge from the rows of plants or fhrubs which grow on the fides of the fteepeft, and, as I fuppofe, are produced from the fmall quantity of earth interpofed between them. It has indeed a few fiffures, or rather impreffions, which do not reach deeper than four or five feet; and near its North end a ftratum of a more compact ftone runs acrofs, which is not above twelve or fourteen inches thick, with its furface divided into little fquares, or oblongs, difpofed obliquely. This ftratum is perpendicular; but whether it cuts the other to its bafe, or is fuperficial, I cannot determine. Its furface is alfo fo fmooth, that it does not appear to have formerly been joined to, or feparated from, any other part by violence, as is the cafe with many other large fragments; but enjoys the exact fituation where it was originally placed,

and

and has undergone little change from being expofed for fo many fucceffive ages to the calcining power of a very hot climate.

I have fent a fpecimen of the rock and of the ftratum, which are both what the mineralogifts call *faxa conglutinata* or *aggregata*, and confequently are different from the more folid ftones which conftitute the greateft part of the mountains here; and is likewife another proof of its being a fingle ftone. But it ought to be obferved, that the piece of the rock was taken from a thin piece or fcale, which the weather may, perhaps, have had fome effect upon, fo as to change or deftroy the cement which keeps the pieces of the different ftones together, as it is very friable.

It would be needlefs to attempt to draw any conclufions from this fhort defcription; nor indeed am I certain if any ufeful reafoning could be made from it. I fhall, however, leave that to your better judgement, and can only fay, as an apology for troubling you, that it aftonifhed me to fee its prodigious fize; and that, as I had never feen or heard of any thing like it before, I thought it worth mentioning, efpecially as it had attracted the attention of one who, though he had travelled a great way in this extenfive country, had certainly

 not

not feen its equal, or he would not have wifhed to have this particularly examined.

Grofvenor-place,
July 2[illegible], 17[illegible].

SIR,

I RETURN you many thanks for the fight of the ftones from the Cape of Good Hope. I have not time to examine them very minutely; but they feem to be both of the fame nature, *granites*, the fmaller piece being only of a finer texture. The higheft points of the Alps are compofed of granite of the fame nature, and feem to have been lifted up by exhalations, volcanic explofions, or fome fuch caufes. This fingular immenfe fragment of granite moft probably has been raifed in the fame manner. Moft of the mountains which are called *primitive* (which I believe is only a term) are of this texture.

I am, &c.

W. HAMILTON.

VII. *A Letter from* Nathaniel Polhill, *Esq. Member of Parliament for the Borough of* Southwark, *to Mr.* John Belchier, *F. R. S. on Mr.* DEBRAW's *Improvements in the Culture of Bees*[a].

Southwark,
October 11, 1777.

DEAR SIR,

Read Jan. 22, 1778.

MR. DEBRAW's paper on bees, which you was fo obliging as to procure me, has afforded me much pleafure. As a farther difcovery of the nature and operations of thefe wonderful infects, it is a matter of great curiofity, and muft have been an high entertainment to the naturalift who has made them the object of his attention; but with me, the merit of his publication does not end here: his difcoveries, if properly purfued, may be of confiderable public utility; thofe who cultivate bees for profit will now be able to increafe the number of their ftocks at pleafure, by adopting his method of compelling the commons to produce a queen. That the working bees fhould be capable of forming a queen in the manner he defcribes, I own at firft ftaggered my belief; and although the experiments

(a) See Philofophical Tranfactions, vol. LXVII. p. 15.

appeared

appeared to be decisive, yet, as the whole depended on his veracity, I could not be satisfied without making him a visit. I found him modest, sensible, and communicative, and have had as much ocular proof as the season of the year would admit. In short, I am convinced of the fact, but not less at a loss to account for it.

The next discovery is the use of the drones: every one who has written on the subject has given some opinion concerning them, but all very unsatisfactory. Many have acknowledged their ignorance; and some have absolutely pronounced them useless, and recommended their being destroyed, to prevent an unnecessary waste of honey: even BUTLER, in his Feminine Monarchy, or History of Bees, which he calls a book written from experience, describes a drone-trap, which he recommends to be used for that purpose: and it is at this time so general an opinion amongst the bee-masters in this kingdom, that I am persuaded, nothing but the trouble and difficulty of catching them prevents many from ruining their stocks of bees by this means: I speak now from knowledge; some years since I lost a strong colony by giving the experiment a fair trial.

I can also confirm his account of the existence of drones no larger than the common bees, having by

accident diſcovered them laſt Midſummer in one of my boxes; DEBRAW ſays, the reaſon of this difference in the ſize of drones he fears muſt remain among the arcana of nature; but I will venture a conjecture from the general œconomy of thoſe creatures. The large drones conſume a great quantity of food, and, as ſoon as the breeding ſeaſon is over, are all deſtroyed by the working bees, evidently to avoid the expence of keeping them: and they do not appear again till the middle of April, when honey is plenty; though the breeding begins in March, or, if the ſpring be forward, the latter end of February: from hence I think it may fairly be concluded, that the ſmall drones are preſerved to impregnate the eggs in ſpring, in preference to the large ones, becauſe they devour leſs honey; and this is no inconſiderable object, few hives being ſo well provided as to have much to ſpare at that ſeaſon.

The only thing wanting to introduce Mr. DEBRAW's diſcoveries to general practice is, to contrive a method of making his experiments ſo eaſy, and with ſo little danger from the ſtings of the bees, as may recommend it to little farmers and cottagers in the country. This I ſhall endeavour to do next ſummer, and am not without hopes of ſucceeding: this once effected, every poor man,

who has room enough in his garden to place twenty hives, may reasonably expect a profit of at least ten pounds a year, with very little trouble and without any expence.

I am, &c.

VIII. *An improved Method of tanning Leather. By* David Macbride, *M. D. communicated by Sir* John Pringle, *Bart. P. R. S.*

Dublin,
May 30, 1777.

SIR,

Read Jan. 22, 1778.

AGREEABLY to the promiſe which I made you ſome years ago, I now ſend you my ſecret method for the more expeditious tanning of leather. If you think the letter, and paper which it incloſes, worthy of the Society's attention, you will pleaſe to preſent them. I have already delivered in one of theſe ſets of inſtructions to our Dublin Society (who have been acquainted with the whole progreſs of this affair ſince the beginning) and have ſent two others to the Societies which are eſtabliſhed in London and Edinburgh, for the purpoſe of encouraging trade and manufactures; as judging it will be more in their way than in the Royal Society's to extend the utility of this invention: for I apprehend it will require ſome encouragement from them,

them, in the way of premium, to ſet the buſineſs a-going, ſo ſtrong are the prejudices amongſt tradeſmen of all ſorts againſt trying new practices, and ſuch the reluctance with which they quit their ancient ways of working.

I am, &c.

SIR, Dublin, May 31, 1777.

YOU may pleaſe to remember that I informed you, ſome years ago, of my having found out a way of tanning leather in leſs time, and at a ſmaller expence of materials, than can be done by any of the ways hitherto known or practiſed; and promiſed that, as ſoon as I ſhould find myſelf at liberty to diſcloſe it, I would communicate my method to the Royal Society.

Accordingly I take the liberty of incloſing a ſet of inſtructions, which I drew up for the perſon who conducted the buſineſs of a large tan-yard belonging to a company with which I have had an engagement for theſe laſt four years; which I apprehend will be found ſuffi-

fufficiently clear for enabling any intelligent tanner to avail himfelf of my improvements.

I beg you will prefent this paper to the Society; but, as it cannot be underftood by gentlemen who are not already, in fome degree, acquainted with the ordinary procefs of tanning, I muft requeft their indulgence while I mention the principal operations in this branch of manufacture.

The ufe of tanning is two-fold; firft, to preferve the leather from rotting; and, fecondly, to render it impervious to water.

An infufion of any ftrongly-aftringent vegetable will ferve to tan leather, fo far as to prevent its rotting; but if this vegetable does not contain a good deal of gum-refin, it will not anfwer for enabling it to keep out water: and hence it is that oak-bark, which is more abundant in the gummy-refinous part than any of our common indigenous aftringents, is preferred to all other fubftances for the purpofe of tanning.

The tanners prepare their bark by gently drying it on a kiln, and grinding it into a very coarfe powder. They then either ufe it in the way of infufion, which is called ooze; or they ftrew the dry powder between the layers

 of

of hides and ſkins, when theſe are laid away in the tan-pits.

The ooze is made by macerating the bark in common water, in a particular ſet of holes or pits, which, to diſtinguiſh them from the other holes in the tan-yard, are termed letches.

The firſt operation of the tanner is to cleanſe his hides from all extraneous filth, and remove any remains of fleſh or fat which may have been left behind by the butcher.

The hair is next to be taken off, and this is accompliſhed either by ſteeping the hides for a ſhort time in a mixture of lime and water, which is termed liming; or by rolling them up cloſe, and piling them in heaps, where they quickly begin to heat and putrify. The hair being looſened is ſcraped off, and the tanner proceeds to the operation called fleſhing, which conſiſts in a further ſcraping, with a particular kind of knife contrived for the purpoſe, and cutting away the jagged extremities and offal parts, ſuch as the ears and noſtrils.

The raw leather is then put into an alcaline ley, in order to diſcharge the oil, and render its pores more capable of imbibing the ooze. The tanners of this country generally make their ley of pigeon's dung; but

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a more active one may be prepared from kelp or pot-aſh; taking care, however, not to make it too ſtrong of the aſhes, nor to allow the leather to remain too long in the ley.

The oil being ſufficiently diſcharged, the leather is ready for the ooze, and at firſt is thrown into ſmaller holes, which are termed handlers; becauſe the hides or ſkins, during this part of the proceſs, are taken up, from time to time, and allowed to drain; they continue to work the leather in theſe handlers, every now and then ſtirring it up with the utenſil called a plunger, which is nothing more than a pole with a knob at the end of it, until they think proper to lay it away in the vatts. In theſe holes, which are the largeſt in the tan-yard, the leather is ſpread out ſmooth, whereas they toſs it into the handlers at random, and between each layer of leather they ſprinkle on ſome powdered bark, until the pit is filled by the leather and bark thus laid in *ſtratum ſuper ſtratum:* ooze is then poured on, to fill up interſtices; and the whole crowned with a ſprinkling of bark, which the tanners call a heading.

In this manner the leather is allowed to macerate, until the tanner ſees that it is completely penetrated by the ooze: when this is accompliſhed (which he knows by

cutting out a bit of the thickeſt part of the hide) the manufacture is finiſhed, ſo far as relates to tanning, ſince nothing now remains but to dry the goods thoroughly, by hanging them up in airy lofts built for the purpoſe. Such in general is the proceſs for tanning calf-ſkins, and thoſe lighter ſorts of hides which are called butts; but the large, thick, heavy hides, of which the ſtrongeſt and moſt durable kind of ſoal-leather is made, require to have their pores more thoroughly opened before the ooze can ſufficiently penetrate them. For this purpoſe, while the hides are in the putreſcent ſtate, from being allowed to heat in the manner already mentioned, and well ſoaked in an alcaline ley, they are thrown into a ſour liquor, generally brewed from rye, in order that the effervеſcence which neceſſarily enſues may open the pores.

The tanners term this operation raiſing, as the leather is conſiderably ſwelled, in conſequence of the conflict between the acid and alcali. This is an Engliſh invention; for it appears from M. DE LA LANDE, who was employed by the Royal Academy of Sciences to write on the art of tanning, that the foreign tanners know nothing of this branch of the buſineſs: indeed, their whole proceſs, according to his account, is ſlovenly,

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and even more tedious than our common method, and muſt make but very indifferent leather.

When the raiſing is accompliſhed, the leather is put into the handlers, and worked in them for the requiſite time; then laid away in the vatts, and there left to macerate until the tanning is found to be completely finiſhed, which, for the heavieſt kind of leather, ſuch as this of which I am now ſpeaking, requires from firſt to laſt full two years. At leaſt, the tanners of this country cannot make ſoal-leather in leſs time; what they are able to perform in England, I am not ſo thoroughly acquainted with.

It is this tediouſneſs of the proceſs which enhances the value of leather; and the returns being ſo ſlow, the trade of tanning never can be carried on to advantage, but by perſons poſſeſſed of a large capital; therefore, one ſure way of increaſing the number of tanners, and of courſe of bringing down the price of their manufacture, is to ſhorten the proceſs; and if at the ſame time we can improve the quality of the leather, and ſave ſomewhat in the expence of tanning materials, the public will be eſſentially benefited in reſpect to one of the neceſſary articles of life.

All this, I will venture to ſay, can be done by purſuing the method which is laid down in the incloſed paper, and which may be introduced into any common tan-yard.

With reſpect to time it is poſſible, in the way that I have found out, to finiſh leather in a fourth part of what is required in the ordinary proceſs; for I have repeatedly had calf-ſkins tanned in a fortnight or four weeks, which in the common way could not be done in leſs than from two to four months.

I ſhall not pretend, however, to affirm, that that buſineſs can be carried on in the large way with ſuch expedition; becauſe a great deal of this abridgement of time was probably owing to frequent handling and working of the leather; but I am confident, and know it from four years experience, that in the ordinary courſe of buſineſs, and in a common tan-yard, the tanner may ſave at leaſt four months out of twelve, produce better leather, and find his bark go much farther than in the old way of tanning.

Having premiſed thus much, I flatter myſelf that the paper of inſtructions will be found perfectly intelligible. It ſhews, that the principles on which my method is eſtabliſhed are derived from chemiſtry, and therefore it will

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not appear ſtrange, that theſe improvements ſhould have been made by a perſon of the medical profeſſion: indeed, they took their riſe from a ſeries of experiments carried on purely for medical purpoſes (the very ſame that confirmed me in the opinion that infuſion of malt would cure the ſea ſcurvy) and any perſon who will look into the account of thoſe experiments, will readily underſtand the theory of the new method of tanning[(a)].

It would be treſpaſſing on the time of the Society, to enter into any detail of the circumſtances that firſt induced me to think of this matter, or to give a hiſtory of the progreſs of my experiments, which at firſt were made at home, and with little pieces of raw leather: it is ſufficient to ſay, that the efficacy of this method has been fully proved by the experience of near ten years (during which I have thought proper to keep it ſecret) and I now beſtow it to the public.

I am, &c.

(a) See the Eſſay on the diſſolvent Power of Quickſilver, among the Experimental Eſſays on Medical and Philoſophical Subjects.

Instructions to Tanners, for carrying on the new method of tanning, invented by Dr. MACBRIDE *of* Dublin; *whereby the leather is not only improved in its quality, but tanned in much less time, and with a smaller quantity of bark, than in any other method hitherto known or practised.*

AS the new method of tanning depends on this principle, " That lime-water extracts the virtues of oak-bark " more completely than plain water;" the first thing in which the tanner is to be instructed, is the making of lime-water.

I. Provide a large vessel, in the nature of a cistern, whose depth shall be at least twice its diameter, and of a capacity adapted to the extent of the tan-yard.

II. This cistern must be fixed in a convenient corner of the yard, under a shed, and should stand so as that the liquor which is to be drawn off from it may run freely into the letches.

III. There must be a cock fixed in the side of the cistern, about a foot from the bottom, to let off the contents; and there must be a hole in the bottom of it, of five

five or ſix inches diameter, which is to be ſtopped with a plug. Let this hole open over a gutter.

IV. The ciſtern muſt be covered with a flooring of boards, ſtrong enough to bear a man's weight; and from ſide to ſide of this lid there muſt be an opening of two or three feet wide.

V. If it can be ſo contrived that a water-pipe may be led into the ciſtern, it will ſave the ſervants a good deal of trouble; but if this cannot be done, a pump muſt be fixed in the moſt convenient way, for the purpoſe of filling it from time to time.

VI. The ciſtern being once fixed (which is all the additional apparatus that the new method of tanning requires) the making of lime-water will be found extremely ſimple and eaſy.

VII. You are firſt to fill the ciſtern with water, and then, for every hogſhead that it may contain, throw in ten or a dozen pounds weight of unſlaked lime.

VIII. Mix the lime thoroughly with the whole body of the water, by ſtirring it exceedingly well from the bottom, with a bucket and plunger, until you perceive that the lime is completely diffuſed, and the whole mixture grows as white as milk; leave it then to ſettle for a couple of days, that the undiſſolved part of the lime may

 entirely

entirely fubfide, and the water become perfectly limpid, and clear as rock-water. Your lime-water will then be fit for immediate ufe.

IX. The cock, as already mentioned, is to be fixed at leaft twelve inches from the bottom of the ciftern, in order that only the limpid part of the lime-water may run off; and the ufe of the hole in the bottom, which is ordered to be ftopped with a plug, is to let off the grofs and infoluble remains of the lime, as often as it may be found neceffary to clean out the ciftern.

X. When the firft brewing (as it may be termed) of lime-water is all expended, you are to fill up the ciftern with water a fecond time; ftir up the lime from the bottom with the bucket and plunger, fo as to mix it thoroughly with the whole body of the water, as before directed, and then leave it to fubfide for the requifite time. Thus you will have a fecond brewing of lime-water; and you may go on in the fame manner to make a third, fourth, fifth, or perhaps a fixth, or more brewings, from the original quantity of lime; provided you fhall find the lime-water continue fufficiently ftrong.

XI. There are two ways of knowing when lime-water is fufficiently ftrong. The one is by the tafte, and this a little practice will teach you to diftinguifh; the other is,

by obſerving a certain ſolid ſcum, like the flakes of very thin ice, which collects and forms itſelf on the ſurface of the lime-water.—As long as you find this ſolid ſcum floating on the top of the water in the ciſtern, ſo long you may conclude that there is no neceſſity for throwing in freſh lime.

XII. But when the ſcum ceaſes to appear, or you find from the taſte that the lime-water is not ſo ſtrong as it ought to be, you muſt then take out the plug from the bottom of the ciſtern, and clear it by ſweeping away the groſs remains of lime: and after you have cleaned the ciſtern, begin your brewings of lime-water a-new, and proceed in the manner already directed, as to ſtirring up the lime, and leaving it to ſettle for the neceſſary time, ſo as to have your lime-water perfectly limpid. In this manner you may go on from year to year, and conſtantly keep yourſelf in ſtock with reſpect to lime-water.

XIII. It is this lime-water which is now to be uſed in making your ooze inſtead of the plain common water; and this is all the difference between the old and the new method of tanning; for when your ooze is prepared, by ſteeping your bark in lime-water (in the letches, as you do at preſent, only running it through two letches) you are to make uſe of it in the very ſame

way that you have hitherto uſed the common ooze, there not being the leaſt variation required with reſpect to any of the previous management before the ſkins or hides are fitted for the ooze. Every thing that relates to cleaning, liming, fleſhing, &c. is to be conducted precisely as in the old or common method of tanning; and the goods are to be worked in the handlers for the requiſite time, and then laid away in the vatts, with layers and heading of bark, juſt as you now practiſe; and when you obſerve that the leather is ſufficiently penetrated with the ooze, that is to ſay, completely tanned, you will take it up, dry it, and afterwards dreſs it according to the different uſes for which it is intended. You are always to obſerve, however, that the ooze is to be turned from one letch on another before it is uſed, otherwiſe it will be apt to blacken the leather.

XIV. What has been hitherto ſaid relates only to butts and calf-ſkins: as to ſoal-leather, which is prepared for the ooze by ſteeping it in ſome ſour liquor, in order to open its pores, and raiſe it (according to the tanner's phraſe) the new method requires a different practice from the old one.

XV. In the old method, the tanners made uſe of ſourings brewed generally from rye, or ſome other grain;

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but theſe liquors are not only troubleſome to brew and to ferment, but they are always uncertain as to their degree of ſourneſs or ſtrength, which depends on the ſtate of the weather, and other variable circumſtances; theſe liquors are moreover exceedingly apt to rot the leather, and, without great care, may injure it very materially in its texture.

XVI. To obviate theſe inconveniences, you are to imitate the bleachers of linen, who make uſe of a ſour prepared by diluting the ſtrong ſpirit of vitriol (vulgarly, but improperly, termed oil of vitriol) with a ſufficient quantity of plain water.

XVII. It was not without much difficulty that the bleachers could be prevailed on to quit their old ſourings, made either like yours of rye or barley, or of ſour butter-milk, from a groundleſs fear, that the vitriolic ſouring would corrode their cloth; but the experience of many years has convinced them of their error, and now no other ſouring is uſed. In like manner the tanners at firſt may ſome of them be afraid to uſe the vitriol, but a little practice will ſhew how far ſuperior this ſouring is to what they have hitherto uſed. They will never find it ſubject to any change in reſpect to ſtrength from variations of weather, or different degrees of heat; and

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ſo far from tending to rot the leather, it gives unuſual firmneſs; and the ſoals which are raiſed by the vitriolic ſouring are remarkably ſound, and always free from the ſlighteſt degree of rottenneſs. Beſides, the ſame ſour may do for many parcels of leather, by adding a little vitriol to it; and it need only be thrown away, when it becomes too dirty for uſe, by the frequent ſucceſſion of hides.

XVIII. A wine pint of the ſtrong ſpirit of vitriol, which will not coſt more than nine or ten pence[(a)], is ſufficient for fifty gallons of water to prepare the ſouring at firſt: therefore all you have to do, in raiſing the ſoals, is only to prepare them before-hand in the uſual way; and, when they are fitted for the ſouring, mix up a quantity of vitriol and water, according to the number of hides that you require to have raiſed, ſtill obſerving the proportion of a pint to fifty gallons, which will be enough, if the vitriol be of the due degree of ſtrength. The hides may lie in the ſouring till you find them ſufficiently raiſed, for they will be in no danger of rotting, as they would be in the common corn ſourings, which in time might turn putrid, and rot the leather; whereas, the vitriolic ſouring keeps off putrefaction.

(a) The oil of vitriol is ſold by the druggiſts in large bottles, containing eight or ten gallons.

XIX. When

xix. When you find your hides ſufficiently raiſed, put them directly into the ooze, and go on with the tanning as in the old way; and you will ſee that the lime-water ooze penetrates raiſed leather even faſter than it does butts or calf-ſkins, allowance being made for their different degrees of thickneſs.

xx. Let it be now ſuppoſed that you have your ciſtern fixed, your lime-water prepared, and ſome letches full of lime-water ooze, which has been run through two letches in order that the lime-water may completely ſpend its force on the bark; you are not to throw away what common ooze you have in ſtock in the yard, but only as it ſhall be ſpent; then, indeed, you are to throw it away, and ſupply its place with the lime-water ooze.

xxi. In a very few days you will perceive the difference between the activity of the two oozes, the new and old, with reſpect to penetrating the leather; and thus, without any kind of loſs or waſte, you will get rid of all your old liquors, and come ſpeedily into a full ſtock of the ooze made with lime-water; and after you have got the new method eſtabliſhed, your buſineſs will go in a regular courſe, and one parcel of goods will ſucceed another, as faſt as you can manufacture and diſpoſe of them.

xxii. Though it is poſſible to tan ſmall parcels of leather, by way of experiment, by the uſe of lime-water

ooze,

ooze, in a fourth part of the time which is required, if only common ooze be made ufe of; yet the bufinefs of a large tan-yard cannot be carried on with fo much expedition: but even in large works, and in the common courfe of bufinefs, foal leather can be completely tanned and finifhed, in, from eleven to fifteen months, according to the different weight and thicknefs of the hides. Butts in, from eight to twelve months, and calf-fkins in, from fix to twelve weeks; in general, the tanner may fave at leaft a third of the time that has hitherto been required.

XXIII. The leather, which is manufactured in the new way, is of a fuperior quality to that of the old tannage, efpecially the foal-leather, which wears remarkably well, and never fhews the leaft fign of rottennefs.

XXIV. Let it always be remembered, that the lime-water is never to be ufed but when it is fufficiently ftrong, and as clear as rock-water.

XXV. Whenever you make frefh ooze, you muft always ufe frefh lime-water, and run the ooze through two letches; and the lime-water ooze, when fpent, from lying on the leather, is never to be returned back upon the bark which is in the letches (as you now return your fpent ooze) but muft always be thrown away, as being

entirely

entirely uſeleſs; for which purpoſe you muſt contrive a gutter in the tan-yard to carry off the ſpent ooze.

XXVI. The letches ought to be under cover, leſt the rain get into them and weaken the ooze, and if the handlers are ſheltered, it will be ſo much the better; but it is of no importance to cover the vatts, provided, when the leather is laid away in them, they are kept conſtantly full to the brim.

XXVII. You muſt always take care to have a ſufficient ſtock of unſlaked lime by you (for if it be ſlaked, it will not anſwer to make lime-water): therefore, get your lime freſh, if poſſible, from the kiln, and immediately pack it in any kind of old dry caſks. Weigh one of theſe caſks, and it will enable you to aſcertain the quantity of lime neceſſary to be thrown into the ciſtern each time you begin a freſh brewing of your lime-water, and thus ſave you the trouble of repeated weighings; not that there need be much nicety about the quantity of lime, a ſcore of pounds over or under making no ſenſible difference in the ſtrength of the lime-water.

XXVIII. Any expence you may be at in procuring lime, which even in the largeſt tan-yards can amount but to a trifle, will be amply compenſated by the ſaving of bark; becauſe, that lime-water ſo completely exhauſts

the bark, and makes it go ſo much farther than when the ooze is made only of plain water. As a proof of this, you may make a pretty ſtrong ooze from the tan or ſpent bark, which you now conſider as completely exhauſted, by infuſing in it lime-water.

Tanners, as they become acquainted with the new method, will find it perfectly eaſy, and may no doubt make further improvements by experience. The foregoing directions were found ſufficiently full for enabling a gentleman at Belfaſt to carry on the buſineſs in an extenſive way for theſe four years paſt; and it is preſumed they will prove equally clear and intelligible to all other perſons in the tanning trade.

Dublin, May the 1ſt, 1777.

IX. *Obſervations on the Population and Diſeaſes of* Cheſter, *in the Year* 1774. *By* J. Haygarth, *M. D.*

Read Jan. 22, 1777.

THE facts aſcertained in the following tables prove Cheſter to be healthy in ſuch an uncommon degree, as will aſtoniſh thoſe who are beſt acquainted with the general ſtate of mortality in large towns. In order to deduce ſatisfactory and uſeful concluſions from theſe facts, it ſeems neceſſary to deſcribe a few peculiarities in the ſituation of this city, which probably contribute to produce a ſalutary effect. The intelligent reader will remark, in the following account, that the ſtructure of Cheſter prevents, in an uncommon degree, two principal ſources of diſeaſe, ſtagnant moiſture and putrefaction.

Cheſter is placed on a red, ſandy, mouldering rock *(ſaxum arenarium friabile rubrum)* which forms a riſing promontory, whoſe ſummit is elevated exactly one hundred feet above high water mark, and forty feet above the adjacent country; from this point the ſtreets deſcend with a gentle declivity every way to the edge of the rock,

 whence

whence there is a perpendicular fall of many yards from every part of the town.

The looſe rock on which the town is built abſorbs moiſture: for being cut into filtering ſtones, water ſoon paſſes through its pores. The principal ſtreets that meet in the centre of the city, are deeply excavated out of the rock, being ſunk ſix or nine feet lower than the ſurface of the ground. By this ſtructure the foundations of the houſes are kept perfectly dry, as the ſtreets quickly drain off the water, and the rock abſorbs all the remaining moiſture. For theſe reaſons the cellars in general are dry, a circumſtance that greatly contributes to health. Stagnant water in a cellar is probably very often the unſuſpected cauſe of putrid diſeaſes: its pernicious influence ſeems to reſemble, in ſome degree, that of bilge-water in a ſhip.

There is a form of building peculiar to Cheſter, called the Rows, which are covered galleries that make a complete communication between moſt of the principal ſtreets. The Rows are always dry and clean even in wet and dirty weather; they moderate the heat of ſummer, and the coldneſs of winter. Theſe uncommon advantages oftener tempt abroad perſons of a delicate and valetudinary conſtitution, whether they be engaged in buſineſs or amuſement; by which they obtain

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the benefit of freſh air and exerciſe, without incurring danger from catching cold.

The walls are near two miles in circumference, and ſurround the central part of the city: they are dry and clean immediately after the heavieſt rains. The Rows form a dry communication with the walls from nearly every place within their circuit: their frequent aſcents and deſcents; their elevated, airy ſituation, and varied proſpects; all contribute to render walking upon them peculiarly well adapted to preſerve or reſtore health.

The Dee, a large navigable river, divides a ſmall part of the town from the reſt, ſkirts the leſs, and ſurrounds three quarters of the larger portion. Where it makes this diviſion, it falls over a cauſeway, forming a widely extended caſcade, and then runs with rapidity down looſe rocks; the whole deſcent is thirteen feet. The tide always flows up to the town, where it riſes, on a medium of ſpring tides fifteen feet, the higheſt tides twenty-one feet: every new and full moon, about ſix or eight tides flow over the cauſeway, and ſometimes more than twenty miles above the town. Beſides waſhing away the liquid filth, which quickly runs into the river by a ſhort courſe from nearly all quarters of the town, the agitation of the waters both by the caſcade and tides is probably of farther ſervice in purifying the atmoſphere.

The

The air of Chefter is uncommonly clear. In a regifter of the weather, kept for the laft four years, there were only fix foggy and thirty-two hazy mornings. In general, the atmofphere on the weftern is much clearer than on the eaftern fhore of Britain, though more rain falls on the weft than on the eaft fide of the ifland.

The modern refinement of manners, under the opprobrious name of luxury, is generally thought to be peculiarly deftructive to health; but the true friend of mankind will candidly, and without prejudice, inquire into the facts which only can determine his judgement on this important fubject. A large portion of the inhabitants of Chefter enjoy, with temperance, the elegant refinements of life; yet no diforder, except the gout, can be peculiarly afcribed to this caufe, in the tables of difeafes for the laft three years, that is, no more than two deaths out of 1277. But it is to be obferved, that refined manners here, as they generally do every where, entirely banifh the deftructive exceffes of gluttony and drunkennefs. It is the large quantity, rather than the rich quality or exquifite flavour, of what we eat and drink, that is injurious to health.

As the healthinefs of Chefter muft appear fo very extraordinary as to be almoft incredible, it is neceffary to declare, that the enumeration was made with all poffible

care

care and fidelity; and that the errors are, for obvious reasons, rather on the side of defect than excess, so as to make the proportional mortality rather appear greater, not less, than the truth. Two facts may be mentioned in proof of this position. In 1772 the inhabitants of St. Michael's parish were reckoned to be 618, and there cannot be the smallest suspicion that there are fewer than this number; and yet by the survey, Table V. they are only 575. Again, the deaths, by fever, appear to be 35 from the parish-registers, and only 28 from the survey. I know no error on the side of excess, except in Trinity parish, where a new street has been inhabited only six years. This circumstance will clearly occasion this district to appear in Table VII. more healthy than the truth.

In estimating the health of the district which belongs to the Cathedral, because it appeared so very extraordinary, particular care was taken to ascertain the exact truth. As some of these houses have not been built the whole period included in this calculation, more than a proportionable allowance of death is made for this deficiency.

In order to exhibit a just and most striking view of the health of Chester, especially the center, beyond other places both of town and country, the reader is particularly requested to compare the seventh with the eighth table.

table. The ſeventh table is compoſed by finding the proportion of inhabitants in the different pariſhes ſeparately and together in the fifth, to the burials for ten years recorded in the ſixth table. In the ſeventh table the mortality of whites in Jamaica is taken from Dr. LIND; of Liverpool and Mancheſter from Drs. ENFIELD and PERCIVAL, who directed particular ſurveys of thoſe towns; of the other places, both town and country, from Dr. PRICE: ſo that no facts can be aſcertained on more reſpectable authorities. That the inhabitants of Cheſter ſhould have near an equal chance of living to twice the age of the inhabitants of Vienna, London, or Edinburgh; and that no large town, as far as inquiries have been hitherto made, ſhould approach to a nearer proportion of longevity than as 28 to 40, are aſtoniſhing facts. The center is by far the moſt ſalubrious part of the city; the average of deaths within the walls is only 1 in 58, a degree of longevity much ſuperior to what in general is recorded even of the country. The pariſhes which include the ſuburbs, *viz.* St. Oſwald's, John's, Mary's, and Trinity, are of the largeſt ſize, and comprehend many central parts of the town which are undoubtedly as healthy as any of the reſt, but they alſo contain all the out-ſkirts, which conſequently muſt be much more unhealthy than appears to be the general average of deaths

in those parishes. It has been suspected, as a reason why the central parishes appear most healthy, that more who die in the city may be buried in the suburbs than the contrary; and, as a reason why the whole town appears so healthy, that more persons who die in it may be buried in the country than the contrary; but on strict inquiry I can find no foundation for either supposition. The extent of the survey both in each parish, and in the town in general, corresponds with much exactness with the extent of the register. However it must be confessed, that there is one circumstance which makes the center appear more healthy than the suburbs, though it rather tends to prove the reverse. The central parishes have a smaller proportion of inhabitants in the weakest period of life, or under the age of 15. The number under 15 in these parishes is 888; whereas, had it borne the same proportion to the whole number that it bears in the town in general, it would have been 1067. But this consideration could only reduce the average of deaths from 1 in 58 to 1 in 55; so that, making all due allowance of this account, the center is still proved to be remarkably the most healthy.

There is one probable cause that renders the suburbs more unhealthy than the rest of the town. A part of the putrid filth, which flows from the center to the circum-

 ference,

ference, ſtagnates in the ditches of the ſuburbs, *viz.* the Headlands, Barker's-lane, Horn-lane, and Greg's-pit, in John's pariſh; Fluckerſbrook and Cow-lane, in Oſwald's pariſh; Nun's-lane and garden, Skinner's-lane and Stye-lane, in Mary's pariſh; the Sluices and the Rood Eye, in Trinity pariſh. There is not one inſtance of ſtagnant water within the city walls, except in Nun's-lane and garden. As there is a ſufficient declivity from all theſe ditches into the river, it would be a very eaſy and moſt ſalutary improvement to drain them perfectly, and ſeems highly to deſerve the attention of our magiſtrates. The ancients were particularly attentive to ſuch regulations, as appears from a letter of the younger Pliny to Trajan. *Amaſtrianorum civitas, domine, et elegans et ornata, habet inter præcipua opera pulcherrimam, eandemque longiſſimam plateam: cujus à latere per ſpatium omne porrigitur nomine quidem flumen, re vero cloaca fœdiſſima: quæ ſicut turpis, et immundiſſima aſpectu, ita peſtilens eſt odore teterrimo. Quibus ex cauſis, non minus ſalubritatis quam decoris intereſt eam contegi.*

By the induction of numerous facts, two principal ſources of continued fevers have been diſcovered, that is, the contagion of human effluvia, and of marſh *miaſmata:* the latter is diſtinguiſhed by frequently aſſuming an intermittent type, or changing into a dyſentery. It may be doubted, to which kind of peſtilence

lence the putrid ditches of towns belong. From this filth being chiefly of animal origin, and from the absence of both intermittents and dysenteries, even in their neighbourhood at Chester, I should conclude that they produce the same kind of fevers as human contagion: and yet I doubt, whether the agues and dysenteries of Edinburgh can probably be attributed to any other cause, unless the frequent fogs of that place, or *miasmata* from the north loch, and from the moist foundation of the houses in the Cowgate, &c. where there was formerly a loch, may be supposed to produce such an effect.

Another reason of greater mortality in the suburbs seems to be, that their inhabitants in general are of the lowest rank: they want most of the conveniences and comforts of life: their houses are small, close, crouded, and dirty: their diet affords very bad nourishment, and their cloaths are seldom changed or washed. These parts of the town are supplied less plentifully than the rest with water. The air they breathe at home is thus rendered noxious by respiration and putrefaction. These miserable wretches, even when they go abroad, carry a poisonous atmosphere round their bodies that is distinguished by a noisome and offensive smell, which is peculiarly disgustful even to the healthy and vigorous, exciting sickness and a sense of general debility. It cannot,

 therefore,

therefore, be wonderful that difeafes fhould be produced where fuch poifon is infpired with every breath. This noxious air is the moft frequent caufe of malignant fevers. In thefe poor habitations, when one perfon is feized with a fever, others of the family are generally affected with the fame fever in a greater or lefs degree. This dreadful confequence is naturally to be expected, as putrid fteams arifing from the difeafed body are added to the other increafing caufes that produce noxious air.

If a regulation could be univerfally adopted of immediately removing out of the family fuch of the poor people as are feized with fevers, it is evident that the moft falutary confequences would follow. Reafonable objections might be made to receiving fuch patients into the general infirmary, even into feparate wards, left the infection fhould fpread through the whole houfe, which in a former paper on this fubject was proved to be healthy to an uncommon degree when compared with other hofpitals. But might not this and every other objection be obviated by erecting, on the ground which adjoins and belongs to the infirmary, a building, to be divided into fpacious, airy, feparate apartments, where patients infected with fevers, and properly recommended, might be received on any day of the week? Befides medical affiftance, they would here enjoy clean linen,

linen, airy rooms, careful attendance, and wholefome diet.

Towards the latter end of Auguft there appeared a fever which from its frequency might be called epidemical. It was preceded by fultry weather, and commenced immediately after a ftrong gale of wind from the weft on the 17th, fucceeded by eight fair days. It has been remarked by Dr. STEDMAN, that ftorms prevent epidemics. I would not alledge this as an inftance to refute the ingenious obfervation: but the want of rain during, and eight days after, the high winds, was perhaps the caufe why they produced a pernicious rather than a falutary effect. The admirable difcovery of Dr. PRIESTLEY, that water corrects and purifies air rendered noxious by refpiration and putrefaction, makes this conjecture extremely probable.

From the fifth table it appears, that this fever attacked 285, and was fatal to 28, that is, to 1 in 10. It had the common fymptoms of malignant fevers produced by human effluvia, and particularly affected the head with pain, giddinefs, and delirium. This fever attacked in general the loweft, few of the middle, and none (or only one) of the higheft rank. Among the poor, when one was feized, the reft of the family fuffered more or lefs with like fymptoms; but in no inftance did any

marks

marks of infection appear, even in the nurſes, where the patients enjoyed the comforts of clean linen and airy rooms.

Antimonials of various kinds were given at different periods of the fever. They rendered the pulſe leſs frequent in ſome inſtances, if reckoned immediately after the operation, but in very few caſes produced any laſting abatement of ſymptoms. After a full effect of the antimonials, the Peruvian bark was given in a conſiderable quantity; but it neither abated nor aggravated the fever. The remedy of moſt manifeſt ſervice in this epidemic was topical evacuations from the head by leeches and bliſters.

From the ſecond table it appears, that there were four fatal inſtances of the puerperal fever in 1774; a diſeaſe which frequently occurred this year, though I had never before ſeen it in Cheſter, during ſeven years practice.

In making the general ſurvey of the town, particular inquiries were made concerning the proportional fatality of the natural ſmall-pox, in order to demonſtrate the advantages of inoculation, and to diſcover at what age this operation ſhould be performed that it may become the moſt extenſively beneficial to ſociety. The proportion of deaths by the natural ſmall-pox to all the deaths this year is 1 to 2 and 7-10ths. From the fifth table it is evi-

dent that 1060 have never had the small pox out of 14713 inhabitants, that is 1 in 14.

The facts recorded in Table IV. seem to determine the age when children should be inoculated in order to secure the greatest possible benefit to mankind. It appears here, that under one month old not one died of the small-pox; that, under six months old only 7 out of 202; and yet that above a quarter of the whole died under one year old. My ingenious friend Dr. PERCIVAL first discovered at Manchester the fatality of the small-pox in early infancy, which induced him with much candour and good sense to correct a former opinion on this subject founded on the greater safety of inoculation in children a few years old. Indeed, where children can be secured from all danger of the natural infection, the greater hazard to young infants from inoculation will be a sufficient reason to defer the operation for three or four years. The small-pox was fatal to 22 males and 29 females under one year old, that is, to seven more females. This fact confirms what Dr. PERCIVAL observed at Manchester. The epidemic small-pox began near the summer, and almost ended at the winter solstice, only 19 remaining ill of the disease in January 1775, when the general survey was taken.

Dr.

Dr. PRICE, in his excellent obſervations on annuities, has adduced numerous facts to prove that women live longer than men. Theſe tables afford many confirmations of the remark. There died this year, under 20 years old 162 males and 149 females, that is, a majority of 13 males; 52 huſbands and 50 wives, that is two more huſbands; 28 widowers and 48 widows, which is only a majority of 20 widows; though by the general ſurvey, Table V. there are in Cheſter 258 widowers and 736 widows, or near three times the number. The total of males is 6697, of females 8016, hence there is 1319 or nearly a fifth majority of females: it may not be improper alſo to obſerve, that the women, eſpecially in the higher and middle ranks of ſociety, are remarkably beautiful. Theſe facts clearly prove, that the manners and ſituation of Cheſter are peculiarly favourable to the female conſtitution.

Other obſervations may be deduced from theſe tables, which confirm, correct, or illuſtrate, various queſtions of importance to ſociety. The number of married perſons in Cheſter is 4881, of unmarried 9832, that is, nearly one-third is married, which is a common proportion. Upwards of one-half of the inhabitants above 15 years old are or have been married, the proportion being as 4 to 7. Though Cheſter is ſo uncommonly healthy, yet this, like

like most other great towns, is unfavourable to population. Thus it appears, from the general bill for ten years, that, on an average, one marriage produces less than three children. One cause of this small proportion is probably the want of manufactures, which might enable the lowest class of people to marry in earlier youth: taking the whole town, the number of persons in each family is 4 and 1-3d. The inhabitants under 15 years old are 4486, that is, more than a third. The proportion of deaths this year to the number of inhabitants is nearly at 1 to 27: this difference from the common degree of health is occasioned by the unusual fatality of the small-pox. Table III. shews that the greater mortality of the summer than the winter quarter of 1774 was occasioned by the epidemic small-pox, which began in July: yet still that winter and autumn taken together were more fatal than the spring and summer in the proportion of 326 to 220, that is, near one-sixth more died in the former than in the latter portion of time.

There is a general prejudice in Chester, that it is unhealthy to inhabit the Rows; a prejudice most clearly refuted by many of the preceding observations. The Rows run along the central streets, which include incomparably the most healthy part of the town.

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That the center is the moſt healthy part of the city; that a leſs proportion die annually here than in moſt country villages; and, as far as obſervations have hitherto been made, that it is probably as healthy as any ſpot upon earth, are ſurprizing facts: yet theſe facts are clearly evinced by the united evidence of ſix ſeparate diſtricts taken on a medium of ten years. Some conjectures, ſupported by a few facts, are hazarded concerning the cauſe of unhealthineſs in the ſuburbs. Future obſervations of a like kind, in different ſituations, will confute or confirm theſe conjectures, which, if true, may be of great importance to ſociety by diſcovering and avoiding the ſource of diſeaſe. Towns divided and numbered in ſeparate diſtricts, compared with their reſpective regiſters, and illuſtrated with a deſcription of every circumſtance peculiar to each, that may be ſuppoſed to influence health, might, by a numerous induction of facts, lead to a certain inveſtigation of the cauſe that renders towns ſo generally unhealty. A diligent and ſagacious attention to this ſubject might produce a diſcovery how to make towns as healthy as the country: a diſcovery of the moſt beneficial conſequence in this age of elegant refinement which collects the greateſt part of mankind into large towns.

TABLE II. DISEASES.

I. Febrile Diseases.	Under 1 year.	Betw. 1 and 2	2—3	3—5	5—10	10-15	15-20	20-30	30-40	40-50	50-60	60-70	70-80	80-90	90-100	Total.
Fever (Gen. 5. 6.)	1	1			5		3	11	3	3	4	4				35
Puerperal fever (5.)								3	1							4
Mortification (7.)					1					1	1	3				6
Sore throat (10.)		1		3					1							5
Inflam. of the bowels (16.)											1					1
Gravel and stone (19.)												1				1
Teething (Sauv. 198.)	3	1														4
Rheumatism (22.)											1					1
Gout (23.)												1				1
Natural small-pox (26.)	51	38	42	49	22											202
Miliary fever (29.)									1	1						2
Consumption (35.)			1	1		1	8	11	9	12	6	5				54
Haemorrhage (puerperal) (37.)								2								2
II. Nervous Diseases.																
Apoplexy and sudden death (40?)								2			1		2			5
Palsy (41.)										1	2		4			7
Indigestion (43.)									1		1					2
Locked jaw (46.)									1							1
Convulsion (48, 50.)	37	11		6	1	1					1					57
Asthma (52.)										1	6	6	3			16
Colic (55.)						1										1
Insanity (63.)												1				1
III. Diseases of the Habit.																
Weakness of infancy (65.)	13		2	1		1										17
Decay of age (66.)											2	14	25	29	2	72
Dropsy (71, 75.)					1			1	5	1	5	1				14
Dropsy of the brain (72.)						1										1
Dropsy of the chest (74.)											1					1
Venereal disease (81.)								1								1
Jaundice (87.)								1	1							2
IV. Local Diseases.																
Cancer (114.)											1	1				2
Rupture (124.)													1			1
Inv. uterus (puerperal) (125.)									1							1
Fistula (128.)								2								2
Ulcer (128.)										1						1
Unknown disease				1				1	2	5		2	4			15
Casualty				1			1	3			1	1	1			8
Total	105	52	45	62	30	5	12	38	26	26	34	40	40	29	2	546

TABLE I. Deaths, Ages and Conditions.

Ages.	Males.	Females.
Under 1 month —	11	6
Between 1—2 months	9	8
2—3	4	1
3—6	9	-
6—9	16	10
9 months and 1 year	7	19
1-2 years old	30	22
2—3	19	26
3—4	24	22
4—5	7	9
5—10	18	12
10—15	1	4
15—20	7	5
Total	162	149

Ages.	Batchelors.	Husbands.	Widowers.	Maids.	Wives.	Widows	Total.
20-25	6	2		6	2		16
25-30	5	2	1	3	11		22
30-35	2	3		3	4	1	13
35-40	3	4	1		5		13
40-45	2	3	2	2	6	2	17
45-50		4			3	2	9
50-55		4	2	2	5	2	15
55-60	2	6	3	2	5	1	19
60-65	3	7	1	4	3	4	22
65-70	1	7	2	2	1	5	18
70-75	2	6	1	4	1	9	23
75-80		3	6		3	5	17
80	1		2			4	7
81			3			2	5
82			1			3	4
83			1			2	3
84				2	1	2	5
85						1	1
86		1	1				2
87						1	1
88			1				1
89							
90						2	2
Total	27	52	28	30	50	48	235
							162
							140
						Total	546

TABLE III.

Season	Total of Deaths.		Month	Deaths by Small-pox.		
Winter	107	46	January	0	1	Winter.
		30	February	1		
		31	March	0		
Spring	90	25	April	0	6	Spring.
		30	May	3		
		35	June	3		
Summer	130	32	July	11	65	Summer.
		53	August	26		
		45	September	28		
Autumn	219	69	October	46	130	Autumn.
		76	November	44		
		74	December	40		
Total	546		Total	202		

Igitur saluberrimum ver est: proximè deinde ab hoc hiems: periculosior æstas: autumnus longè periculosissimus. CELS. lib. II. c. I.

TABLE IV.

Deaths by the small-pox under one year old.

	Males.	Females.	Total.
Under 1 month	0	0	0
Between 1 and 2 months	1	1	2
2 and 3	1	0	1
3 and 6	2	2	4
6 and 9	12	10	22
9 months and 1 year	6	16	22
Total	22	29	51

TABLE

TABLE V.

State of population, small-pox, and Fevers, in 1774.

Parishes.	Families.	Inhabitants.	Males.	Females.	Married.	Widowers.	Widows.	Under 15 years old.	Above 70 years old.	Recovered small-pox in 1774.	Dead of small-pox in 1774.	Ill of small-pox in Jan. 1775.	Not had small-pox in Jan. 1775.	Recovered fever in 1774.	Dead of fever in 1774.	Ill of fever in Jan. 1775.
St. Oswald's,	924	4027	914	2113	1340	64	189	1302	143	321	40	5	350	58	2	1
John's,	774	3187	1411	1776	1057	51	190	970	153	284	52	6	218	55	4	3
Mary's,	583	2392	1097	1295	892	41	89	805	100	240	45	3	205	70	5	8
Trinity,	330	1605	730	875	485	43	95	521	65	127	24	3	97	9	1	1
Peter's,	193	920	414	506	267	8	28	221	43	52	6	—	39	15	1	3
Bridget's,	154	623	283	340	218	7	27	170	26	52	6	1	35	4	2	—
Martin's,	154	611	280	331	230	12	30	164	30	47	18	—	35	9	3	5
Michael's,	135	575	239	336	152	22	40	130	30	15	2	—	31	4	2	1
Olave's,	134	536	246	290	194	4	37	185	21	42	8	1	43	22	9	3
Cathedral,	47	237	83	154	46	6	11	18	14	3	1	—	7	1	—	—
Total	3428	14713	5697	8016	4881	258	736	4486	625	1183	202	19	1060	257	28	24

TABLE VI.

General bill of the several parishes for ten years, viz. *from* 1764 *to* 1773 *inclusively.*

	Chr.	Bur.	Mar.
St. Oswald's,	949	1053	456
Mary's,	820	795	294
John's,	939	892	271
Peter's,	246	149	75
Trinity,	445	394	127
Olave's,	173	96	29
Michael's,	158	115	56
Bridget's,	131	110	69
Martin's,	101	103	50
Cathedral,	8	24	0
Total	3970	3731	1427
General Bill for the year 1774, males, 226; females, 195	421	546	141

TABLE VII.

The numbers that die annually in the several parishes, taken upon an average of ten years, viz. *from the year* 1764 *to* 1773 *inclusively.*

St. Mary's, ...	1 in 30
Oswald's, ...	1 in 36
John's, ...	1 in 36
Trinity, ...	1 in 41
Michael's, ...	1 in 50
Olave's, ...	1 in 55
Bridget's, ...	1 in 56
Martin's, ...	1 in 59
Peter's, ...	1 in 61
Cathedral, ...	1 in 87
The whole town, ...	1 in 40
The parishes within the walls, *viz.* Michael's, Olave's, Bridget's, Martin's, Peter's, and Cathedral, ...	1 in 58

TABLE

TABLE VIII.

The proportionable number of inhabitants that die annually in the following places.

Whites in Jamaica,	1 in 5
Vienna,	1 in $19\frac{1}{2}$
London,	1 in $20\frac{3}{4}$
Edinburgh,	1 in $20\frac{4}{5}$
Leeds,	1 in $21\frac{3}{5}$
Dublin,	1 in 22
Rome,	1 in 23
Amſterdam,	1 in 24
Breſlaw,	1 in 25
Berlin,	1 in $26\frac{1}{2}$
Northampton,	1 in $26\frac{1}{2}$
Shrewſbury,	1 in $26\frac{1}{2}$
Liverpool,	1 in $27\frac{1}{2}$
Mancheſter,	1 in 28
Country pariſhes.	
Pais de vaud,	1 in 45
Country pariſhes in Brandenburgh,	1 in 45
Others in Brandenburgh,	1 in 50
A country pariſh in Hampſhire for 90 years,	1 in 50
Iſland of Madeira,	1 in 50
Stoke Damerel in Devonſhire, for one year,	1 in 54

IX. *An Account of some Electrical Experiments, by Mr.* William Swift, *in a Letter to* John Glen King, *D. D. F. R. S.*

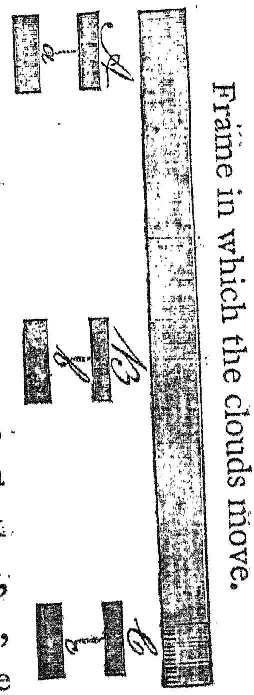
REV. SIR,

Greenwich, Jan. 26, 1778.

Read Jan. 29. 1777.

I BEG leave to lay before you an account of an electrical apparatus, which I have contrived, to shew the different effects of points and balls at the upper terminations of conductors, to secure houses and magazines of powder from damage by lightning. I have represented the clouds, which are added to my machine, by interposing three feet of water insulated, instead of continuing the metal from the prime conductor; this I apprehend to be analogous to the natural clouds, though it is not in the least necessary for the experiments I am first going to mention, the results of which are not affected by one method more than the other.

The clouds being charged slide on a frame with a graduated edge; and, as they pass the length of the frame, they make five revolutions round their own axis; for they are represented by a semi-circle, the radius of which is eighteen inches, consequently the extent of it is



is nearly four feet and a half, and is formed with materials well covered with metal. I place three houfes, ftanding in the ftate of nature (not connected with the cufhion) at a certain diftance from the frame, and equally diftant from each other, as may be feen in the fketch A, B, C, each houfe has a conductor, and is connected with magazines of powder, *a*, *b*, *c*; the reafon for making the clouds a femi-circle is, that when turned back they may be charged from the machine, without affecting, or being affected by, the points or balls on the tops of the houfes A, B, and C; and, by means of their motion round their own axes, I can increafe or diminifh at pleafure the velocity, which is afcertained by the graduated edge of the frame. I fix an electrometer on one of the conductors of the machine, and put points for the upper terminations of the conductors of the houfes.

Having thus prepared the machine, the femi-circular cloud being turned back, that is, within; the machine is charged till the index of the electrometer rifes upwards of 90°; the cloud being then put in motion, as it flides along the frame, revolves over the houfe A, with its length of $4\frac{1}{2}$ feet: in its paffage it empties itfelf, the elec-

trometer falling to o, but not the leaſt exploſion is perceived. The cloud then turning back in its progreſſive motion in the frame, is charged again while it paſſes on to B; at which point, by means of its motion round its axis, it revolves over the conductor B; it empties itſelf, the electrometer falls, and no exploſion is perceived: the ſame thing happens in the paſſage over the houſe C.

The machine remaining in the poſition as before, I place balls of a quarter of an inch diameter, at the upper terminations of the conductors of the houſes A, B, C, and with theſe balls, the experiments proceed almoſt as before; that is, the matter paſſes off with a little hiſſing noiſe, and now and then it gives a ſlight exploſion, the ſmallneſs of theſe balls differing little from points; but when I place balls of three-quarters of an inch diameter inſtead of the ſmall ones, the cloud, every time it paſſes over them, makes one or more exploſions, and fires the magazines *a*, *b*, *c*; and, notwithſtanding that, the index of the electrometer does not deſcend above 20°, and ſtarts up again as ſuddenly as it fell.

If balls are ſafer at the upper ends of conductors than points, it ſhould follow, that the larger the balls are, the greater the ſecurity; but from all theſe experiments I never found a ſhock with a point, and not always with a very ſmall ball: but the electrical matter paſſes off

 ſilently

ſilently with the points, and ſo entirely, that the electrometer falls to 5°. With balls a quarter of an inch diameter, indeed, it paſſes off with a little hiſſing noiſe, but this ſeldom amounts to a ſhock: but with balls three-quarters of an inch diameter an exploſion conſtantly happens, and the magazines are fired.

To put this matter ſtill more out of doubt, I place a ball of nine inches diameter on one of the conductors, and the exploſion is very violent, always more certain; and yet the machine does not diſcharge itſelf, for the electrometer falls not more than 20°.

The next experiment I make with the water conductor is, placing the houſes A, B, C, in a negative ſtate, by connecting them with the cuſhion of the machine, or with the outſide of a battery: when the cloud is charged and paſſes over the houſes, with points at the upper end of their conductors, there is no exploſion; the points ſeem to draw off all the electrical matter during the paſſage of the clouds of four feet and a half long: but when, in this poſition of the houſes, balls of three-quarters of an inch diameter are placed inſtead of points, there is a ſmall exploſion, and a conſiderable *reſiduum* of the matter is left in the battery. I then change the inſulated water for wire to compleat the circle: on the paſſage of the clouds over the houſes there is a conſi-

derable explosion, whether points or balls are the upper terminations of the conductors of the houses; but no *residuum* is left in the battery.

Hence appears the difference of effect, whether the houses stand in a state of nature, or in a negative state; and whether the conductors be made complete with wire, or water insulated.

I have by sixteen years practice been convinced how difficult it is to draw general conclusions from any electrical experiments, and therefore it becomes me to propose my conjectures with the greatest diffidence; but, I apprehend, the result of many experiments shew that points at the upper termination of conductors gradually diminish or draw off the electrical matter, so as to prevent any damage to the buildings on which they are placed, by preventing a violent explosion; and that, on the contrary, balls, though perhaps they will repel the electrical matter in some degree, yet from that very circumstance, probably, the explosion, when it happens, is violent, and attended with danger.

I am, &c.

X. *An Account of the Island of* Sumatra, *&c.* *By Mr.* Charles Miller. *Communicated by* Edward King, *Esq.*

TO EDWARD KING, ESQ.

Bedford-row,
December 12, 1777.

DEAR SIR,

Read Jan. 29, 1778.

THE attention which has been paid by the learned world to the accounts lately publiſhed of the iſlands of the South-ſea, has led me to think, that the incloſed account of the iſland of Sumatra, particularly of ſome of its interior parts, together with that of the neighbouring iſland of Enganho, might not be wholly unacceptable.

It is compiled from ſeveral letters of Mr. CHARLES MILLER (ſon of the late botanic gardener) now in the ſervice of the Eaſt India Company at Sumatra; and, as they were addreſſed to different friends, without the moſt diſtant idea of their contents being communicated beyond that circle, allowance muſt be made for inaccuracies of ſtile and want of connection; for I was unwilling to attempt to ſupply any thing that ſeemed wanting, judging

judging that authentic information is more valuable than the beſt wrought tale.

If you think this paper contains any thing likely to afford either information or amuſement to the Royal Society, you will do me the honour to preſent it.

I am, &c.

JOHN FRERE.

Extracts from ſeveral letters from Mr. CHARLES MILLER *(ſon of the late botanic gardener) now ſettled at* Fort Malbro' *near* Bencoolen; *giving ſome account of that place, of the interior parts of* Sumatra, *and of a neighbouring iſland never known to have been viſited by any* European.

FORT MALBRO' is ſituated about a mile and a half to the South of the Malay town [Bencoolen] where the company formerly had their factory; but removed from thence about the year 1710, on account of the unhealthineſs of the place.

The fort, from which the ſettlement takes its name, ſtill remains in the ſame ſtate in which the French left it

in

in 1761; when, after taking the place, they thought it not worth keeping, and accordingly blew up the baftions, and deferted the fettlement.

The houfes here are, almoft all, built, cicled, roofed, and floored, with a kind of reed called bamboo, and thatched with the leaves of the fage-tree, and would all be called cottages in England, making a very mean appearance. They are placed in no kind of order; moft of them are raifed from the ground on wood or brick pillars fix or eight feet high; within they are not much unlike a fet of rooms in a college, as they confift of one large room called a hall, out of which two doors lead, the one to a bed-room, and the other to an office or ftudy.

The climate is far from being fo difagreeably hot as it is reprefented to be, or as one might expect from our vicinity to the line; the thermometer (of which I have kept a journal for a year paft) is never lower in a morning at fix than 69°, or higher than 76°. At noon it varies from 79° to 88°; and at eight P. M. from 73° to 78° or 80°. I have once only feen it at 90°, and in the Batta country, immediately under the line, I have feen it frequently at fix A. M. as low as 61°. We have always a fea-breeze, which fets in at about nine o'clock, and continues to Sun-fet, and is generally pretty frefh: this tempers the heat fo much, that I have never been incommoded

moded by it (even in the midſt of the day) ſo much as I have frequently been on a ſummer's day in England. Rain is very frequent here; ſometimes very heavy, and almoſt always attended with thunder and lightning. Earthquakes are not uncommon; we have had one in particular, ſince my arrival, which was very violent, and did much damage in the country. There are ſeveral volcanos on the iſland; one within ſight of Malbro', which almoſt conſtantly emits ſmoke, and, at the time of the earthquake, emitted fire.

The Engliſh ſettled here (excluſive of the military) are between ſeventy and eighty, of which about fifty are at Malbro'. They live full as freely as in England, and yet we have loſt but one gentleman during the laſt ſix months; a proof that this climate is not very unhealthy.

The people who inhabit the coaſt are Malays, who came hither from the peninſula of Malacca: but the interior parts are inhabited by a very different people, and who have hitherto had no connexion with the Europeans. Their language and character differ much from thoſe of the Malays, the latter uſing the Arabic character; but all the interior nations which I have viſited, though they differ from one another in language, uſe the ſame character.

The people between the diſtricts of the Engliſh company, and thoſe of the Dutch at Palimban on the other ſide the iſland, write on long narrow ſlips of the bark of a tree, with a piece of bamboo; they begin at the bottom, and write from the left-hand to the right, which I think is contrary to the cuſtom of all other Eaſtern nations.

This country is very hilly, and the acceſs to it exceedingly difficult, there being no poſſibility of a horſe going over the hills. I was obliged to walk the whole way, and in many places bare-foot, on account of the ſteepneſs of the precipices. The inhabitants are a free people, and live in ſmall villages called Dooſans, independant of each other, and governed each by its own chief [Doopattee]. All of them have laws, ſome written ones, by which they puniſh offenders, and terminate diſputes. They have almoſt all of them, particularly the women, large ſwellings in the throat, ſome nearly as big as a man's head, but in general as big as an oſtrich's egg, like the goitres of the Alps. It is by them ſaid to be owing to their drinking a cold white water; I fancy it muſt be ſome mineral water they mean. Near their country is a volcano: it is very mountainous, and abounds with ſulphur, and I dare ſay with metals too, though no mines are worked here. If this diſtemper be produced here by this cauſe, perhaps in the Alpine countries it may take

its

its origin from a ſimilar one, and not, as has been imagined, from ſnow-water: certain it is, there is no ſnow here to occaſion it. In almoſt all the central parts from Moco-moco northwards, they find gold and ſome iron; but this diſtemper is unknown there. I have met here with a rivulet of a ſtrong ſulphurated water, which was ſo hot a quarter of a mile below its ſource, that I could not walk acroſs it.

The country called the Caſſia country lies in latitude 1° N. inland of our ſettlement of Tappanooly: it is well inhabited by a people called Battas, who differ from all the other inhabitants of Sumatra in language, manners, and cuſtoms. They have no religious worſhip, but have ſome confuſed idea of three ſuperior beings; two of which are of a benign nature; and the third an evil genius, whom they ſtile Murgiſo, and to whom they uſe ſome kind of incantation to prevent his doing them hurt. They ſeem to think their anceſtors are a kind of ſuperior beings, attendant always upon them. They have no king, but live in villages [Compongs] abſolutely independant of each other, and perpetually at war with one another: their villages they fortify very ſtrongly with double fences of camphire plank pointed, and placed with their points projecting outwards, and between theſe fences they put pieces of bamboo, hardened by fire, and

likewiſe pointed, which are concealed by the graſs, but will run quite through a man's foot. Without theſe fences they plant a prickly ſpecies of bamboo, which ſoon forms an impenetrable hedge. They never ſtir out of theſe Compongs unarmed; their arms are match-lock guns, which, as well as the powder, are made in the country, and ſpears with long iron heads. They do not fight in an open manner, but way-lay and ſhoot or take priſoner ſingle people in the woods or paddy-fields. Theſe priſoners, if they happen to be the people who have given the offence, they put to death and eat, and their ſkulls they hang up as trophies in the houſes where the unmarried men and boys eat and ſleep. They allow of polygamy: a man may purchaſe as many wives as he pleaſes; but their number ſeldom exceeds eight. They have no marriage ceremony; but, when the purchaſe is agreed on by the father, the man kills a buffalo or a horſe, invites as many people as he can; and he and the woman ſit and eat together before the whole company, and are afterwards conſidered as man and wife. If afterwards the man chuſes to part with his wife, he ſends her back to her relations with all her trinkets, but they keep the purchaſe-money; if the wife diſlikes her huſband, her relations muſt repay double the purchaſe-money.

A man detected in adultery is puniſhed with death, and the body eaten by the offended party and his friends: the

the woman becomes the ſlave of her huſband, and is rendered infamous by cutting off her hair. Public theft is alſo puniſhed with death, and the body eaten. All their wives live in the ſame houſe with the huſband, and the houſes have no partition; but each wife has her ſeparate fire-place.

Girls and unmarried women wear ſix or eight large rings of thick braſs wire about their neck, and great numbers of tin rings in their ears; but all theſe ornaments are laid aſide when they marry.

They often preſerve the dead bodies of their Radjas (by which name they call every freeman that has property, of which there are ſometimes one, ſometimes more, in one Compong, and the reſt are vaſſals) for three months and upwards before they bury them: this they continue to do by putting the body into a coffin well caulked with dammar (a kind of rezin): they place the coffin in the upper part of the houſe, and having made a hole at the bottom, fit thereto a piece of bamboo, which reaches quite through the houſe, and three or four feet into the ground: this ſerves to convey all putrid moiſture from the corpſe without occaſioning any ſmell. They ſeem to have great ceremonies at theſe funerals; but they would not allow me to ſee them. I ſaw ſeveral figures dreſſed up like men, and heard a kind of ſinging

and dancing all night before the body was interred: they alſo fired a great many guns. At theſe funerals they kill a great many buffaloes; every Radja, for a conſiderable diſtance, brings a buffalo and kills it at the grave of the deceaſed, ſometimes even a year after his interment; we aſſiſted at the ceremony of killing the 106th buffalo at a radja's grave.

The Battas have abundance of black cattle, buffaloes, and horſes, all which they eat. They alſo have great quantities of ſmall black dogs, with erect pointed ears, which they fatten and eat. Rats and all ſorts of wild animals, whether killed by them or found dead, they eat indifferently. Man's fleſh may rather be ſaid to be eaten *in terrorem*, than to be their common food; yet they prefer it to all others, and ſpeak with peculiar raptures of the ſoles of the feet and palms of the hands. They expreſſed much ſurprize on being informed that white people did not kill, much leſs eat, their priſoners.

Theſe people, though cannibals, received me with great hoſpitality and civility; and though it was thought very dangerous for any European to venture among them, as they are a warlike people, and extremely jealous of ſtrangers; yet I took only ſix Malays as a guard, but was eſcorted from place to place by thirty, forty, and

ſometimes

ſometimes one hundred of the natives, armed with match-lock guns and matches burning.

It is from this country that moſt of the caſſia ſent to Europe is procured; and I went there in hopes of finding the cinnamon, but without ſucceſs. The caſſia tree grows to fifty or ſixty feet, with a ſtem of about two feet diameter, with a beautiful regular ſpreading head; its flowers or fruit I could not then ſee, and the country people have a notion that it produces neither.

Camphire and Benjamin trees are in this country in great abundance; the former grows to the ſize of our largeſt oaks, and is the common timber in uſe: I have ſeen trees near one hundred feet high. Its leaves are acuminated and very different from the camphire tree ſeen in the botanic gardens, which is the tree from which the Japaneſe procure their camphire by a chemical proceſs; whereas in theſe trees the camphire is found native in a concrete form. Native camphire ſells here at upwards of 200£. *per* Cwt. to carry to China; what the Chineſe do to it, I cannot ſay; but, though they purchaſe it at 250£. or 300£. they ſell it again for Europe at about a quarter of the money. I have never been able to ſee the flower of the camphire tree; ſome abortive fruit I have frequently found under the trees, they are in a cup, like an

acorn, but the *laciniæ calycis* are four or five times longer than the feed.

I have taken other journies into different parts of the interior country, never before vifited by any Europeans. Thefe journies were performed on foot, through fuch roads, fwamps, &c. as were to appearance almoft impaffable. I have been hitherto fo fortunate as to meet with no obftruction from the natives; but, on the contrary, have been hofpitably received every where. Almoft all the country has been covered with thick woods of trees moftly new and undefcribed, and is not one-hundredth part inhabited.

It is amazing how poor the *Fauna* of this country is, particularly in the *mammalia* and *aves*. We have abundance of the *fimia gibbon* of BUFFON: they are quite black, about three feet high, and their arms reach to the ground when they ftand erect; they walk on their hind legs only, but I believe very rarely come down to the ground. I have feen hundreds of them together on the tops of high trees. We have feveral other fpecies of the *fimia* alfo; but one feldom fees them but at a great diftance. The *oerang oatan*, or wild-man (for that is the meaning of the words) I have heard much talk of, but never feen; nor can I find any of the natives here that have feen it. The tiger is to be heard of in almoft every

part

part of this iſland: I have never ſeen one yet, though I have frequently heard them when I have ſlept in the woods, and often ſeen the marks of their feet. They annually deſtroy near one hundred people in the country where the pepper is planted; yet the people are ſo infatuated that they ſeldom kill them, having a notion that they are animated by the ſouls of their anceſtors.

Of tiger-cats we have two or three ſorts; elephants, rhinoceros, elks, one or two other kind of deer, buffaloes, two or three ſorts of muſtelæ, porcupines, and the ſmall hog-deer, almoſt compleat the catalogue of our *mammalia*.

Birds I have ſeen very few indeed, and very few ſpecies of inſects. Ants, of twenty or thirty kinds, abound here ſo much as to make it almoſt impoſſible to preſerve birds or inſects. I have frequently attempted it, but in vain.

I have met with one inſtance, and one only, of a ſtratum of foſſil ſhells. I had ſome notion that it was an obſervation (of CONDAMINE's I think) that no ſuch thing was to be found between the tropics.

The iſland of Enganho, though ſituated only about ninety miles to the Southward of Malbro', was ſo little known, on account of the terrible rocks and breakers which entirely ſurround it, that it was even doubtful

 whether

whether it was inhabited: to this iſland I have made a voyage. With great difficulty and danger we beat up the whole South-weſt ſide of it, without finding any place where we could attempt to land; and we loſt two anchors, and had very near ſuffered ſhipwreck before we found a ſecure place into which we might run the veſſel. At laſt, however, we diſcovered a ſpacious harbour at the South-eaſt end of the iſland, and I immediately went into it in the boat, and ordered the veſſel to follow me as ſoon as poſſible, for it was then a dead calm. We rowed directly into this bay; and as ſoon as we had got round the points of an iſland which lay off the harbour, we diſcovered all the beach covered with naked ſavages, who were all armed with lances and clubs; and twelve canoes full of them, who, till we had paſſed them, had lain concealed, immediately ruſhed out upon me, making a horrid noiſe: this, you may ſuppoſe, alarmed us greatly; and as I had only one European and four black ſoldiers, beſides the four laſcars that rowed the boat, I thought it beſt to return, if poſſible, under the guns of the veſſel, before I ventured to ſpeak with them. In caſe we were attacked, I ordered the ſeapoys to reſerve their fire till they could be ſure their balls would take effect; and then to take advantage of the confuſion our firing would throw the ſavages into, and attack them, if poſſible, with their

bayonets. The canoes, however, after having purſued for a mile, or a mile and a half, luckily ſtopped a little to conſult together, which gave us an opportunity to eſcape them, as they did not care to purſue us out to ſea. The ſame afternoon the veſſel came to an anchor in the bay, and we were preſently viſited by fifty or ſixty canoes full of people. They paddled round the veſſel, and called to us in a language which nobody on board underſtood, though I had people with me who underſtood the languages ſpoken on all the other iſlands. They ſeemed to look at every thing about the veſſel very attentively; but more from the motive of pilfering than from curioſity, for they watched an opportunity and unſhipped the rudder of the boat, and paddled away with it. I fired a muſquet over their heads, the noiſe of which frightened them ſo, that all of them immediately leaped into the ſea, but ſoon recovered themſelves and paddled off.

They are a tall, well-made people; the men in general about five feet eight or ten inches high; the women ſhorter and more clumſily built. They are of a red colour, and have ſtraight, black hair, which the men cut ſhort, but the women let grow long, and roll up in a circle on the top of their heads very neatly. The men go entirely naked, and the women wear nothing more than a very narrow ſlip of plantain leaf. The men

 always

always go armed with ſix or eight lances, made of the wood of the cabbage-tree, which is extremely hard; they are about ſix feet long, and topped with the large bones of fiſh ſharpened and barbed, or with a piece of bamboo hardened in the fire, very ſharp-pointed, and its concave part armed with the jaw bones and teeth of fiſh, ſo that it would be almoſt impoſſible to extract them from a wound. They have no iron or other metal that I could ſee, yet they build very neat canoes; they are formed of two thin boards ſewed together, and the ſeam filled with a reſinous ſubſtance. They are about ten feet long, and about a foot broad, and have an outrigger on each ſide, to prevent their over-ſetting. They ſplit trees into boards with ſtone wedges.

Their houſes are circular, ſupported on ten or twelve iron-wood ſticks about ſix feet long: they are neatly floored with plank, and the roof riſes immediately from the floor in a conical form, ſo as to reſemble a ſtraw bee-hive; their diameter is not above eight feet.

Theſe people have no rice, fowls, or cattle, of any kind: they ſeem to live upon cocoa-nuts, ſweet potatoes, and ſugar-canes. They catch fiſh, and dry them in the ſmoke; theſe fiſh they either ſtrike with their lances, or catch in a drawing net, of which they make very neat ones.

They

They do not chew betel, a cuſtom which prevails univerſally among the Eaſtern nations.

I went on ſhore the day after the veſſel anchored in the bay, hoping to be able to ſee ſomething of the country, and to meet with ſome of the chiefs. I ſaw a few houſes near the beach, and went towards them; but the natives flocked down to the beach, to the number of ſixty or ſeventy men, well armed with their lances, &c. and put themſelves in our way; yet, when we approached them, they retreated ſlowly, making ſome few threatening geſtures. I then ordered my companions to halt and to be well on their guard, and went alone towards them: they permitted me to come amongſt them, and I gave them ſome knives, pieces of cloth, and looking-glaſſes, with all which they ſeemed well pleaſed, and allowed me to take from them their lances, &c. and give them to my ſervant, whom I called to take them. Finding them to behave civilly, I made ſigns that I wanted to go to their houſes and eat with them; they immediately ſent people who brought me cocoa-nuts, but did not ſeem to approve of my going to their houſes: however, I determined to venture thither, and ſeeing a path leading towards them, I went forward attended by about twenty of them, who, as ſoon as we had got behind ſome trees, which prevented my people ſeeing us, began to lay violent hands

on my cloaths, and endeavour to pull them off; but having a ſmall hanger, I drew it, and, making a ſtroke at the moſt officious of them, retreated as faſt as poſſible to the beach. Soon after we heard the ſound of a conch-ſhell; upon which all the people retired, with all poſſible expedition, to a party of about two hundred, who were aſſembled at about a mile diſtance. It was now near Sun-ſet, and we were near a mile from our boat; and, as I was apprehenſive we might be way-laid in our return if we ſtaid longer, I ordered my people to return with all poſſible ſpeed; but firſt went to the houſes the natives had abandoned, and found them ſtripped of every thing; ſo that I ſuppoſe this party had been amuſing us while others had been employed in removing their wives, children, &c. into the woods. I intended to have attempted another day to have penetrated into the country, and had prepared my people for it; but the inconſiderate reſentment of an officer, who was ſent with me, rendered my ſcheme abortive. He had been in the boat to ſome of the natives who had waded out on a reef of rocks and called to us; they had brought ſome cocoa-nuts, for which he gave them pieces of cloth: one of them ſeeing his hanger lying beſide him in the boat, ſnatched it and ran away; upon which he fired upon them, and purſued them to ſome of their houſes, which,

finding

finding empty, he burnt. This set the whole country in alarm; conch-shells were sounded all over the bay, and in the morning we saw great multitudes of people assembled in different places, making use of threatening gestures; so that finding it would be unsafe to venture among them again, as, for want of understanding their language, we could not come to any explanation with them, I ordered the anchor to be weighed, and sailed out of the bay, bringing away two of the natives with me.

In our return home my desire of seeing some yet unexplored parts of the island of Sumatra, occasioned me to order the vessel to put me on shore at a place called Flat Point, on the Southern extremity of the island, from whence I walked to Fort Malbro'. In this journey I underwent great hardships, being sometimes obliged to walk on the sandy beach, exposed to the Sun, from six in the morning till six at night, without any refreshment; sometimes precipices to ascend or descend, so steep that we could only draw ourselves up, or let ourselves down, by a rattan; at other times rapid rivers to cross, and then to walk the remaining part of the day in wet cloaths. The consequence of these hardships has been a violent fever; but, much as I then regretted having quitted the ship, I had, when I came to Fort Malbro', more reason to rejoice; for I then found, that the vessel, in her voyage

home,

home, was loſt, and every ſoul on board periſhed. This has, however, been a ſevere ſtroke upon me; for as I was obliged to leave all my baggage on board, it being impracticable to carry it over land, I loſt all my cloaths, books, ſpecimens, manuſcripts, notes, arms, &c. from Enganho; in ſhort, almoſt every thing which I had either brought with me, or collected during my reſidence in this iſland.

I forgot to mention, that when I was at Tappanooly I ſaw what I find in PURCHAS's Pilgrim called *the wonderful plant of Sombrero:* his account, however, is ſomewhat exaggerated, when he ſays it bears leaves and grows to be a great tree. The name by which it is known to the Malays is *Lalan-lout*, that is, ſea-graſs. It is found in ſandy bays, in ſhallow water, where it appears lik ſlender ſtrait ſtick, but, when you attempt to touch immediately withdraws itſelf into the ſand. I could never obſerve any *tentacula:* a broken piece, near a foot long, which, after many unſucceſsful attempts, I drew out, was perfectly ſtrait and uniform, and reſembled a worm drawn over a knitting-needle; when dry it is a coral.

The ſea cocoa-nut, which has long been erroneouſly conſidered as a marine production, and been ſo extremely ſcarce and valuable, is now diſcovered to be the fruit of a palm

a palm with flabelliform leaves, which grows abundantly on the ſmall iſlands to the Eaſtward of Madagaſcar, called in our charts Mahi, &c. and by the French *Les Iſles de Sechelles*. To theſe iſlands the French have ſent a large colony, and planted them with clove and nutmeg-trees, as they have likewiſe the iſlands of Bourbon and Mauritius.

XII. *A Meteorological Diary*, &c. *kept at* Fort St. George *in the* Eaſt Indies. *By Mr*. William Roxburgh, *Aſſiſtant-ſurgeon to the Hoſpital at the ſaid Fort. Communicated by Sir* John Pringle, *Bart. P. R. S.*

Read Jan. 29, 1777.

THE manner in which I keep my meteorological obſervations is as follows:

A thermometer without doors; a barometer and thermometer within doors: the barometer and thermometer within doors are kept cloſe together, for the ſake of correcting the barometer if required. I obſerve them three times a day, as *per* diary. I alſo ſet down the direction and ſtrength of the wind, and the ſtate of the weather. I diſtinguiſh four degrees of ſtrength of the wind; namely, gentle, briſk, ſtormy, and what we call a tufoon in India, which you will find marked with the numbers 1, 2, 3, and 4, beſides no ſenſible wind, which is marked with a cypher.

I am aſhamed to ſay, that the rain-gage I had during the rainy ſeaſon was ſo indifferent, that I could not with any degree of certainty meaſure the quantity of rain that fell

fell here. I have now got a tin cylinder, $5\frac{2}{10}$ths inches in diameter, or $16\frac{4}{10}$ths inches in circumference, and 30 inches deep: the quantity of rain that falls I intend to meaſure with a ſcale divided into inches and twentieths of an inch; the depth ſhall be ſet down every morning if it has rained. I have it placed on the roof of my houſe, which is about twenty-five feet high; at a conſiderable diſtance from any other building, &c. except the hoſpital, which is diſtant about one hundred yards, and of the ſame height; no trees above twelve feet high within many hundred yards.

The thermometer without doors I have placed under a ſmall ſhady tree, through which the Sun cannot penetrate, at the ſame time it is well expoſed to air and wind. Every inch on the ſcale of the barometer I uſe is divided into twenty equal parts; it is a portable one, made by RAMSDEN.

The tube of the thermometer without doors is twelve inches long, placed upon a plain open box-wood ſcale, made by NAIRNE and BLUNT.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the weather, &c. at Fort St. George.
1776 Oct. 1	18	—	80	29.18	1	W	Cloudy, some rain in the night.
	1½	—	83	29.17	1	W	Cloudy.
	10	—	82	29.18	0	—	Some clouds.
2	18	—	80	29.17	1	W	Cloudy, much dew on the grass.
	2	—	87	29.16	0	—	Many heavy clouds all round, very sultry.
	10	—	81	30.00	1	W	Cloudy, rained hard two hours this evening.
3	18	—	79	29.14	1	W	Fair, much dew.
	1½	—	89	29.13	1	W	Fair.
	10	—	83	29.17	0	—	Hazy, some clouds.
4	18	—	79	29.18	1	W	Fair.
	2	—	88	29.19	1	N	Hazy, sun-shine.
	10	—	83	29.19	0	—	A few clouds, exceedingly close.
5	18	—	80	29.18	1	W	Fair.
	2	—	88	29.17	2	N	Fair, ther. rose to 91° in the wind.
	9	—	85	29.18	0	—	Fair.
6	18	—	80	29.19	0	—	Fair, much dew.
	1	—	87	29.18	1	ESE	Fair, the sea-breeze just come in.
	10	—	84	29.18	1	S	Fair.
7	18	—	82	29.19	0	—	Fair.
	2	—	87	29.19	2	NE	Fair, very pleasant.
	10	—	84	29.19	1	N	Clear, not a cloud to be seen.
8	17½	—	82	29.19	0	—	Clear.
	2	—	86	30.00	1	E	Fair.
	10	—	83	29.19	0	—	Fair.
9	18	—	79	29.19	0	—	Fair.
	2	—	85	29.19	2	NE	Fair.
	10	—	83	29.19	1	N	Fair.
10	18	—	79	29.19	1	NW	Fair, a little dew.
	2	—	85	29.18	1	NE	Fair.
	10	—	83	29.19	1	E	Clear.
11	18	—	80	29.19	0	—	Fair.
	2	—	85	29.19	2	NE	Fair.
	10	—	83	30.00	1	NNE	Clear.
12	18	73	81	29.19	1	NW	Fair and pleasant, a little dew.
	2	85	85	29.19	1	NE	Fair.
	10	81	83	29.19	1	N	Fair.
13	18	74	82	29.19	0	—	Fair.
	1½	87	85	29.19	1	E	Fair.
	10	82	83	30.00	1	E	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the weather, &c.
1776 Oct. 14	18	74	82	29.19	1	NW	Fair.
	1½	87	85	29.19	1	E	Fair.
	9½	82	83	29.19	1	NE	Fair.
15	18	76	83	29.19	1	NW	Fair.
	3	87	84	29.19	1	E	Fair.
	10½	82	81	30.00	1	NE	Fair.
16	18	80	83	30.00	0	——	A few light clouds round the horiz.
	2	86	86	30.00	1	ENE	Fair.
	10	82	84	30.01	1	NE	Fair.
17	18	76	83	30.00	0	——	Fair.
	10	82	84	30.00	1	E	Fair.
18	18	76	81	30 00	0	——	Fair.
	2½	86	87	30.00	1	E	Fair.
	10	82	84	30.00	0	——	A few clouds.
19	18	75	81	29.19	1	W	Hazy, little or no dew.
	1	91	89	29.19	1	N	Hazy, can fee the fea-breeze at a diftance.
	8½	83	85	29.19	0	——	Fair.
20	18	76	81	29.19	1	W	Fair.
	2	86	85	29.19	1	SE	A little hazy.
	10	83	85	30.00	1	S	Hazy, a large woolly circle round the Moon.
21	18	77	82	30.00	1	W	Clouds to the eaftward.
	23	79	83	30.00	2	SE	Very black in the SE, begins to blow hard.
	2	85	84	29.19	1	E	Fair, all the threatening come to nothing.
	10	81	83	30.01	1	E	Clear.
22	18	75	82	30.00	1	W	A few clouds.
	2	88	87	30.00	1	E	Black to the Eaftward.
	10	80	83	30.01	0	——	A few clouds.
23	18	75	82	30.00	0	——	Fair, a fhower in the night.
	9	82	84	30.00	1	SE	Fair.
24	18	76	82	30.00	0	——	Fair.
	2	87	87	29.19	1	E	Fair.
	10	81	84	30.00	0	——	Clouds round the horizon.
25	18	75	82	29.19	0	——	Some clouds.
	2	86	85	30.00	1	E	Some clouds.
	10	74	80	30.00	1	N	Hard rain fince 7, and continues.
26	18	72	77	30.00	1	N	Hard rain, fome light. and thund.
	2	79	80	29.19	2	N	Rained till 1, now cloudy.
	11	79	80	29.19	1	N	Cloudy.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the weather, &c.
1776 Oct. 27	18	74	78	29.19	1	NW	Cloudy.
	2	83	83	29.19	2	N	Cloudy.
	9½	77	80	29.18	2	N	Cloudy, with some rain, lightning to the S.
28	18	74	79	29.18	1	N	Cloudy.
	6	86	78	29.18	1	NW	Cloudy, has rained hard and long.
	10	75	78	29.19	0	——	Cloudy, rainy, lightning.
29	18	74	78	29.17	0	——	Cloudy.
	1½	83	81	29.18	1	SE	Cloudy, frequent hard showers, with thunder.
	9	75	79	29.19	1	SE	Cloudy, ditto.
30	18	74	78	29.18	0	——	Cloudy, ditto.
	1½	85	82	29.19	2	E	Fair all forenoon, now begins to blow and rain.
	10	79	81	29.19	1	E	Cloudy round the horizon.
31	18	78	80	30.00	0	——	A few clouds.
	1	85	82	30.00	2	ESE	Fair.
	10	79	81	30.00	1	ESE	Clear.
Nov. 1	18	78	80	30.00	1	E	Fair.
	2	85	83	30.00	2	ESE	A few clouds.
	10	79	80	30.00	1	E	Fair.
2	18	77	80	30.00	1	E	Fair.
	2	84	84	29.19	2	NE	Fair.
	11	78	81	30.00	1	NE	Fair.
3	18	76	80	29.19	1	W	Fair.
	2	85	84	29.19	2	N	Fair.
	10	78	81	30.01	1	N	Fair.
4	18	74	78	30.00	1	N	Cloudy.
	2	85	83	30.00	2	N	Cloudy, threatens to the NE.
	10	79	81	30.00	2	N	Cloudy.
5	18	73	80	30.00	3	N	Cloudy, begins to rain hard.
	2	73	76	30.00	1	NW	Hard rain.
	9	79	78	30.00	1	NE	Rainy, very dark.
6	18	73	78	29.19	1	NW	Cloudy.
	2	81	79	29.19	2	N	Cloudy.
	10	77	78	29.18	2	N	Cloudy.
7	18	71	76	29.19	1	NW	Cloudy, rained hard in the night.
	2	74	79	29.19	2	N	Cloudy, frequent hard showers.
	10	73	77	29.18	2	N	Cloudy, ditto, very dark.
8	18	72	76	29.17	1	W	Cloudy, ditto.
	3	75	75	29.16	1	NW	Cloudy, ditto.
	10	71	74	29.17	2	NW	Cloudy, very dark.

1776

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the Weather, &c.
Nov. 9	18	71	74	29.17	1	NW	Cloudy, frequent hard ſhowers.
	2	76	76	29.16	1	NW	Cloudy, looks threatening.
	10	74	76	29.17	0	——	Cloudy, lightning.
10	18	72	76	29.18	0	——	A few clouds, a great dew on the graſs.
	2	82	79	29.17	1	SW	Cloudy, ſmall rain at times.
	10	76	78	29.19	1	SE	Clear.
11	18	74	80	29.18	0	——	A dark cloſe morning.
	3	79	80	29.18	1	NE	Hazy.
	11	79	79	29.19	0	——	Some clouds.
12	18	73	77	29.18	1	SW	A cloſe, rainy morning.
	2	77	77	29.18	0	——	Cloudy, has rained all forenoon.
	10	75	78	29.19	0	——	Fair.
13	18	74	82	30.00	1	E	Cloudy, a great dew.
	1	83	80	30.00	1	SE	A pleaſant ſea breeze.
	10	78	79	30.00	0	——	Fair.
14	18	75	79	30.01	0	——	A little hazy, and black to the NE.
	22	76	79	30 02	2	NE	Juſt beginning to rain hard.
	2½	79	80	30.00	0	——	Fair, rained only about 30′.
	11	78	80	30 01	1	NE	Fair.
15	18	77	79	30.00	0	——	Fair, a great dew.
	2	82	81	30.00	1	NE	Fair.
	10	79	81	30.01	1	NE	Fair.
16	18	76	80	30 01	1	NW	Many heavy clouds at a diſtance.
	3	84	82	30.00	1	NE	Fair.
	11	79	82	30.02	1	NE	Some clouds.
17	18	75	80	30.01	1	NW	Cloudy.
	2½	83	84	30 00	1	N	Fair.
	11	78	81	30.01	1	N	Fair.
18	18	77	80	30.00	1	N	Fair, much dew.
	3	82	84	30 01	1	NE	Fair.
	11	77	81	30.02	1	N	Fair.
19	18	70	79	30 01	1	NW	Fair.
	2	82	82	30.00	1	N	Fair.
	11	85	80	30.01	1	N	Clear.
20	17½	74	76	30.01	1	N	Cloudy.
	2	82	82	30.01	1	N	Hazy.
	10	79	81	30.02	1	NW	Some clouds.
21	18	74	80	30.01	1	NW	Some clouds.
	2	80	80	30 01	2	N	Some clouds.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the weather, &c.
1776 Nov. 23	18	77	80	30.03	2	N	Cloudy, dark, and threatening.
	2	80	80	30.01	2	N	Cloudy, ftill gloomy.
	11	78	80	30.03	2	N	Hazy, with fome clouds.
24	18	71	77	30.02	1	NW	Cloudy, little or no dew for fome morn. paft.
	3	80	81	30.01	1	NE	Cloudy.
	10	77	80	30 02	1	N	Cloudy, at ½ paft 8, two flight fhocks of an earthquake.
25	18	71	78	30.02	1	NW	Cloudy.
	1	81	81	30.02	1	N	Cloudy.
	12	76	80	30.01	1	N	Fair.
26	18	70	78	30.01	1	NW	Fair.
	1	82	81	30.01	1	NE	Fair.
	9	77	80	30.01	2	N	Fair.
27	18	70	77	30.01	1	NW	Fair, a great dew.
	2	81	82	30.01	1	NE	Fair.
	12	73	81	30.02	1	N	A little hazy.
28	18	71	77	30.00	1	NW	A few clouds and great dew.
	2	82	82	30.01	2	N	Fair.
	10	77	80	30.01	2	N	Some clouds.
29	18	70	78	30.00	1	NW	Fair.
	3	82	83	30.01	1	N	Fair.
	11	77	80	30.01	1	N	Fair.
30	18	70	77	30.01	1	NW	Fair.
	1	82	82	30.01	1	NE	Fair.
	10	77	80	30.01	2	N	Fair.
Dec. 1	18	70	77	30.01	1	NW	Fair.
	2½	80	81	30.00	1	NNE	Fair.
	10	77	80	30.01	2	N	Clear.
2	18	70	70	30.01	1	N	Fair.
	1	82	81	30.01	1	NE	Fair.
	11	87	80	30.01	1	N	Fair.
3	18	70	77	30.00	1	NW	Fair.
	1	82	83	30.00	1	NE	Fair.
5	18	72	73	30.01	1	N	Some clouds.
	1	82	82	30.01	2	N	Some clouds.
	10	77	80	30.01	2	N	Some clouds.
6	18	70	79	30.01	1	W	Some clouds.
	2	82	82	30.01	2	N	Fair.
	10	76	81	30.02	2	N	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the Weather, &c.
Dec. 7	18	76	80	30.01	2	NNE	Some clouds, had a light shower.
	0	81	81	30.01	1	NNE	Fair.
	11	77	80	30.01	1	N	Fair.
8	18	72	80	30.01	1	N	Cloudy.
	1	82	81	30.01	2	N	Cloudy, looks black to the N.
	11	76	79	30.02	2	N	Clear, hard rain between 1 and 2.
9	18	77	78	30.03	2	N	Cloudy.
	1	81	81	30.03	2	NNE	A few clouds.
	10	76	79	30.03	2	N	A few flying clouds.
10	18	75	77	30.03	2	N	Fair.
	2	80	81	30.01	1	N	Fair.
	6	76	78	30.01	2	N	Cloudy, rained hard all day.
14	18	76	77	30.01	1	N	Fair.
	2	80	80	30.01	2	N	Fair.
	11	76	79	30.02	1	N	Fair.
15	18	70	75	30.02	1	NW	Fair.
	1	79	79	30.02	2	N	Thin clouds.
16	18	70	76	30.01	1	NNE	Fair.
	1	79	79	30.03	1	NE	Fair.
	11	72	77	30.03	1	N	Fair.
17	18	67	74	30.02	0	——	Fair, much dew.
	2	79	79	30.02	1	N	Fair.
	10	74	78	30.02	1	N	Fair.
18	19	67	75	30.02	0	——	Fair, a great dew.
	2	79	80	30.02	1	NNE	Fair.
	10	74	78	30.02	1	N	Fair.
19	18	67	76	30.02	1	N	Fair.
	2	80	80	30.02	1	NNE	Fair.
	11	76	78	30.02	1	NE	Fair.
20	17	69	76	30.02	0	——	Fair.
	2	80	80	30.02	1	NE	Fair.
21	18	70	77	30.02	1	NW	Fair.
	3	79	80	30.02	1	NE	Fair, about 9 and 10 A.M. the barometer is generally about 30—$\frac{3}{20}$ or $\frac{4}{20}$.
	10	75	80	30.02	1	N	Fair.
22	18	70	78	30.02	1	WNW	Fair, a little dew.
	2	80	81	30.02	1	NNE	Fair.
	8	75	80	30.01	1	N	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Points.	State of the weather, &c.
1776 Dec. 23	17	66	75	30.01	1	N	Fair.
	3	79	82	30.00	1	NE	Fair.
	9	75	80	30.01	1	NNE	Fair.
24	18	68	77	30 01	1	NW	Fair, a little dew.
	½	80	79	30.01	1	NE	Fair.
	11	74	80	30.02	1	N	Fair.
25	19	68	77	30.02	1	NNW	Fair.
	1	81	81	30.01	1	NE	Fair.
26	19	69	78	30.01	0	——	Fair, much dew.
	2½	80	80	30.00	2	N	A few clouds.
	11	75	79	30.02	1	N	Fair.
27	18	66	76	30.00	1	NNW	Fair.
	2	80	80	30.00	1	N	Fair.
	9	76	79	30.01	1	NNE	Cloudy.
28	18	69	77	30.01	1	NW	Fair.
	2	80	81	30.01	1	ENE	Fair.
	11	75	79	30.02	1	NE	Fair.
29	18	69	78	30.02	1	NW	Fair.
	2	79	81	30.02	1	NE	Fair.
	11	75	78	30.03	1	ENE	Fair.
30	18	75	77	30.02	1	N	Cloudy.
	1	79	79	30.02	2	NNE	Fair.
	11	72	79	30.03	2	N	Cloudy.
31	18	75	78	30.03	1	NNE	Cloudy, rained hard in the night.
	1	79	79	30.02	2	NNE	Fair.
	11	72	79	30.02	2	N	A few clouds.
1777 Jan. 1	18	76	78	30.02	2	NE	A few clouds.
	3	80	80	30.02	1	NE	Fair.
2	22	—	77	30.03	1	N	Fair, found my out therm. broke.
3	11	—	77	30.03	1	NE	Fair.
4	18	—	76	30.03	1	N	Fair.
	1	80	82	30.03	2	NE	Fair.
	10	76	76	30.04	1	NNE	Fair.
5	18	74	75	30.03	1	N	Fair.
	1	82	80	30.03	2	N	Cloudy, at 10 barometer at $30\frac{5}{20}$.
6	18	73	75	30.03	1	N	Fair.
	1	82	81	30.03	2	NNE	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the weather, &c.
1777 Jan. 7	10	—	79	30.01	1	NNE	Fair. A different barometer, and 9 miles W. on a rising ground.
	12	—	80	30.01	1	NNE	Fair.
8	20	—	73	30.02	1	NNW	Fair.
	2	82	80	30.01	1	NE	Fair.
	10	74	77	30.02	1	N	Fair.
9	18	70	73	30.01	1	NW	Fair.
	1	81	80	30.02	1	ENE	Fair.
	11	75	77	30.01	1	NW	Fair.
10	18	70	73	30.01	1	NW	Fair.
	2	81	80	30.01	1	E	Fair.
	11	75	77	30 02	0	—	Fair.
11	18	70	73	30.01	1	NW	Fair.
	1	80	79	30.01	1	E	Fair.
12	18	70	72	30.01	0	—	Fair.
	1	82	79	30.00	1	SE	Fair.
	10	76	77	30.01	1	E	Fair.
13	18	72	76	30.00	1	W	Fair.
	5	80	80	30.00	1	SE	Fair.
	10	74	77	30.00	1	SE	Fair.
14	18	70	76	30.01	1	S	Fair.
	3	80	80	30.00	1	E	Fair.
	12	74	77	30.01	1	SE	Fair.
15	18	69	73	30.01	1	NW	Fair.
	2	—	81	30.00	1	E	Fair.
16	1	81	80	30.02	1	E	Fair.
	10	75	78	30.02	1	NE	Fair.
17	18	71	75	30.01	1	NE	Fair.
	7	78	77	30.02	2	NNE	Fair.
19	18	69	73	30.03	1	NW	Fair.
	0	81	78	30.01	1	E	Fair.
	11	77	77	30.01	1	E	Fair.
20	18	70	73	30.00	1	NW	Fair.
	3	81	80	30.00	1	E	Fair.
21	19	70	76	30.01	1	NW	Fair.
	10	74	77	30.01	1	E	Fair.
22	18	68	73	30.00	0	—	Fair.
	1	81	79	30.01	1	NE	Fair.
	10	74	74	30.01	1	NE	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the weather, &c. at Fort St. George.
1777 Jan. 23	18	70	74	30.01	1	NW	Fair.
	2	84	79	30.01	1	N	Fair.
	10	74	76	30.01	1	N	Fair.
24	19	74	76	30.01	1	NE	Cloudy.
	2	84	81	30.01	1	NE	Fair.
	10	78	78	30.03	2	N	Fair.
25	0	84	81	30.03	2	NE	Fair.
	10	78	79	30.03	2	N	Fair.
26	19	76	78	30.03	2	NE	Fair.
	1	84	82	30.03	2	NNE	Fair.
	10	78	79	30 04	1	NE	Fair.
27	19	74	78	30.03	2	N	Cloudy.
	3	82	81	30.02	2	N	A few clouds.
	10	77	77	30.03	2	N	Hazy.
28	18	70	76	30.03	1	NW	Fair.
	2	86	80	30.03	1	NE	Cloudy.
	10	74	—	—	0	—	Fair, in the country about 6 miles W.
29	18	68	—	—	1	NW	Fair.
	10	72	—	—	1	NE	Fair.
30	18	65	—	—	0	—	Fair.
	2	81	—	—	1	ENE	Fair.
	10	75	—	—	0	—	Fair.
31	18	66	—	—	1	NW	Fair.
	2	83	—	—	2	NE	Fair.
	10	74	—	—	1	N	Fair.
Feb. 1	18	70	—	—	1	NW	Fair.
	2	81	—	—	2	NE	Some clouds.
	8	73	—	—	1	NNE	Fair.
2	18	68	—	—	0	—	Fair.
	1	81	—	—	1	NE	Fair.
	10	72	—	—	1	N	Fair.
3	18	65	—	—	1	N	Fair.
5	10	73	—	—	1	NW	Fair.
6	18	67	—	—	1	NW	Fair.
	10	75	—	—	1	NE	Fair.
7	18	67	—	—	0	—	Fair.
	2	82	—	—	1	NE	Fair.
	10	72	—	—	0	—	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. Str.	Winds. Points.	State of the weather, &c.
1777 Feb. 8	18	67	—	—	0	—	Fair.
	2	83	—	—	1	SE	Fair.
	10	74	—	—	1	NE	Fair.
9	18	68	—	—	1	W	Fair.
	2	82	—	—	2	E	Fair.
	10	73	—	—	1	NE	Fair.
10	18	67	—	—	1	NW	Fair.
	2	82	—	—	2	NE	Fair.
	10	75	—	—	1	N	Fair.
11	18	68	—	—	1	NW	Fair.
	2	82	—	—	1	NE	Fair.
	10	72	—	—	1	N	Fair.
12	18	65	—	—	1	NW	Fair.
	0	84	—	—	1	NE	Fair.
	9	69	—	—	0	—	Fair.
13	18	65	—	—	1	SW	Fair.
	1	86	—	—	2	SE	Fair, wind came round by the S.
	10	71	—	—	0	—	Fair.
14	18	64	—	—	1	W	Fair.
	1	86	—	—	1	E	Fair, wind came round by the S.
	10	70	—	—	0	—	Fair.
15	18	63	—	—	1	W	Fair.
	9	71	—	—	0	—	Fair.
16	19	69	—	—	0	—	Fair.
	1	83	—	—	2	ESE	Fair.
	10	70	—	—	0	—	Fair.
17	18	67	—	—	1	W	Fair.
	1	84	—	—	1	ESE	Fair.
	11	70	—	—	0	—	Fair.
18	18	64	—	—	1	W	Fair.
	1	83	—	—	2	ESE	Fair.
	10	72	—	—	1	E	Fair.
19	18	67	—	—	1	W	Fair.
	1	83	—	—	2	SE	Fair.
	10	74	78	30.01	1	E	Fair. In town again.
20	18	67	72	30.01	1	W	Fair.
	10	75	78	30.01	1	E	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Winds. S r.	Winds. Points.	State of the weather, &c.
1777 Feb. 21	18	65	73	30.01	0	——	Fair, every night a great dew.
	$\frac{1}{2}$	85	80	30.02	1	E	Fair.
	$10\frac{1}{2}$	76	78	30.02	1	E	Fair.
22	18	68	75	30.02	1	W	Fair.
	0	85	80	30.03	1	E	Fair.
	11	76	79	30.02	1	E	Fair.
23	18	68	76	30.02	1	W	Fair.
	0	83	80	30.03	2	ENE	Fair.
	10	78	80	30.02	2	E	Fair.
24	18	75	79	30.02	1	W	Some clouds, no dew on the grafs this morn.
	2	84	82	30.01	2	E	Fair.
	9	78	80	30.02	1	E	A little hazy.
25	18	68	78	30.00	0	——	Fair, a little dew.
	5	81	83	30.00	1	SE	Fair.
	12	77	80	30.00	1	SE	Hazy.
26	18	69	77	30.01	0	——	Fair, much dew.
	2	84	82	30.00	1	SE	Hazy, Sun-fhine.
	10	77	80	30.01	1	ESE	A little haze.
27	18	65	74	30.01	1	NW	Fair, much dew.
	1	86	80	30.01	1	E	Fair.
	10	78	80	30.01	1	E	Hazy.
28	18	64	73	30.00	0	——	Fair, much dew.
	23	85	80	30.00	1	SE	Fair.
	9	78	79	30.00	1	ESE	Fair.

177$\frac{6}{7}$	At the end of October.	November,	December,	January,	February,
Fevers, — —	6	8	7	18	14
Liver, — —	7	9	16	8	20
Liver Cough, —	5	7	6	5	6
Liver Flux, —	9	14	18	4	2
Fluxes, moſtly of the belly,	19	18	28	35	16
Fever and Flux, —	2	–	7	2	–
Rheumatiſms, —	29	25	17	32	22
Abdominal obſtructions,	39	23	12	5	8
Dropſy, —	2	1	1	1	2
Epilepſy, —	–	–	5	2	2
Peripneumonia vera,	1	–	–	–	–
Gravel, —	4	3	1	5	6
Cough, pectoral complaints,	2	3	4	4	–
Accidents —	1	5	5	6	5
Diſorders of the eyes,	1	2	2	2	2
Piles, — —	2	3	4	2	–
Ruptures, —	1	2	2	2	–
Nervous caſes, —	1	1	3	2	2
Fiſtula, —	2	1	1	–	–
Rheumat. pains without ſores,	1	–	–	–	–
Venereals, —	42	32	41	46	53
Intermitting fevers, —	–	1	3	–	–
Diabetes, —	–	1	2	–	5
Itch, —	–	2	1	–	–
Surgical patients, —	22	21	20	17	15
Anomalous, —	5	11	9	8	4
Total, —	203	193	215	206	184

XIII. *Experiments upon Air, and the effects of different kinds of Effluvia upon it; made at* York. *By* W. White, *M. D. F. S. A. Communicated by* John Fothergill, *M. D. F. R. S.*

Read Feb. 5, 1778.

THE experiments of which the following are only a part, and which I purpose at my leisure to pursue farther, were originally undertaken with the design of ascertaining the state of the common atmospheric air in and about this city. But in order to form a just idea of this, it seemed to me necessary to find, by exact experiments, the real effects of the different kinds of effluvia upon air, especially such as are in a natural state constantly mixing with the atmosphere, and to the effects of which all respiring animals are constantly exposed; such are those from animal and vegetable substances, and from different kinds of soils. As the result of those inquiries appeared to me not only curious, but in a medical view very interesting, I am in hopes they may be acceptable to the Royal Society.

It may not be amiſs to premiſe a ſhort deſcription of the ſoil and ſituation of this city.

It is for the moſt part built upon a moraſs; this is more particularly the caſe in the part of the city ſituated to the Eaſt of the river, which is much the largeſt. The ſoil to the Weſt of it is more of a ſand or clay. It is divided into two unequal parts by the navigable river Ouze, running from N.W. to S.E. Its ſituation is in the middle of an extenſive vale, well cultivated, and drained in general; nor is it kept very moiſt and unventilated by numerous thick woods. We have no very high grounds near us, but at ſome miles diſtance, eſpecially to the N. and E. are high hills of great extent, called the Wolds. To the South there is a gradual deſcent down to the Humber. Our waters are in general hard: we have one or two ſprings of exceeding pure, ſoft water. Some of our ſprings contain a conſiderable quantity of various neutral ſalts, eſpecially the magneſia and Glauber's ſalt, ſo as to be purgative: we have two or three pretty ſtrongly chalybeate. The higheſt ſtate of the barometer in the three laſt years was 30.58; the loweſt, 28.20. Thermometer in the ſhade, higheſt, 81; loweſt, 8. Having no Ombrometer, ſhall only obſerve in regard to rain, that in 1774 we had 193 days in which more or leſs rain fell; in 1775, 232 days; and in the laſt year, 240.

Besides the navigable river Ouze, we have a brook called the Foss, which, rising about twelve miles Eastwards of the city, runs towards it, and, washing the castle walls, empties itself into the Ouze. This stream, after floods and in winter, overflows a large quantity of land, which in summer and autumn becomes an offensive, stinking morass, almost surrounding the East part of the city. The unhealthfulness of the evaporation from some hundred acres of stinking mud, is farther increased by its being made a receptacle for all kinds of nastiness; in this respect we are more remiss than our ancestors. This has been its state many ages: LELAND thus speaks of it, *Fossa amnis piger, instar stagnantis aquæ collectæ ex pluviâ et terræ uligine, originem habet* &c." In the thirtieth year of EDWARD the third, before the king at York, divers persons were punished for erecting *porcariæ* [hog's-styes] upon the banks of the foss: and in HENRY the fourth's time the throwing in of dung and other nastiness into the foss was forbid under the severe fine of one hundred pounds, as we find in DRAKE's *Eboracum*. This was all done for the preservation of the fish; I wish it was now attended to for a more important purpose. The draining of it has been some time in agitation, the utility of which is obvious.

The

The apparatus ufed in making the following experiments is very fimple; and, though lefs oftentatious, may perhaps be more accurate than more complete inftruments. Firft, a veffel full of water, of a proper fize and figure. Secondly, a common barometer tube of a large bore, fo that an ounce phial full of air, being introduced into it, occupied at a medium 134 decimal parts of an inch; and upon a further addition of an half-ounce phial of nitrous air, 205: this tube is graduated by inches and decimals. Thirdly, glafs funnels, with necks of fuch a fize as to enter the tube.

The air, the fubject of the experiment, was conveyed into the tube, by means of the glafs funnel, under water; the nitrous air is then added to it by the fame method. The fpace occupied by them both, immediately upon mixture, is noted down, as alfo the time by a watch: after ftanding the appointed time (half an hour, except where it is mentioned otherwife) the fpace then occupied is marked down, which being deducted from the firft gives the refult of diminution fought for: for example, an ounce phial of air from a putrid plumb, with the addition of half of nitrous air, took up the fpace of 195 (part of the firft being abforbed by the water in its paffage through it); after half an hour, ftill 195; fo that no diminution following, it was known to be mephitic.

Auguſt 30th, the ſame quantity of the air of my garden, with the nitrous, occupied 205; after half an hour it was diminiſhed to 145, which being deducted gave 60, the ſtate of the air that day; and ſo of the reſt.

The medium ſtate of the air of the atmoſphere, in upwards of two hundred experiments, was 60° or 61°.

EXP. I. Sept. 13th, it was in the worſt ſtate I ever obſerved it, 58°, the barometer being 30.30, thermometer 69°, with a calm, clear ſky, wind S.E. air dry and ſultry, no rain having fallen for above a fortnight; on the ſame day we had a ſlight ſhock of an earthquake.

EXP. II. Sept. 20th, much rain falling, barometer 30.00, thermometer 60°, wind being South, it was 63°.

EXP. III. The next day, Sept. 21, a high wind cleared the air, barometer 29.50, thermometer 52°, the air was 64°. It was the ſame Oct. 5, the wind high and Weſterly. This was the pureſt I ever obſerved.

EXP. IV. I have only obſerved it ſo good as 68° in three inſtances, Auguſt 16, Sept. 20, and 29; theſe were all ſhowery days, with a briſk wind.

EXP. V. As to the influence of the different winds upon one atmoſpheric air, my experiments are as yet too few to aſcertain it. I have generally found it the pureſt

during

during Wefterly winds, and the worft when it blew from the Eafterly points.

EXP. VI. The difference of the air a little way out of the city, from that in the city itfelf, is perceptible enough. Auguft 9th, the air of the city was 59°, beyond the city walls 62°. On the 11th of the fame month, the firft was 60°, the laft 62°.

EXP. VII. Common air being brifkly agitated with water for half an hour, was found to be made worfe. In one experiment it was reduced from 59° to 57°; in another, from 61° to 59°; in a third, from 61° to 57°; in a fourth, from 62° to 58°. Air obtained from glazier's putty by the nitrous acid was meliorated by the fame procefs.

In order to find the effects of animal exhalations upon air, the following experiments were made.

EXP. VIII. The air of my bed at night I found to be 62°, the next morning it was reduced to 58°; this was feveral times repeated. The diminution here will appear very confiderable upon obferving, that it was the effect of the breath, &c. of a fingle perfon, in a large, airy room, the bed-curtains always open, except on the fide facing the window, which is quite open to large gardens, and never fhut with curtains. It fully fhews the unwhole-

fomenefs

ſomeneſs of ſmall rooms, cloſe beds, &c. eſpecially in diſeaſes.

EXP. IX. Some air which I had reſpired as long as could be without manifeſt inconvenience, was by it reduced from 62° to 40°. This illuſtrates the preceding experiment.

EXP. X. A ſmall piece of freſh veal was put into a phial containing eight ounces of common air, and ſuffered to remain therein twenty-four hours: the fleſh was then perfectly ſweet, but the air was much injured, being diminiſhed from 64° to 55. Being left together twenty-four hours longer, the air was reduced to 10°, or rendered nearly mephitic; yet the fleſh was not putrid, only ſmelling rather faint and muſty.

It is evident from hence, that ſomething had eſcaped from the fleſh, whilſt yet void of any putrid ſmell, ſo as to render the air very noxious: I ſuppoſe this effluvia to be pure phlogiſton. Hence it ſeems, that this principle is capable of riſing, *per ſe*, uncombined with the ſaline part of animal bodies, the union of which is ſuppoſed to give the putrid ſmell. It proves Sir JOHN PRINGLE's ſuppoſition, that phlogiſton, when ſingle, is imperceptible to the ſmell; but I think it alſo ſhews it to be peſtilential. In our experiments it was devoid of ſmell, conſequently contained no mixture of volatilized acid; yet it had

had the common property of all putrid effluvia, that of rendering common air noxious.

EXP. XI. Air taken from within a privy was found in feveral experiments to be equally good with the common atmofpheric air. One trial only gave a different refult; here the external air being 62°, that of the privy was only 60°.

The refult of thefe experiments was contrary to my expectation, and I was not fatisfied without making feveral trials. Sir JOHN PRINGLE obferves, that the *fæces humanæ* are, perhaps, in a natural ftate little if at all infectious. Thefe experiments confirm the juftnefs of his fuppofition. The recent *excreta* of a perfon in perfect health are here underftood; in putrid difeafes they muft neceffarily partake of the general ftate of the fyftem, and become very noxious and infectious.

EXP. XII. The following experiments were made to difcover the effects of vegetable effluvia upon air. They were put into a phial of air, containing eight ounces, immediately after being gathered out of my garden; the time of ftanding together half an hour, except in a few cafes particularly noticed.

Flowers of	Ulmaria, diminiſhed it from	63 to 52
	Ten-week Stocks,	63 to 53
	Mignionette,	60 to 54
	Calendula vulgaris,	60 to 54
	French ditto,	60 to 55
	Naſturtium indicum,	60 to 55
	Carnations,	60 to 56
	Tree primroſe,	60 to 56
	Antirrhinum,	60 to 57
Leaves of	Sage	61 to 55
	Thyme,	61 to 56
	Mint (common)	61 to 57
	Ditto (pepper)	61 to 57
	Parſley,	61 to 57

It is evident from theſe experiments, that vegetables, when freſh and vigorous, exhale a noxious matter in conſiderable quantity, which quickly renders common air noxious. This is moſt remarkable in the flowers, next in the leaves, and this in proportion to their firmneſs and texture.

EXP. XIII. In the laſt experiments we have ſaid, that the air only ſtood in contact with the vegetables half an hour; let us ſee here what effect a longer time of ſtanding together may have, *viz.* 16 hours.

Flowers

Flowers of Ulmaria diminiſhed it from	.	60 to 2
Ten-weeks ſtocks,	. .	60 to 1
Leaves of Sage,	. . . .	61 to 9

The vegetables were at the end equally ſweet as when firſt gathered and put into the phial of air.

Theſe facts are very curious, intereſting, and convincing. It is amazing, that vegetables, whilſt freſh and free from the leaſt degree of putreſcency, ſhould have ſuch a noxious tendency as to ſpoil the air, and render it not only uſeleſs but fatal to animal life, and that in ſo ſhort a time.

We have here a ſtriking example of the neceſſity of faithful experiments: by them alone we can add certainty to ſcience, and develop nature in her moſt ſecret and abſtruſe operations; and as ſhe is unchangeable in herſelf, every diſcovery extorted from her is immutable. For want of attention to this laborious but ſole method of coming at truth, it is a pretty general opinion in the world, that even rotten vegetables are little noxious: and a late author, whom I only mention becauſe his book is pretty generally read, in a chapter upon putrid fevers and infection, expreſsly ſays: "The effluvia of rotten "vegetable matters have little effect in contaminating "the

"the air; from ſome experiments it appears, that they "poſſeſs rather an antiſeptic virtue."

We know, however, by fatal experience, that both animal and vegetable ſubſtances, when in a corrupted ſtate, are the obvious ſources of the moſt dreadful and alarming diſeaſes, from the mildeſt putrid fever up to the plague itſelf. Sir JOHN PRINGLE gives us an inſtance of the jail or hoſpital-fever, cauſed by the infection of a gangrened limb. A dreadful fever was cauſed at Venice by a quantity of corrupted fiſh; and at Delft by putrid cabbages and other vegetables. Many inſtances of this kind may be brought, by which countries have been almoſt depopulated.

But it is no wonder that animal and vegetable matter, when in a ſtate of abſolute corruption, ſhould be pregnant with ſuch dreadful effects. Inſtinct leads us to fly from the danger when we perceive the cadaverous ſmell.

The ninth, tenth, twelfth, and thirteenth experiments demonſtrate, that our ſenſes are by no means capable of diſtinguiſhing infection, nor, by warning us of the danger, of leading us to avoid it. They ſhew, that both animal and vegetable matter, when perfectly freſh, ſweet, and devoid of putreſcency, exhales ſomewhat of a very noxious nature, inducing a putrid ſtate in the living body, which proves deſtructive to animal life.

Hence I do not heſitate to declare, that in jails, hoſpitals, and other crowded places, we ought not by any means to eſtimate their wholeſomeneſs by the abſence of diſagreeable ſmells alone. The principle of diſeaſe may lurk therein unperceived by our limited ſenſes. The method uſed in theſe experiments is the only true one by which we may judge with ſome degree of ſafety.

The crowding together of a number of men in camps, hoſpitals, jails, ſick rooms, &c. will preſently generate a moſt malignant and infectious fever; and in a very ſhort time, eſpecially if the place be cloſe, unventilated, and the weather hot, the moſt fatal effects will follow. Of this we have a moſt remarkable example in the affair at Calcutta.

Mr. HOLWELL and one hundred and forty-five more people, in perfect health, were, by order of the vice-roy, ſhut up in a place of confinement, at ſeven o'clock in the evening. The place was 18 feet by 18 feet, containing 324 ſquare feet, ſo that there was a ſquare for each perſon of $26\frac{1}{2}$ inches by 12 inches, which was ſufficient to hold them without preſſing violently upon each other. The weather was extremely ſultry, and the place of confinement having only one ſmall grated window to the Weſt, the air within could neither circulate nor be changed. In leſs than an hour after their being incloſed, many of the

unhappy people were feized with violent difficulty of breathing, feveral were delirious, the place was filled with incoherent ravings, exclamations, and cries of diftrefs: the cry of *water*, *water*, was predominant; it was handed to them by the centinels, but had no effect in eafing their thirft. Before eleven o'clock many were fuffocated, or died violently delirious. By twelve o'clock all that furvived, except a few at the grate, were to the higheft degree phrenetic and outrageous; they now found no relief from water, but air could not be procured. Soon after, thofe at the grate grew fo infenfible, that we have no account of what happened till they were releafed from their confinement at fix o'clock next morning. Such was the effects of animal effluvia in a clofe and unventilated place in the fpace of eleven hours, that, out of one hundred and forty-fix fouls, no more than twenty-three came out alive, and thofe in a high, putrid fever, of which, however, by frefh air, &c. they gradually recovered.

In all confined places, in proportion to their airynefs, we find more or lefs of this. In hofpitals, though the wards may give no marks of it by any apparent dirtynefs or difagreeable fmell, we may obferve its effects; difeafes which ufually admit in private practice of an eafy cure, are often very tedious, and apt to affume anomalous

fymptoms.

ſymptoms. Healthy perſons, admitted for the cure of recent wounds and other accidents, ſoon become pale, loſe their appetite, and are generally diſcharged weak and emaciated, but ſoon recover by the benefit of freſh air. In ſome hoſpitals the cure of a compound fracture is rarely ſeen; in private practice, and a pure air, ſuch caſes ſeldom fail. Such and many more are the effects of bad air, which, though not virulent enough to cauſe a putrid fever in its more malignant form, is yet ſufficient to excite it to ſuch a degree as to undermine the conſtitutions of the patients, and render the diſorders, for which they were admitted, anomalous, tedious, and fatal.

We have demonſtrated, that the effluvia of vegetables, even whilſt perfectly ſweet and freſh, are equally poiſonous with thoſe from animal ſubſtances. The vegetables were ſeparated from their parent plant, conſequently not in a growing or vegetating ſtate.

EXP. XIV. Being deſirous of finding the effects of effluvia from ripe fruit upon air, ſix ripe gooſeberries ſliced were incloſed ſixteen hours in a phial with eight ounces of common air: the air being then put to the teſt, was found to be diminiſhed from 62° to 40°.

Hence it appears, that fresh fruit have, in common with other vegetable matters, a great power in polluting the air, and rendering it noxious.

EXP. XV. In order to find whether any part of the pernicious effects of vegetables upon air in the twelfth experiment might be owing to their odorous particles, the following experiments were made. In each, the quantity of inclosed air was eight ounces; the time of standing together sixteen hours.

10 grains of Musk diminished it from		63 to 62
Half a drachm of	Camphor,	63 to 62
	Assa-fœtida,	62 to 62
	Saffron,	62 to 62
	Opium,	60 to 58
	Vol. Sal. Ammoniac.	60 to 58

Musk and camphire were selected as examples of essential oils; the first of the animal, the second of the vegetable class. The assa-fœtida as an instance of the fœtid odour; opium of the narcotic. Saffron, from its mode of preparation, is incapable of corruption whilst kept dry, and could give nothing but pure odour. The volatile salt was an example of the volatile odour.

Hence we find, that pure odour has little, if any, effect in polluting the air. For where any difference occurred,

occurred, it is ſo ſmall, that I attribute it rather to ſome little inaccurracy in conducting the experiment. Nor did I think it neceſſary to repeat the trials, being ſatisfied that their poiſonous effects in the twelfth and thirteenth experiments were not in the leaſt owing to their odour, but to their organized ſtructure, tending to diſſolution from the time they are deprived of nouriſhment; ſuch is the perfect agreement between vegetable and animal ſubſtances.

It is demonſtrable from hence, that the filling of rooms with noſe-gays and bunches of flowers is by no means a ſafe practice, eſpecially in cloſe rooms or ſick chambers; their effluvia are of ſo noxious a nature as quickly to render the air unfit for the purpoſes of reſpiration, and cannot fail of having bad effects upon ſick and valetudinary people in particular.

But it is alſo evident, that the odorous parts of vegetables, when ſeparated by art from the putreſcent, are by no means hurtful. Hence, except in particular conſtitutions, or in caſes where their ſtimulus may be hurtful, they may be ſafely uſed as agreeable odours, and to obviate the ſmell in ſick rooms, &c. The volatile alcali, as Sir JOHN PRINGLE obſerves, appears in this view perfectly innocent.

What is here ſaid is underſtood of plants gathered and ſeparated from the roots. Dr. PRIESTLEY diſcovered a different property in them when in a vigorous, growing ſtate, they then abſorb from the atmoſphere; but this ceaſes with their life, they then exhale putridity, and haſten to diſſolution.

We come next to another, not leſs curious and important, part of our experiments; the effects of the effluvia from moiſt, marſhy, and other kinds of ſoils, upon air.

This ſubject, as particularly connected with our art, regarding the endemic diſeaſes of different countries, and a plentiful ſource of the moſt dangerous diſeaſes, has much employed the attention of phyſicians and philoſophers. The nature of *miaſmata* is, as far as I know, as yet but imperfectly underſtood; hence general unanimity of opinion is not to be expected: nor can a perfect coincidence in the reſult of experiments be at once attained, eſpecially if made by perſons with different views, and under the influence of different ideas and perhaps prejudices.

In order to attain truth, we muſt take faithful experiments, made with ſedulous obſervation, for our guides; we ſhall find them to reflect mutual light and truth upon each other. Thus we gradually lift up the veil of nature, and become acquainted with her genuine form; nor let us imagine

imagine her ways to be infcrutable; it was merely through ignorance that the ancient Egyptians covered Ifis with a veil, making her declare that no mortal could lift it up.

EXP. XVI. The air over the river Ouze was conftantly purer than that of my garden by two or three degrees.

EXP. XVII. The fame was obfervable in the air over the fofs. This was at a time when, in confequence of floods, the current was pretty rapid, all the mud and marfhy ground being covered to a confiderable height with water.

I next tried what effect the fame waters might have upon air, when confined together. Two ounces of the water was put into an eight-ounce phial, fo that there were fix ounces of air; being corked up, they were fuffered to ftand together fixteen hours.

EXP. XVIII. The air from the Ouze water was equally good as at firft; and this in feveral experiments.

EXP. XIX. The fame was the refult in the fofs water. It was perfectly free from mud, yet not fo clear as the river water, and had fome of the *lens paluftris* fwimming in it.

Hence we find, that the air was not any way polluted by ftanding over the furface of water. Perhaps if longer time had been allowed in the nineteenth experiment, the

the *lens palustris* might have grown putrid, and hurt the air.

EXP. XX. Some of the foss water was next tried; so foul as to deposit a muddy sediment upon standing.

In one experiment the air was reduced from 62° to 58°; in another, from 62° to 57°; in a third, from 60° to 56°.

EXP. XXI. It has been observed by those physicians who have had the most opportunities of being acquainted with the diseases peculiar to low, stagnant, and fenny situations, that they seldom begin to appear until the water is so far evaporated, that the black and slimy mud begins to appear. In order to know this, the following experiments were made.

Two ounces of the black stinking mud of the foss was put into the eight-ounce phial of air; being closed, they were suffered to remain together twelve hours. The air in twelve trials being put to the test, the results were as follow.

In seven experiments the air was reduced from 62° to 34°; in three, from 62° to 36°; in two others, from 60° to 35°.

These are convincing examples of the noxious effects of the effluvia from putrid bogs and marshes. Although I was perfectly convinced of this by the authorities of

Sir

Sir JOHN PRINGLE[a], Dr. LIND[b], CLEGHORN[c], &c. as also by my own reflections and obſervations; yet I was pleaſed to prove it in my own ſtudy, and to be able to bring it to the evidence of the ſenſes. It is not a little ſatisfactory to prove, by modern experiments, the truth of obſervations made in remote antiquity.

A late ingenious author[d], from experiments made with raw fleſh ſuſpended over bogs, finding that the effluvia rather retarded than haſtened putrefaction, ventures to declare, that he is "even inclined to doubt of "their (marſh effluvia) inſalubrity in any reſpect."

But it ſhould be remembered, that an atmoſphere, already ſaturated with putrid matter, was by no means a proper *menſtruum* for taking off and ſuſpending a farther portion of putreſcent matter.

And we ought to conſider, that *miaſmata, æquè ac medicamenta, non agunt in cadaver*. As all impreſſions upon our ſyſtem are made through the medium of the nerves, no motions can be excited, nor farther *inertia liquidi nervoſi* be produced, in bodies diveſted of nervous energy. My experiments prove, that marſh effluvia are poiſonous to a living animal; yet they may probably

(a) Diſeaſes of the Army, 8vo.
(b) Eſſay on the Diſeaſes incidental to Europeans in hot Climes.
(c) Diſeaſes of Minorca.
(d) Experimental Enquiry into the Cauſes of putrid Diſeaſes.

bably act as an antiſeptic upon the dead one. Fixed air is a powerful antiſeptic in the one, but is deadly to the other. Nitrous air preſerves all fleſh from corruption after death; yet let any living animal but once breathe in it, and it inſtantly expires. Some of our bogs have the ſingular property of preſerving dead bodies not only ſweet but pliable for many years; but we are certain they are at the ſame time deadly to living animals.

EXP. XXII. A fourth part of an eight-ounce phial was filled with the ſame mud as in the laſt experiment, but ſo much dried in the ſun as to be eaſily rubbed into a powder, the reſt being air; after being corked, they were ſet by for twenty-four hours, and in the interim frequently agitated. The air being at the end put to the teſt was ſcarcely altered, the greateſt diminution in ſeveral different experiments was only from 62° to 60°. So that the air was yet quite good, although they ſtood double the time of that in the laſt experiment.

Hence it is evident, that bogs and marſhy grounds, when dry, or perfectly drained of their moiſture, become healthy, and emit no noxious exhalations.

This illuſtrates the obſervation, that ſuch ſituations are not liable to produce their peculiar diſeaſes during the dry ſeaſons, or after being well drained. And it is obſerved, that in the moſt unhealthy of our ſettlements

in Africa, the Eaſt and Weſt Indies, the inhabitants are at ſuch times healthy. But when the wet ſeaſons begin, the ſcene is reverſed; the air immediately becomes vitiated, polluted, and deſtructive; putrid fevers ariſe, and ſpread deſtruction over the country. The ingenious Mr. IVES gives a dreadful inſtance of this, and of the diabolical revenge of the Arabs, when they think themſelves injured by the Turks at Baſſora: they, by breaking down the banks of the river near that city, lay all its environs under water. After the water is nearly evaporated, the mud and other impurities corrupting, pollute the air to ſuch a degree as to cauſe a moſt mortal fever in that populous city. This was the caſe when Mr. IVES was there: of this fever fourteen thouſand ſouls periſhed; and of the Europeans ſettled there only three eſcaped with life: a moſt horrid mode of revenge, and a dreadful example of the deadly effects of marſhes and ſtagnant waters in hot climates. Let us ſee if we can prove this by actual experiments.

EXP. XXIII. To the ſame powdered mud uſed in the laſt experiment, was added as much water as was required to bring it to the ſame conſiſtency with that in the twenty-firſt experiment. This being incloſed with ſix ounces of air as before, ſtood twenty-four hours.

The

The air had then contracted a noifome fmell, like a new-cleaned ditch, and was diminifhed from 62° to 49°. Several experiments gave the fame refults: on ftanding longer, it was diminifhed from 62° to 29°.

This experiment proves and illuftrates the effects of moifture co-operating with unhealthy foils in producing their pernicious effects. Moifture to a certain degree is neceffary to every kind of fermentation; hence I fufpect, that by the falling of a certain quantity of rain upon marfhy grounds, a fermentation immediately commences in the putrid foil, a quantity of vitiated particles are fet at liberty, by which the air is polluted. The degree of fermentation is influenced by the degree of heat, and the greater or lefs quantity of moifture.

EXP. XXIV. To the mud ufed in the laft experiment, fo much more water was added as to dilute it fo that, upon fubfiding, a confiderable height of water fwam above it; it was confined with the air, and ftood as in the laft experiment.

The air being then tried by the teft, it was in no inftance found farther diminifhed than from 62° to 56°.

This experiment was made with a view of difcovering the effect of marfhes and bogs when laid under water; and we find that their danger is in a great meafure obviated by it: fo that the putrid fermentation is either prevented

prevented by too much moiſture, or the effluvia are abſorbed in paſſing through the ſuperincumbent bed of water: perhaps the cold generated by evaporation may have ſome effect.

This fully proves the propriety of Sir JOHN PRINGLE's remark, where, in giving cautions for avoiding diſeaſes ariſing from putrid air, he ſays, "As for cantonments in "marſhy grounds, if the troops muſt remain there in "the dangerous ſeaſon, it will be better to float the fields "entirely, than to leave them half dry; for the ſhal-"lower the water the more it will corrupt, and the eva-"poration will be greater in proportion." How beautifully is this illuſtrated by the twenty-firſt, twenty-ſecond, twenty-third, and twenty-fourth experiments! An inſtance of the perfect agreement of faithful obſervation with truth and nature.

EXP. XXV. Two ounces of dirt ſwept from the ſtreets were incloſed in the phial as before; after ſtanding together twenty-four hours, the air was found to be diminiſhed conſiderably, from 62° to 50°.

Hence it appears how well the magiſtrates conſult the health of the inhabitants, as well as the neatneſs of cities and large towns, by enforcing due attention to the cleaning and paving of the ſtreets in their reſpective diſtricts.

EXP. XXVI. The ſame quantity of loamy, vegetable earth, out of my garden, and brought to the conſiſtence of thick mud by addition of water, was next tried. The air was found but little worſe; in one inſtance only diminiſhed from 59° to 55°; in another, from 64° to 61°.

It is probable from hence, that fine loamy vegetable earth contains little putreſcent matter, as it gives little noxious effluvia. The addition of animal and other kinds of manures will much vary their effects in this reſpect.

EXP. XXVII. A maſs of the ſame conſiſtence was formed of pure clay and water, the other circumſtances of the experiment being the ſame. The air was not found the worſe by it in ſix trials: in one there was only the ſmall difference of 62° to 61°, certainly the reſult of ſome ſlight inaccuracy.

So that the pure clay ſoils appear to be favourable to health; they emit no kind of ſeptic or noxious effluvia.

EXP. XXVIII. Wet ſand was tried in the ſame manner, and found to have no noxious effect upon air: from which it may be concluded, that the general notion of the ſalubrity of ſandy ſoils is founded on truth.

I ſhall at preſent conclude with recapitulating a few inferences, which ſeem to be proved by the preceding experiments.

1. The atmoſpheric air is rendered worſe by a long continuance of dry weather.

It

2. It is purified by rains and winds, eſpecially Weſterly ones.

3. It is conſiderably worſe in cities and large towns, than in the country, even at a ſmall diſtance.

6. It is quickly poiſoned by the effluvia from animal bodies, even whilſt perfectly ſweet and free from putridity.

7. Vegetable matters, when not in a growing ſtate, have a ſimilar effect, and in a degree equally powerful.

8. And this is not any ways owing to their aroma or odorous parts.

9. Phlogiſton riſes alone.

10. Phlogiſton is imperceptible to the ſmell, *per ſe*.

11. Phlogiſton is, *per ſe*, peſtilential.

12. The abſence of diſagreeable ſmells is by no means a criterion of the healthful ſtate of jails, hoſpitals, &c. or of their freedom from infection.

13. Mere odour does not injure the air, nor do volatile alcalies.

14. The air is generally pure over waters.

15. The air is greatly injured by the effluvia from the thick mud of bogs and marſhes.

16. But this is much obviated by laying them under water.

17. Air is not hurt by ſuch mud when perfectly dry.

18. Air is alſo infected by the dirt of the ſtreets.

19. Pure loamy vegetable earth has little of ſuch effect.

20. Air is not at all polluted by pure clay ſoils.

21. Nor by thoſe of pure ſand.

XIV. *An Account of the Earthquake which was felt at* Mancheſter *and other Places, on the 14th Day of* September, 1777. *In a Letter from Mr.* Thomas Henry, *F. R. S. to* William Watſon, *M. D. F. R. S.*

SIR, Mancheſter, October 21, 1777.

Read Feb. 19, 1778.

THOUGH the ſhock of an earthquake which was felt on Sunday the 14th of September, in this and ſome of the neighbouring counties, was by no means equal to thoſe terrible concuſſions which ſome foreign countries have at times experienced; yet as it appears to have been at leaſt as violent as any that has happened in this iſland for many years, I thought a particular account of it might not be unacceptable to you, eſpecially as ſome circumſtances attending it ſeem to be connected with a branch of natural philoſophy, for the elucidation of which mankind are much obliged to your induſtrious and ingenious reſearches.

On the morning of the day on which the earthquake happened, I was confined to my bed beyond my uſual hour by a head-ach, with which I am generally troubled

 previous

previous to any ſtorms or conſiderable changes in the atmoſphere. About five minutes before eleven o'clock, I was alarmed by a noiſe which ſeemed as if it might have proceeded from a large bale of goods thrown down on a boarded floor below ſtairs: the houſe ſhook. I called out to my wife, who was in an adjoining cloſet, to know what could have fallen; when inſtantly I was aſtoniſhed by ſuch a rattling noiſe at the North-eaſt corner of the houſe, that I cried out that a part of the houſe (which had been built within theſe few years, and was not ſo firmly connected with the old part as it ſhould have been) was fallen; and in this opinion I was immediately confirmed by a third and more violent craſh, reſembling the tumbling down of a large and lofty wall. Each of theſe noiſes was ſucceeded by a ſeparate concuſſion.

Theſe events muſt have taken up the ſpace of at leaſt half a minute. During that time I got out of bed, and putting on my coat and waiſtcoat, ran to a window which commanded a view of one ſide of the ſuſpected building, and to my great ſurprize found it ſtanding. I then went to a window at the front of the houſe, where I alſo found every thing ſafe; and on being informed by ſeveral people, who had fled affrighted into the ſtreets, that their houſes and furniture had been violently ſhaken, I con-

cluded the difturbance muft have been occafioned by an earthquake.

I had now time to make inquiry how my wife had been affected; for my mind had been hitherto filled with anxious fears for the fafety of my two youngeft children, who were in that part of the houfe where I had apprehended the danger to be. The dimenfions of the clofet in which fhe ftood were three yards by two. At the North-eaft corner, on the outfide of the wall, is a leaden fpout, which communicating with a wooden one conveying water from a lower building, difcharges it, without coming into contact, into a leaden ciftern, from whence a fmall pipe defcends into the cellar. At a confiderable height above thefe, another leaden fpout proceeds obliquely from the Northern along the Eaftern fide of the houfe, collecting the water from the whole furface of the roof. From this quarter the noife, which was heard before the two laft concuffions, feemed to have proceeded.

My wife informed me, that at the inftant of the fecond explofion fhe had received a very fmart ftroke on the top of her head, and, imagining that fomething had fallen off a fhelf, looked down on the floor and perceived it heaving under her, but could fee nothing that could have given the blow. Lifting up her eyes fhe faw her china and

every

every thing in the clofet dancing on the fhelves; and, during the third fhock, the vibration of the walls was fo great that fhe expected they would have fallen upon her. A pain, attended with a degree of ftupor, remained in the part of her head which had been affected, for feveral hours after. Several other perfons likewife received ftrokes fimilar to electrical ftrokes in different parts of their bodies.

In the churches, it being in the time of divine fervice, the greateft confufion and terror were occafioned. The congregations, fufpecting that either the galleries or the roofs were falling in, endeavoured to efcape with the utmoft precipitation. Several people were thrown down and trampled on, and fome few had their limbs broken. Nor is it to be wondered at that they were fo terrified, as the pillars and walls evidently tottered, and the motion was fo great as to toll the bells in the Collegiate and St. Mary's churches. My fons, who were at the latter, affured me on their return, that they heard the bell twice during the laft fhock, and the facts are befides well authenticated by variety of evidence.

The alarm was equally great in moft of the places of worfhip in this town, except at St. Paul's church, which is a low building at the North-eaft fide of the town, without a fteeple, and has a common fhore running under it.

How

How far these circumstances may have contributed to render the concussion less sensibly perceived there, I do not pretend to determine.

All the neighbouring towns were affected in a similar manner; but very considerable differences were observed in different parts of the same towns. At Blackley, a small village about three miles from this place, the shock was violent in the episcopal church, though very moderate in the dissenting chapel, situated not above three hundred yards from the other. The latter is a very low structure, stands at the foot of a hill, and has no leaden spouts to convey the water from the roof. At a house, about one hundred yards beyond this, placed on an eminence, a servant, stooping at some little distance from a chest of drawers which stood up to the wall, received so severe a blow from it as to strike her to the ground.

The water in many places was agitated. The passengers in the duke of Bridgewater's boat, who were on the canal, did not perceive any change; but the steersman recollects, that the vessel was suddenly stopped at that time, which he could not then account for.

The noise was particularly loud in those houses which were furnished with conductors; and, as far as I have been able to collect, it was loudest in those parts of the houses where the conductors were fixed.

Many

Many people complained, for ſeveral days after, of nervous pains and hyſteric affections, and of ſenſations ſimilar to thoſe of perſons who have been ſtrongly electrified. Perhaps the fright might have contributed to have produced ſome of theſe effects. For my own part, my head-ach, which ſeldom leaves me before evening, was intirely and immediately removed. A report prevailed, and it was poſitively aſſerted, that a boy at Rochdale, who had been long deaf, had recovered his hearing at the inſtant of the earthquake; but, upon the moſt ſtrict inquiry, the fact does not appear to be ſufficiently authenticated.

Different people in the ſame rooms were affected in various degrees, and felt the ſhock more or leſs violently. Neither the vibration nor noiſe were perceived by moſt perſons who were travelling on the roads or walking in the ſtreets. Yet others, on looking at the houſes, perceived a great undulatory motion in them. Thoſe who ſtood on moſs or looſe garden ground felt it heave under them very perceptibly; and others, who ſat or lay upon the ground, were ſo ſhocked as to be thrown forcibly out of the poſition they were in.

To myſelf and ſeveral others, who obſerved the progreſs of this phenomenon coolly, three ſhocks were very

clearly diſtinguiſhable. Some perſons were ſenſible of two, and ſome of one only.

The motion of the earthquake, at leaſt of a ruſhing wind which attended it, was from South-weſt to North-eaſt. It was felt at York, Lancaſter, Liverpool, Cheſter, Birmingham, Derby, and Gainſborough; and within this circuit, the diameter of which muſt be 130 or 140 miles, with greateſt violence in this neighbourhood, which appears to have been the center of it.

In Derbyſhire, through a great part of which county I have ſince travelled, the ſhock was ſtrong on the Weſtern, and weak on the Eaſtern ſide of the Peak. I cannot find that any of the mines were injured by it, though it had been reported that ſome of the ſoughs had fallen in. Nor does it appear, that in the great extent of country, which was thus violently agitated, any more material damage was ſuffered than the throwing down ſome chimnies. Praiſed be that kind, ſuper-intending Providence, who rides on the whirlwind, and directs the ſtorm; and who graciouſly put a period to this awful and tremendous ſcene, when we were apparently on the brink of deſtruction!

To you, SIR, who have ſo intimate a knowledge of electrical ſubjects, it would be impertinent to make any obſervations on the above facts. Perhaps many of them

may tend to confirm Dr. STUKELEY'S theory of earthquakes being occaſioned by the accumulation and diſcharge of the electrical fluid: yet I cannot but obſerve, that the ſtate of the atmoſphere and of the ſeaſon ſeems to have differed in many points from that which he deſcribes as preceding the earthquakes in the years 1749 and 1750.

Dr. STUKELEY ſays, that for four or five months the weather had been warm to an extraordinary degree, the wind generally South and South-weſt without rain. That in the marſhy parts of Lincolnſhire the drought had been ſo great on the ſurface of the earth, that the inhabitants had been obliged to drive their cattle many miles to water. That before the London earthquakes, vegetation was as forward in February as it uſually is in April. That the *aurora borealis* was frequent, unuſual in its colours, and even removed to the South; and that the whole year had been remarkable for fire-balls, lightning, and coruſcations.

In the preſent year the ſpring and ſummer had been in general remarkably cold and unſeaſonable, the wind varying from North-weſt by Weſt to South-eaſt by South; the latter of which commonly brought rain. During the latter end of the month of May, and part of June, the weather was exceedingly dry, and very ſharp froſts deſtroyed

deftroyed moft of the early fruit; particularly one in the middle of June was fo fevere as to kill whole fields of potatoes; an inftance fcarcely ever known at that feafon. In July the ground was refrefhed for a fortnight with frequent and plentiful fhowers, fucçeeded by about an equal period of dry and warm weather. Throughout moft of the month of Auguft the rains were violent and the air cold. Vegetation was backward, and all kinds of fruit crude and infipid. On the fifth of September the weather became warm and ferene, and continued fo with an Eafterly wind, except on the ninth, when fome fhowers fell, till the day of the earthquake, and for fome days after. Vegetation now became more quick. An electrical machine worked with uncommon vigour the day before the earthquake; but was obferved to act as weakly two days earlier. During the fummer I do not recollect above two thunder-ftorms; nor was the *aurora borealis* by any means frequent. A fire-ball was obferved about two months before; and a water-fpout fell on the 23d of July, near Huddersfield, in the Weft-riding of Yorkfhire, which did confiderable damage to the country[(a)].

The

(*a*) It is Father BECCARIA's opinion, that in a thunder-ftorm the clouds ferve as conductors to convey the electric fluid from thofe places of the earth which

The morning on which the earthquake happened was clear and ſerene. The air was ſo far from being ſultry, that ſome perſons who rode out early in the morning complained of the coldneſs of it. The wind was Eaſterly. At the inſtant of the ſhock it is ſaid to have veered to the Weſt, and to have immediately returned to its former ſtation. My barometer had riſen in the night. When I obſerved it, about fifteen minutes after the earthquake, it ſtood at thirty inches, and it continued to riſe all the day. One gentleman, who had marked the height of the quickſilver in his barometer that morning, obſerved that it had fallen a few lines at the ſhock, but it ſoon roſe again to the ſame place. The thermometer at noon ſtood at 63°.

No cloud, except a few ſcattered white ones, ſuch as our atmoſphere is ſeldom free from, was obſervable either before or after the concluſion, and no rain was diſcharged either on that or ſeveral following days.

which are overloaded with it to thoſe which are exhauſted of it. In the ſummer of the preſent year, while the Southern counties were deluged with rain, this part of the kingdom was thirſting for want of it. And afterwards, while long continued heavy rains impeded the ripening of the corn, and threatened deſtruction to the harveſt in this country, I am informed, that the counties in the neighbourhood of London enjoyed a clear ſky and fine weather. I am ignorant what was the ſtate of the atmoſphere in the South in the month of September.

On the 20th, 21ſt, and 22d of September much rain fell, attended with thunder and lightning. The ſtorm was particularly violent on the 21ſt in the neighbourhood of Rochdale, twelve miles from hence; and early on the morning of the 22d the whole hemiſphere appeared, from this place, to be involved in one general blaze.

Should this account appear to you ſufficiently intereſting to be communicated to the Royal Society, I ſhall be obliged to you if you will introduce it when they meet. I have given a plain but authentic narrative of facts, and have avoided drawing any inferences from them, conſcious of my own inability to inveſtigate ſo obſcure and intricate a ſubject.

XV. *Sundry Papers relative to an Accident from Lightning at* Purfleet, May 15, 1777.

1. *A Letter from Mr.* Boddington, *Secretary to the Board of Ordnance, to Dr.* Horſley, *Sec. R. S. with Two Encloſures from Mr.* Nickſon, *Store-keeper at* Purfleet, *giving an Account of the Accident.*

TO DR. HORSLEY, SEC. R. S.

Office of Ordnance,
May 31, 1777.

SIR,

I AM directed by the lieutenant-general and the reſt of the principal officers of the Ordnance, to tranſmit to you the copies of the reports and plan received from Purfleet, on occaſion of ſome damage done by lightning; which reports and plan they deſire you will pleaſe to lay before the Royal Society.

I am, &c.

JOHN BODDINGTON.

TO

TO SIR CHARLES FREDERICK, KNT. OF THE BATH.

Purfleet,
May 16, 1777.

HONOURABLE SIR,

YESTERDAY afternoon we had much rain and diſtant thunder; but at ſix a very heavy cloud, in paſſing over the houſe, preſented us with part of its contents, which ſtruck the North-eaſt corner of the houſe on one of the cramps that held the copeing ſtones together, forced off about a ſquare foot of that ſtone and one brick, and has diſplaced about a cube foot of brick-work underneath. It has not been yet diſcovered that any of the conductors have acted during the paſſage of that cloud, although the flaſh and report were both very great. One of my ſervants was out of doors by the coach-houſe at the ſame time, and narrowly eſcaped falling by the ſtrength of the flaſh: the others were in the houſe, but were much frightened. My ſon ſays, that there is a dent in the cramp, on which the lightning fell, and I intend to preſerve it as a curioſity. If the conductor on the houſe has acted, it is imperceptible as I am informed. I thought this account would be acceptable to you from,

Honourable ſir, &c.

EDWARD NICKSON.

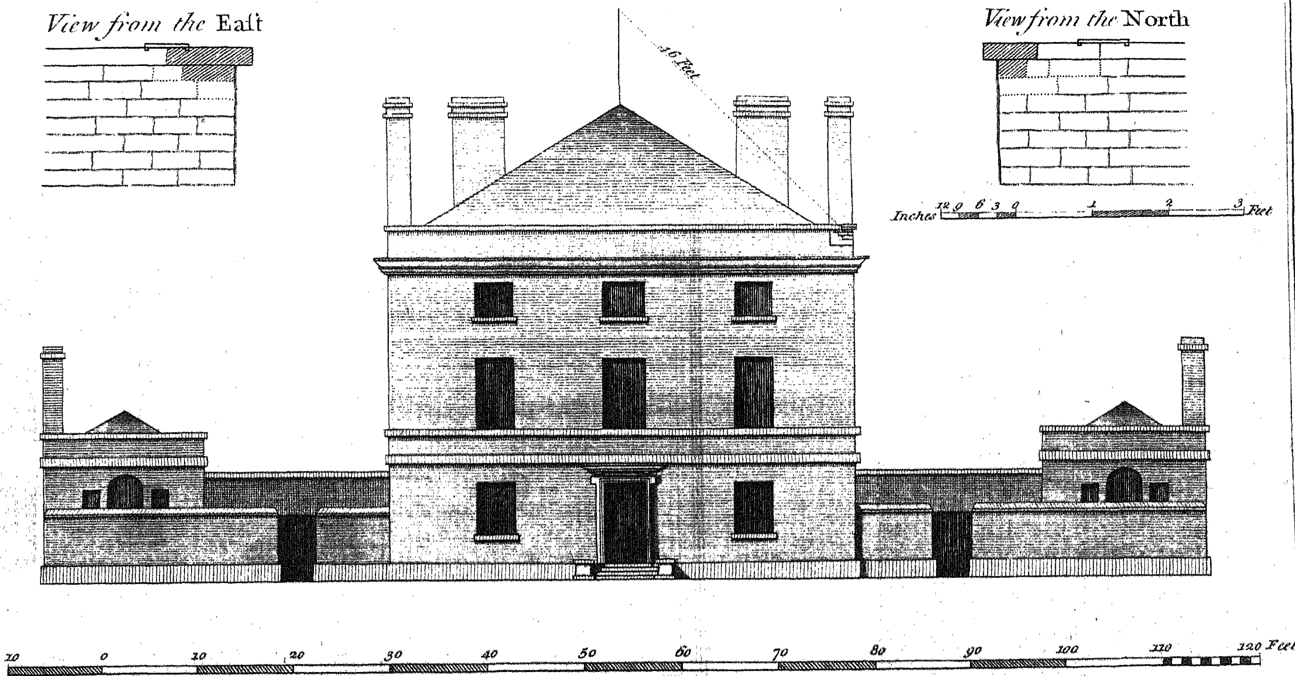
TO

TO THE RIGHT HONOURABLE LORD AMHERST, LIEUTENANT-GENERAL OF HIS MAJESTY'S ORDNANCE, &c.

Office of Ordnance,
Purfleet, May 20, 1777.

MY LORD,

IN obedience to your lordſhip's commands of the 19th inſtant, directing me to report concerning the accident that happened to the Board's houſe by lightning on the 15th, I beg leave to acquaint your lordſhip, that on that day there had been much rain and diſtant thunder; but, about ſix o'clock in the afternoon, a very heavy cloud hung over the houſe for ſome time, which I looked at from the back-parlour window, and it being quite calm, made me ſuſpect, that ſome of our conductors might find employment from it. I had not been long at the window before a violent flaſh of lightning and clap of thunder came together; and, as ſoon as the rain would permit any body to move about, one of the labourers brought me ſome pieces of ſtone and a brick, which were ſtruck off from the copeing on the parapet-wall of the building from the North-eaſt corner. On my ſon's coming home, I deſired him to go and view the top of the houſe. On his return he told me, that the lightning had ſtruck one of the cramps that hold the copeing together, and had made a dent in the lead of the cramp, and



East Front of the Board house at Purfleet.

Basire Sc.

the ſtone adjoining to it, as if ſtruck by a muſquet-ball; that the quantity of ſtone thrown down might amount to about a ſquare foot; and that it had diſturbed about a cube foot of brick-work underneath: and, according to your lordſhip's commands, the diſtance from the point of the conductor on the houſe to the part ſtruck has been meaſured by him this day, and amounts to forty-ſix feet.

All the conductors at this place are pointed, and it has not yet been diſcovered that any of them have acted on this occaſion.

If your lordſhip ſhould want any farther explanation, my ſon waits on you with a ſmall drawing of the elevation of the Eaſt-front of the houſe, and of the part ſtruck, which I hope will be ſatisfactory to your lordſhip and to the honourable board.

I am, &c.

EDWARD NICKSON.

2. *The Report of the Committee appointed by the Royal Society, for examining the Effect of Lightning, May* 15, 1777, *on the Parapet-wall of the House of the Board of Ordnance, at* Purfleet *in* Essex.

THE iron cramp on which the lightning fell was cemented into the copeing stones on the parapet-wall, near the North-east corner, with lead: and on that lead, at one end of the cramp, there appeared to have been a small fusion; the end of the lead, and part of the adjoining stone, being indented about half an inch in diameter, and a quarter of an inch deep, as though a musquet-ball had been fired against them.

The iron cramp was situated over a plate of lead, and the ends of it, which were inserted in the stone, came within seven inches of that plate, which communicated with the gutter, and served as a fillet to it: this gutter was a part of the main conductor of the building.

When the lightning had quitted the iron cramp, and had passed through seven inches of stone, brick, and mortar, it fell upon the corner of the plate of lead above-mentioned, as appeared by the fusion of a very small portion of it discovered by pulling out the bricks, mor-

tar, &c. on purpofe to examine into this particular. From this place no farther effect of the lightning could be traced; the metallic conductors to the earth having effectually performed their office. At the diftance of feven feet and an half from the place ftricken, a large leaden pipe went down from the gutter to a ciftern of water in the yard.

The rain, Mr. NICKSON informed us, had fallen plentifully for fome time before the ftroke; fo that the mortar, bricks, &c. did probably form an imperfect conductor for the diftance of feven inches between the iron cramp in the copeing ftones and the filleting of lead above-mentioned.

At the termination of the iron cramp in the copeing ftone, a piece of the ftone, with one brick, was ftricken off; and a few other bricks were loofened, and removed lefs than half an inch from their places. The damage done to the parapet of the building is fo inconfiderable, that it would fcarce deferve notice, was it not an evident proof that the metallic communication with the earth hath, in this cafe, effectually prevented any farther injury.

The conducting rod on the ridge, near the center of the houfe, fhewed no marks of its having been affected by the lightning in this cafe: and it is remarkable, that the furface of one of the hip-rafters, four inches and a

half in diameter, covered with lead (communicating with the gutter) and reaching within twenty-eight inches of the place ſtricken, ſeems not to have been at all affected.

The method we would recommend of preventing ſimilar accidents to the parapet of this building for the future is the following. Let a channel of the ſame ſize with the cramps be made from cramp to cramp in the copeing ſtones, quite round the building: let this channel be filled with lead, and let a metallic communication by plates about ſix inches broad be made from that lead in four places (one at each ſide or corner of the parapet) to the filleting of lead which is in contact with the gutter, which gutter is part of the main conductor to the building.

W. HENLY.
June 19, 1777. T^Y. LANE.
E. NAIRNE.
J. PLANTA.

3. *Mr.* WILSON's *Diſſent from the above report.*

WHEN this important ſubject was firſt debated in the Purfleet committee of 1772, a paſſage was quoted from Dr. FRANKLIN's philoſophical publications, reſpecting the nature of ſuch buildings as were ſecure from attacks by lightning.

The paſſage alluded to is this: "Buildings that have "their roofs covered with lead or other metal, and ſpouts "of metal continued from the roof into the ground to "carry off the water, are never hurt by lightning; as, "whenever it falls on ſuch a building, it paſſes in the "metals, and not in the walls." FRANKLIN's Exp. p. 481.

With this idea the building at Purfleet, called the Board-houſe, was conſidered by that committee to be in a ſimilar ſituation, and conſequently ſecure from ſuch attacks, without having any other conductors than the leaden gutters, pipes, &c.

As the members of that committee then preſent ſeemed to be fully ſatisfied with that determination, I propoſed that the magazines themſelves ſhould be put into the ſame circumſtances; otherwiſe there would ap-

pear to be an inconfiftency in the different methods of fecuring thofe buildings.

My argument had no other effect than to occafion, at the next meeting of the committee, a refolution for fixing pointed conductors to all the buildings.

From this refolution I diffented, and gave in writing my reafons at large for differing in opinion, which are printed in your Tranfactions.

What has been the confequence fince the conductors were put up?

Behold! this very Board-houfe, which was never attacked before by lightning, hath very lately been ftruck, and that within a few inches of the conductor; contrary to Dr. FRANKLIN's affertion, which pofitively fays, that in fuch circumftances the lightning paffes in the metals, and not in the walls.

We may refine in our reafoning upon the philofophy of this event as much as we pleafe; but let me tell you, gentlemen, there is no getting rid of the fact: which, according to my judgement, appears to be truly alarming. And as, I apprehend, the reputation of this learned Society is greatly concerned therein, we ought immediately to avail ourfelves of this providential warning, and reject an apparatus which threatens us every hour with fome unhappy confequences.

It

It is with very great concern, that I am obliged to take notice, in this Society, of a houſe, which is of the firſt conſequence in this kingdom, that hath pointed conductors alſo fixed upon it: I mean the KING'S, our moſt gracious Patron and Benefactor's. Who were the adviſers of them I know not; but as they are there, I thought it my duty to mention them.

In conſidering the propriety of pointed conductors, I think it neceſſary to obſerve, that increaſing the number of them in any given ſpace does not by any means, in my opinion, leſſen the riſque of accidents by lightning; but on the contrary (at leaſt in many caſes) a greater number of ſuch conductors will neceſſarily invite a larger quantity of lightning. At Purfleet there are ſeveral of thoſe conductors; and by the ſtorekeeper's letter ſent to the Board of Ordnance, which was lately read before us, it appears, that he himſelf obſerved a very heavy cloud hanging over the houſe for ſome time before the ſtroke happened.

According to Dr. FRANKLIN'S idea, this event ought never to have happened; becauſe he ſays, that pointed conductors will draw all the lightning out of the clouds, and carry it away into the earth ſilently.

This philoſophy I never had any faith in, unleſs the quantity of lightning contained in the clouds happens to

be

be very little, and incapable of producing any fatal conſequences.

I have now only to add, that I did not propoſe to have troubled this Society any more, had I not thought, upon this great occaſion, it was my duty to ſtand forth, and give my opinion againſt the preſent report; as I know of no poſſible advantage to be derived from ſuch conductors; at leaſt none that are conſiſtent with true philoſophy, and a ſincere regard to the welfare of ſociety.

June 19, 1777. B. WILSON.

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4. *A Letter from the Board of Ordnance to Sir* John Pringle, *Bart. P. R. S. encloſing a Letter from Mr.* Wilſon *to* HIS MAJESTY, *and an Account of his Experiments on the Nature and Uſe of Conductors.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR, Office of Ordnance, Nov. 18, 1777.

MR. WILSON having laid before this Board a copy of the report made by him to His Majeſty upon ſome experiments in conſequence of the accident by lightning, in May laſt, to one of the buildings belonging to the royal magazine of gun-powder at Purfleet:

We

We beg leave to tranſmit to you a copy thereof, to be laid before the Royal Society; and, at the ſame time, we deſire the favour of their inſtructions, if any thing more can be done, in order to the preſervation of his Majeſty's magazines.

We are, &c.

AMHERST.

CHARLES FREDERICK.

CHA^{S}. COCKS.

TO THE KING.

SIR,

YOUR MAJESTY, in conſequence of the accident from lightning that happened to one of the buildings at Purfleet in May laſt, having been graciouſly pleaſed to intimate the propriety of making ſome farther experiments, to aſcertain the beſt method of preventing ſuch accidents for the future; and having alſo condeſcended to be preſent at the exhibition of thoſe experiments at the *Pantheon*; I have preſumed to addreſs to YOUR MAJESTY

 this

this faithful and circumſtantial account of what was there attempted, together with ſome obſervations thereupon, as an humble teſtimony of my duty and gratitude for the great honour conferred upon me.

How far I may have ſucceeded in theſe my zealous endeavours to aſcertain the moſt proper conſtruction for conductors is, with the greateſt deference, ſubmitted to YOUR MAJESTY and the public. And whatever conſequences may be derived from theſe experiments, I am happy in the thought of having done every thing in my power, with the utmoſt candour and impartiality, to inveſtigate truth, in a queſtion of real advantage to ſcience, and of ſuch importance to the public, as ſeems, in my humble opinion, worthy the attention of the ableſt philoſopers.

I am,

SIR,

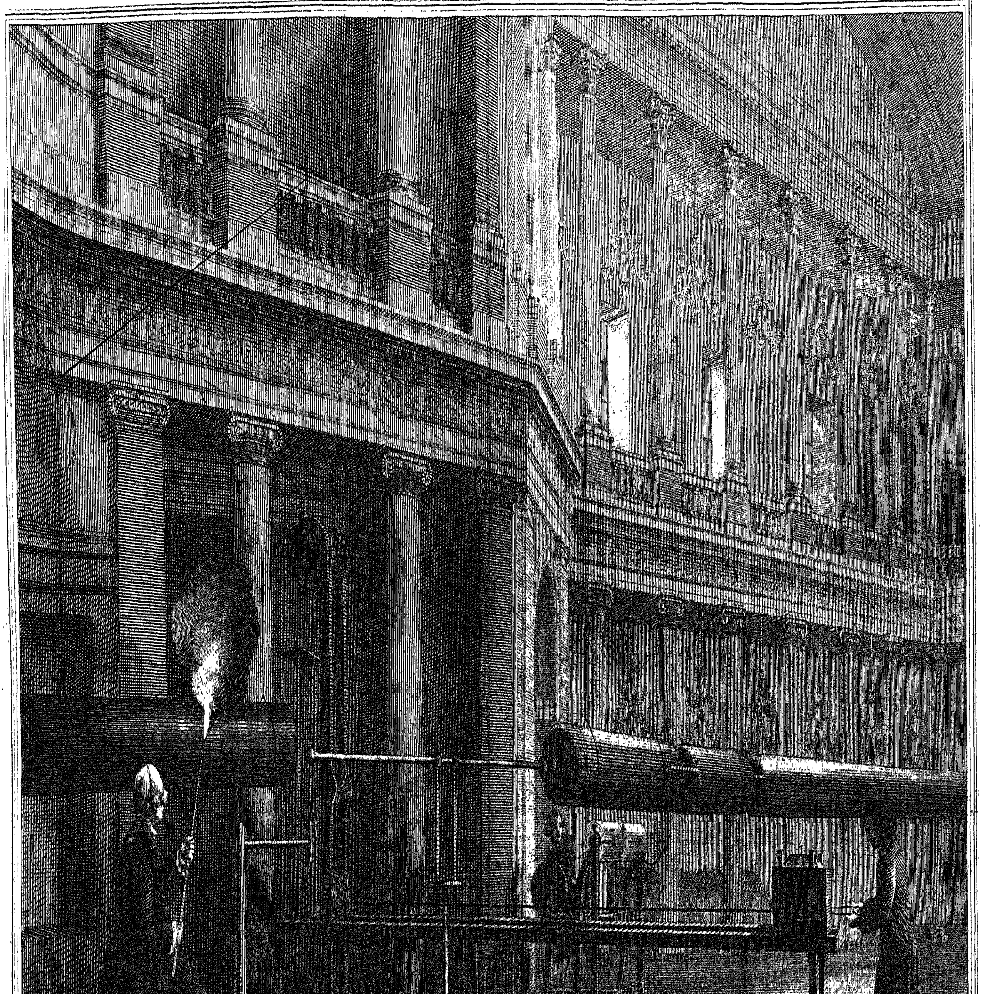
YOUR MAJESTY'S

moſt faithful and

moſt dutiful ſubject,

NOV. 12, 1777. BENJAMIN WILSON.

New



Engraved by Js. Basire.

A View of the Apparatus and part of the Great Cylinder in the Pantheon.

New Experiments and Obſervations on the Nature and Uſe of Conductors, by BENJAMIN WILSON, *F. R. S. of the Imperial Academy of Sciences at* Peterſburg, *of the Royal Society at* Upſal, *and of the Academy of Inſtitutes at* Bologna.

THE experiments I propoſe to give an account of in this paper, were made in conſequence of the accident from lightning, which happened to one of the buildings belonging to his Majeſty's magazine of gun-powder at Purfleet, on the 15th of May laſt.

Soon after that event, an official and particular account having been ſent by the Board of Ordnance to the Royal Society, a committee of the members was immediately appointed, to examine the damage done to that building, and afterwards to make a report of the ſame.

When that report was laid before the Society, I thought it my duty, in particular, to ſtand forth, and offer ſome objections to the uſing pointed conductors at Purfleet, or indeed any where elſe.

This public proceeding was, I apprehended, the more neceſſary, as I had, upon a former occaſion, in the year 1772, declared my *diſſent* from the report then made by

 the

the committee, who had recommended ſharp-pointed conductors for that magazine, to be fixed ten feet higher than the reſpective buildings.

But notwithſtanding I had read the paper alluded to above, I did not apprehend that my duty was fully diſcharged, without trying other methods of having ſo ſerious and intereſting a ſubject farther inquired into.

I had the ſatisfaction, ſoon after, to meet with ſufficient encouragement to induce me to conſider of ſome experiments, which might make the ſubject in diſpute more intelligible.

The plan I conceived to be the moſt proper for this purpoſe, was to have a ſcene repreſented by art, as nearly ſimilar as might be, to that which was ſo lately exhibited at Purfleet by nature.

To carry a deſign of that kind into execution, it was neceſſary that attention ſhould be given to the ſeveral circumſtances concerned in the event at Purfleet.

The moſt material of thoſe circumſtances I apprehended to conſiſt in having a ſubſtitute for a *thunder-cloud*, as it is vulgarly called, and large enough, or ſufficiently long, to admit of being charged with a conſiderable quantity of the matter of lightning by artificial means; and likewiſe, that this ſubſtitute ſhould admit of being eaſily moved, and with any velocity the experi-

ment

ment required: or, at leaſt, ſo as to equal the motion of a thunder-cloud.

An apparatus ſufficiently large for theſe purpoſes could not conveniently be put in motion: therefore I propoſed to get rid of this difficulty, by moving the building itſelf, inſtead of the ſubſtitute; as that would anſwer the ſame end.

In order to obtain a conſiderable charge of artificial lightning, I propoſed to have one great cylinder covered with *tin-foil*, and a wire joined to one end of it, that ſhould, when extended properly, conſiſt of ſeveral hundred yards.

This idea leading to an expence too conſiderable for an individual, I preſumed to hope for other aſſiſtance.

Upon an humble repreſentation of theſe matters, his Majeſty, who is always diſpoſed to promote every purſuit which tends to the advancement of ſcience and the good of the public, moſt graciouſly condeſcended to encourage the undertaking; and, by the favour of the right honorable and honorable Board of Ordnance, I was immediately enabled to carry the intended plan into execution.

Very ſoon after this encouragement, I procured correct drawings of the building called *The Board-houſe* at Purfleet: from theſe an exact model was made, excepting the

the windows and doors, which were omitted, becaufe they were immaterial upon this particular occafion.

In this model, a ftrict attention was paid to thofe parts of the building where metal had been introduced; fuch as the hips and gutters of the roof, and the feveral fpouts to carry off the water. And as the North-eaft corner of the houfe was the part that fuffered by lightning in May laft, particular attention was paid to the two cramps at that corner, and the two fpouts on the North fide. Thefe cramps, in the model, were made of fmall wire, that bore nearly an exact proportion to thofe in the building itfelf; not only in regard to length and thicknefs, but alfo their diftance from each other, and from the turning up of the lead appertaining to the gutter.

The two fpouts were reprefented each by a thick wire, the fhorter of which communicated (at the bottom) with a ciftern. This ciftern refting upon two wooden pillars, or pofts, about one foot and a half in length at Purfleet, the fame circumftances and proportions were attended to, and made to correfpond exactly in the model.

The other wire, in conformity to, and nearly in proportion with, the other fpout at the Board-houfe, defcending from the gutter for about feven inches, was there bent almoft at right angles, and then continued on for twelve inches and three-quarters, in a line nearly

horizontal, till it reached within two inches (or little more) of the ſhort wire: after which it was bent again almoſt at a right angle, and then lengthened out to the bottom of the model, from whence it communicated, by another wire, with a pump or well, in another part of the houſe.

This kind of communication was neceſſary, becauſe, conſiſting of metal, it, in that reſpect, was ſimilar to the communication at Purfleet.

Beſides the two cramps mentioned above, another parapet was made to put on occaſionally, which contained all the cramps: theſe were properly fixed therein, and at their proper diſtances from each other. And the Royal Society having thought proper, ſince the accident, to order that a metallic communication ſhould be made between the cramps upon the parapet, quite round the building, as a better ſecurity from ſuch accidents for the future, care was taken to make a ſimilar connection with the wire cramps, by means of ſmall ſlips of tin-foil that were paſted upon the parapet of this model.

On the top of the roof in the middle, conductors of different lengths and terminations were occaſionally put, juſt as the experiments required.

The ſcale from which this model was made, when

compared

compared with the houſe, was one-third of an inch to a foot.

In regard to the wood of which the model was made, I took care that it was well baked, and ſoaked, whilſt hot, in drying oil, before the ſeveral parts were joined together, that it might be the more ſimilar to the bricks and other materials of the building itſelf, in the power of reſiſting the paſſage of the fluid, whenever any attack thereof ſhould be made. For brick, ſtone, dry lime, &c. had been obſerved many years ago by Mr. DELAVAL and others to reſiſt the paſſage of this fluid very conſiderably.

In order to move this model with the velocity required, it was neceſſary to have a frame of wood, of ſuch a length as would ſuffer the model, with the pointed conductor upon it, to be out of the reach or influence of the charge contained in the cylinder, both at its ſetting off, and when it had arrived at the end of its journey.

To this frame, two upright poſts of wood, ten feet and a half long, were fixed at the farther end, and at a diſtance from each other equal to the width of the frame. Upon the top of theſe poſts, and in the middle between them, were fixed two wheels of different diameters upon the ſame axis. The larger took the line

that was propofed to draw the model, and the leffer another line fufpending two weights which regulated its motion. For after the heavier weight had defcended fo far as to bring the model directly under the fubftitute, it was then checked; but the leffer weight continuing to defcend, the model moved forward with its acquired velocity, joined to the power of the leffer weight. And that the remaining motion might at laft be overcome, without ftriking againft the two pofts, fome narrow flips of cloth feven feet long, were nailed upon the frame, in thofe parts over which the model was to pafs before it reached the end. This model moved like a fledge, by means of two flips of wood that were fixed at the bottom, which ran in two grooves that were cut along the frame from end to end. And the line which drew the model along, was fixed very near the center of refiftance.

To conftruct the fubftitute for a cloud, I firft joined together, in fifteen lengths, the broad rims of one hundred and twenty drums (merely to have them portable) by means of wood cut into long flips, which were fixed on the infides thereof: but, as thofe drums were not accurately of a fize, the feveral joinings were covered over with cloth, and pafted down, to make the furface throughout more even. After this, the whole number were properly

 covered

covered with tin-foil, excepting eight; for these being brafs, required to be covered only at their joinings. All these drums together formed a cylinder above one hundred and fifty-five feet in length, and above fixteen inches in diameter.

The whole cylinder was made in four feparate parts; three of thofe parts could eafily be made to communicate, or not, with each other: the fourth, being brafs, was referved for a different purpofe. The feveral ends of thofe four parts were clofed up with board, rounded off at the edges in every part, and covered with tin-foil likewife.

This great cylinder (confifting of the three parts) was fufpended about five or fix feet from the floor by filk lines; and formed a curve in the room, fomething like an horfe-fhoe; one end of which hung over the middle of the long frame, on which the model was propofed to move; the other (which I call the farther end) was joined occafionally to the end of a long wire, that was fufpended through the whole fpace of the room. And left the feveral atmofpheres round this wire, in its charged ftate, might, in confequence of the unavoidable returns of the wire, interfere too much with each other, it was fufpended in fuch a manner (by filk lines alfo) that each length was five or fix feet from its neighbour: and thofe that were fufpended neareft to the great cylinder hung at

the ſame diſtance from it. The remote end of this long wire hooked on occaſionally at the end of the braſs drums, which made a ſeparate cylinder (the fourth part alluded to above) about ten feet in length: this was ſuſpended likewiſe by ſilk lines, and about ſix feet from the floor; but in ſuch a manner, that the fartheſt end thereof from the wire was within nine or ten feet of the great cylinder.

The long wire with the great cylinder and braſs drums made the whole of the ſubſtitute for a thunder-cloud, when they were properly charged.

The machine, employed to charge this apparatus, conſiſted at firſt of two large glaſs cylinders that were turned by one wheel. But as the friction ariſing from the two together rendered it difficult to work them, and the advantage gained from both in the charge itſelf was found to be not ſo conſiderable as might reaſonably be expected, one of them only was made uſe of in the following experiments. The place where this machine charged the great cylinder, was about ten or eleven feet from its neareſt end. It was found expedient to be provided alſo with another machine; but this was employed only upon particular occaſions, and was generally placed at the farther end of the great cylinder.

The floor of the room being of baked wood, it was neceſſary to have wires properly connected with the

 cuſhions

cuſhions of both machines along the floor, where they were joined to another wire, which communicated with the well, in order to conduct the fluid more readily than the baked wood admitted of.

The whole of this apparatus, ſo contrived, was diſpoſed in the great room of the *Pantheon*, by the favour of the proprietors, who, having heard that a large apartment was wanted, in which to ſhew before the Board of Ordnance and the Royal Society theſe experiments, were pleaſed to honour me with a very polite letter, offering the uſe of that elegant building for the purpoſes intended.

EXP. I. The model, with a pointed conductor upon it (which, in a degree of ſharpneſs, was nearly equal to that of a common darning needle) being placed directly under the nearer end of the great cylinder, ſo that the diſtance between the point of the conductor and cylinder was little more than four inches, the machine was then put into motion. After two turns of the wheel, or thereabouts, a ſmall ſtream of light appeared at a little interval, between the top of the longeſt thick wire which repreſented the bent ſpout, and its little ciſtern next to the gutter, where the metallic communication was purpoſely interrupted. This ſtream continued to be viſible, though

though the model was moved along the frame from its fixed ftation to more than the diftance of forty-three inches.

EXP. II. When a conductor of the fame length with the former, but rounded at the end, and no more than three-tenths of an inch in diameter, was put in the place of the pointed one, every other circumftance continuing the fame, the fmall ftream of light appeared again; but upon moving the model a little beyond the diftance of fixteen inches, it totally difappeared.

FIRST OBSERVATION. By the firft experiment it was manifeft, that the point acted upon the charge all the time the model was moving through a fpace equal to forty-three inches; and confequently was, all that time, diminifhing the charge in the great cylinder. On the other hand, the fecond experiment fhewed, that the rounded end acted upon the charge only whilft the model moved through a fpace equal to fixteen inches. And from the two experiments compared, it appears, that a charged body is exhaufted of more of the fluid by a pointed, than by a blunted, conductor.

EXP. III. If in the place of the rounded conductor a fimilar one was put, but about one-fifth of the length, whilft the model ftood directly under the great cylinder

as before, the charge contained therein produced no appearance of light whatſoever.

EXP. IV. But when a pointed conductor of the ſame length with the laſt was put in its place, the ſmall ſtream of light appeared, and continued viſible all the time the model was moved through a ſpace equal to eighteen inches and a half.

SECOND OBSERVATION. Theſe laſt experiments, compared with the former two, ſhew that a rounded conductor, little more than one foot and a half above the higheſt part of a building, receives a far leſs quantity of the matter of lightning from a cloud fully charged therewith, than a pointed conductor placed ten feet above a building, circumſtanced alike in every other reſpect. Nay, a pointed conductor of the ſame length with the ſhort one that was rounded, appears, from theſe experiments, to collect a greater quantity of the fluid, than even the long conductor with a rounded end.

The difficulty of meaſuring exactly the effects, when the great cylinder was charged, was ſo great, that after a variety of endeavours to aſcertain the quantity of the charge remaining, when different terminations and different lengths of conductors were employed, I was obliged at laſt to have recourſe to the ſenſe of feeling, uncertain as it is, in many caſes, to determine the different

effects

effects occafioned by the interpofition of thefe different terminations.

EXP. V. On repeating the firft experiment (that is, with the long pointed conductor upon the model) with ten turns of the wheel only, the charge remaining in the great cylinder was immediately received on the hand; the fenfation it occafioned was little more than perceptible.

EXP. VI. But upon repeating the fecond experiment (that is with the long rounded conductor upon the model) with ten turns of the wheel alfo, the charge remaining was taken; but the fenfation in this cafe was increafed confiderably.

EXP. VII. When the third experiment was repeated (that is with a rounded conductor five times fhorter than the laft) and with the fame number of turns, the fenfation was obferved to be full as violent, as if no fuch metallic interpofition had been prefented to it.

EXP. VIII. Upon repeating the fourth experiment (that is, with the pointed conductor equal in length to that in the laft experiment) and with ten turns of the wheel alfo, the fenfation was not near fo confiderable; for it feemed fomething lefs than what was experienced with the long rounded conductor in the fixth experiment.

THIRD

THIRD OBSERVATION. The ſeveral effects obſerved in the four laſt experiments agreeing ſo exactly with thoſe in the four firſt, prove, at leaſt ſo far, that rounded ends, in theſe caſes, received the fluid leſs readily, and in leſs quantity, than points.

But, before other experiments are related, it may be proper to take notice of what paſſed at the ſhort ſpout upon the model during the making of the four firſt experiments; becauſe the conſequences to be drawn from thence are very material.

EXP. IX. In the firſt experiment (when the long pointed conductor was put upon the model, and whilſt the wheel was turning) a very ſmall ſpark might be taken from the ſhort ſpout; but if the hand, or a wire, was applied, which communicated with the well, and continued in contact with the ſhort ſpout, or if it was connected with the long ſpout, the ſmall ſtream of light ſeen before at the top of that ſpout now ceaſed.

EXP. X. But this was not the caſe when the third experiment was repeated (that is, with the ſhort rounded conductor); for this, by reaſon of its greater diſtance from the cylinder, and the nature of its termination, did not draw a ſufficient quantity of the charge from the great cylinder to cauſe the leaſt appearance of a ſtream of light, as in the former caſe.

EXP.

EXP. XI. The effect, however, was different, when a pointed conductor of the same length as in the fourth experiment was made use of; for then the short spout was charged nearly in the same manner as in the first experiment, and the stream of light at the top of the bent spout disappeared the instant a communication was made from the short spout to the earth.

FOURTH OBSERVATION. The two posts of wood upon the ground, which supported the cistern at the bottom of the short spout, were therefore the true cause of these effects taking place in the short spout, by preventing a communication with the earth, and hindering the fluid, that was constantly charging the short spout, from discharging itself properly into the earth.

It may now be proper to take notice of other experiments, and also of two other circumstances that seem to be of considerable consequence in this inquiry.

One of these circumstances respects the motion of the model, instead of that of the cloud; and the other, the quantity of the fluid contained in the great cylinder when properly charged.

As to the former of these, it appears from observations, that clouds, in a very high storm, frequently move at the rate of eighty miles in an hour; and, with a moderate wind, about twenty miles[(a)]. Now when the clouds

(a) In the Phil. Transf. there is an account of a storm that was computed to move at the rate of three miles in a minute.

move at the rate of four or five miles an hour, the wind which occaſions that motion is, I apprehend, in general, little more than what is uſually ſaid to be ſenſible: for it has been frequently obſerved, that a traveller on horſe-back muſt ride with a pretty good ſpeed, to keep pace with the ſhadow from a cloud, when it chances to move in the direction of his journey, even though the wind at ſuch a time can ſcarcely be ſaid to blow.

Now a thunder-cloud, as it is uſually called, moves, as I apprehend, in many caſes at leaſt, with a much greater velocity; and though there may be a few inſtances, where ſuch clouds move with a leſs, I think, if the motion of them be put, in general, at four or five miles an hour, it ought to be conſidered, in this caſe, as a very moderate computation. For theſe reaſons, the weight to draw the model was adjuſted to that velocity.

In regard to the quantity of the fluid required to charge the great cylinder, I found, from many experiments, that twenty uniform, and rather briſk, turns of the wheel, were the moſt favourable for the following experiments; becauſe half a turn in twenty made a far leſs difference in the charging, than half a turn of the ſame wheel, when eight or ten turns only were required, and with nearly the ſame uniform velocity.

In obſerving this laſt rule, proper allowance was made for the different ſtates of the air, the ſtate of the glaſs itſelf, and that of the cuſhion or rubber, which excited the fluid by a proper preſſure upon the glaſs, joined to other circumſtances. The moſt material of theſe circumſtances conſiſted in the cuſhion's having a free communication with the earth and glaſs cylinder, in that part where the friction was applied; and alſo a ſimilar communication between the great cylinder and the oppoſite ſide of the glaſs. Now, becauſe three pointed wires conducted the fluid readily from the glaſs to the great cylinder, I apprehended it was proper to have three ſuch points within the cuſhion itſelf to conduct the fluid as readily from the earth to the glaſs. For thoſe points communicated by a wire with the well.

It will appear, that the effects produced by this artificial method of charging the cylinder muſt be very different from thoſe produced in nature.

Suppoſe a thunder-cloud coming already charged, the lightning from this cloud ſtrikes at Purfleet; the ſame cloud, paſſing afterwards over other places, will ſtrike the earth again and again, without any apparent diminution of the quantity of lightning contained therein, as hath been frequently obſerved. But the caſe is not the ſame with the great artificial apparatus, becauſe we not

only charge it by degrees; but when a ſtroke is taken from it, the greateſt part of the charge by far is at that inſtant taken out of it: and therefore we are conſtantly under the neceſſity of renewing the charge before ſuch another ſtroke can be taken.

Beſides, thunder-clouds from their nature, and a variety of circumſtances accompanying them, never aſſume the ſame ſhape and ſize: neither are they always at the ſame diſtance from each other. And yet we are told, that they have been obſerved to ſtrike from one to another at different diſtances, juſt as they happen at the time to be circumſtanced.

For all theſe reaſons, I thought it was proper to make particular experiments in different caſes, where ſome of the circumſtances varied. And becauſe a ſingle cloud, after it hath ſtruck any one object, ſometimes continues to diſcharge vaſt quantities of lightning, I propoſed to begin the experiments with the great cylinder only.

EXP. XII. The model (furniſhed with the wire of communication, and with the longeſt pointed conductor upon it) being properly placed upon the long frame, and held there in readineſs to be drawn forward by the line and weight at the other end; the great cylinder was charged by twenty turns of the wheel. On letting go the model, and almoſt at the inſtant before the point came

under the center of the cylinder, it was ſuddenly ſtruck with the matter of lightning, and frequently ſooner. The leaſt diſtance of the point from the great cylinder, when this ſtroke happened, meaſured nearly five inches. The quantity of charge that remained in the cylinder was very little to the ſenſe of feeling, though taken immediately after the ſtroke happened.

EXP. XIII. On putting into the place of the pointed conductor one of the ſame length, that was rounded at the end, and without any other change of circumſtances, the wheel having been turned the ſame number of times, I ſuffered the model to paſs: the rounded end, in this caſe, was not ſtruck. However, the inſtant after it had paſſed, the quantity of the charge that remained in the cylinder was taken in the ſame manner by the hand; on the doing of which, the ſenſation was more violent than in the laſt experiment.

FIFTH OBSERVATION. From the two laſt experiments it appears, that though every circumſtance was the ſame, excepting the different terminations of the two conductors, yet the pointed one only was ſtruck; notwithſtanding they were both of the ſame length, and paſſed at equal diſtances from the cylinder. From whence we collect, that the quantity of lightning diſcharged from the great cylinder into the point, when an exploſion happened, was

was confiderably greater than the quantity difcharged into the rounded end, when there was no explofion.

At the firft rife of a difference in opinion, refpecting the proper termination and length for conductors, I was prevailed upon by fome learned members of the Royal Society, in the year 1764, to publifh my fentiments upon that fubject. Accordingly, in a letter addreffed to the Marquis of Rockingham [(a)] (after ftating feveral reafons againft the ufe of points, as I fuppofe they invited the lightning) I there recommended that conductors fhould not only be rounded at their ends, but be made confiderably fhorter than thofe which Dr. FRANKLIN contended for, and indeed fhould not exceed the higheft part of the building.

In the following experiment, however, I did not place the pointed conductor below, nor upon a level with, the higheft part of the building, but above it, even one-third of the length of that in the twelfth and thirteenth experiment.

EXP. XIV. The model being thus furnifhed, and every thing elfe put exactly into the fame circumftances as in the thirteenth experiment, the great cylinder was charged by twenty turns of the wheel. Upon letting go the model, it paffed the cylinder at the diftance of feven inches, without being ftruck: but the charge that

(a) Phil. Tranf. vol. LIV.

remained

remained in the cylinder at the inſtant after the model had paſſed it, was ſo conſiderable, that there appeared no material difference whether the model thus circumſtanced was ſuffered to paſs or not.

SIXTH OBSERVATION. This laſt experiment ſhews, that a thunder-cloud may paſs a conductor ſo circumſtanced without the latter being ſtruck, or ſuffering the leaſt injury; which it will not do in other circumſtances, that is, when the conductor is pointed, and raiſed ten feet above the building.

EXP. XV. On repeating the fourteenth experiment, but with a rounded conductor, which was three-fourths of the whole length of that in the twelfth experiment (all other circumſtances remaining the ſame) and after charging the cylinder by an equal number of turns, it paſſed alſo without being ſtruck. In this caſe, the remaining charge in the cylinder was ſomething leſs than in the laſt experiment.

SEVENTH OBSERVATION. This is a further inſtance of the advantage derived from having rounded terminations of a given length upon a building, compared with pointed ones, that are only two or three feet longer.

Having ſo far experienced the different effects of different terminations in the preceding experiments, it may be proper to mention another experiment, where the

rounded

rounded end was ſtruck. But as its diſtance from the cylinder, at that inſtant, was only one quarter of an inch leſs than the diſtance at which the point had been ſtruck, I ſhall take no farther notice of it in this place; becauſe, the ſeveral ſtriking diſtances of thoſe different terminations will (in another part of this paper) be correctly aſcertained, in a different manner, by other experiments.

We are now to examine, whether the ſtroke by lightning, which happened at Purfleet, fell firſt upon the corner of the building where the cramps were affected, as hath been repreſented; or whether it did not fall upon the point of the conductor itſelf? And if it fell upon the point, how could it poſſibly affect thoſe cramps, as they had no metallic communication with the main conductor which extended from the top to the bottom of the houſe, and of which the gutter below the parapet, that was neareſt to the cramps, made a part?

It muſt be remembered, that though in attempting an experiment of this kind, we are provided with an apparatus, greater perhaps than was ever conſtructed before; yet, great as it is, it bears but a very ſmall proportion to that which nature makes uſe of. On this account, we muſt expect but very faint appearances compared with thoſe which are produced by a thunder-cloud. But, before we relate theſe appearances, it may be proper firſt to

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fee what effect the charge itfelf has upon the model, without any conductor upon it.

EXP. XVI. Upon charging the great cylinder as before, that is, by twenty turns of the wheel, and when the model, without any conductor upon it, was let go, there was no explofion in, or on, any part of it, during the time of its paffing by the cylinder; notwithftanding the model itfelf was properly connected with metal from the top of the roof to the bottom of it, and afterwards to the well.

EXP. XVII. But when the experiment, with the long pointed conductor upon the model, was repeated, and after the wheel had been turned an equal number of times, the point was ftruck as it paffed the cylinder; and, at the fame inftant alfo, a very fmall ftream-like explofion appeared between the two cramps at the corner of the model, darting as it feemed from one to the other, in a direction that was rather particular. This ftream-like appearance, I apprehended, was nothing more than the effect of a fmall explofion in confequence of the motion of the model.

Mr. WYATT, who is well acquainted with this part of Philofophy, and to whom great obligations are due from me for his very friendly affiftance in the whole of this undertaking, having obferved this appearance feveral

 times,

times, was curious to confirm the fact, left the prejudices of viſion, &c. might poſſibly deceive him.

The method he took upon this occaſion, was to obſerve an appearance of light along that hip of the roof which was next to the corner ſtruck; and whether the direction of that appearance was different from the direction of the other appearance he had obſerved at the corner between the two cramps. Theſe appearances he endeavoured to aſcertain by means of a paper tube, blacked on the inſide, that was laid upon a ſtand of a proper height, ſo that, when he looked through it, a pin, being ſtuck upright at the further end thereof, coincided with the part in the model he was to examine; that is, when the model ſtood directly under the cylinder. Being thus circumſtanced, and when the corner of the model (during its motion) paſſed the pin, he ſaw the direction of the light along the hip next the corner, and alſo between the cramps at the ſame time; and was poſitive that the direction of the light appeared to be downward from the roof, and, as he thought, horizontal between the two cramps. Theſe appearances were obſerved by others afterwards, exactly as Mr. WYATT had before deſcribed them.

EIGHTH OBSERVATION. The fifteenth experiment ſhewed, that the corner of the model, where the two

cramps

cramps were inferted, paffed fafely by the charged cylinder, without affording even the leaft luminous effect; and confequently proved, that in fuch circumftances the two cramps could not poffibly be ftruck, becaufe the charge in the cylinder remained the fame, or very nearly fo, after the model had paffed.

NINTH OBSERVATION. But in the feventeenth experiment, the pointed conductor was fixed in its place upon the model, juft like that at Purfleet; when not only the point was ftruck, as the model paffed the cylinder; but, at the fame inftant, a fmall explofion was feen between the two cramps at the corner. That this light between the cramps arofe from a different caufe than what had been fuggefted by the fecond Purfleet committee, appeared from fome circumftances accompanying that effect. For example, thefe cramps had no connection with the gutter or fpouts next them, but were quite feparate, and at the diftance of fix or feven inches from any metallic communication. And it is well known to philofophers, that lightning always paffes where it meets with the leaft refiftance; they alfo know, that the leaft refiftance in the prefent inftance muft have been along the conductor at the top to the hips, gutters, and fpout, and fo on to the wire at the bottom to the well.

According to this law, the cramps themſelves, then, were not properly circumſtanced to receive the fluid as it paſſed to the earth, on account of the metallic communication, deſcribed above, being interrupted more than ſix inches.

Another cauſe therefore was neceſſary to explain the appearance; I mean that which is called the *lateral* effect; a term lately adopted, in conſequence of an experiment I made near thirty years ago with the Leyden phial (which experiment has ſince been improved upon by Dr. PRIESTLEY): and though a charged glaſs (for ſuch I call the Leyden phial) is by no means ſimilar to the great cylinder when charged, for reaſons that have been already publiſhed[(b)]; yet in the courſe of theſe experiments I apprehended the lateral effect produced with the charged glaſs might be conſidered in ſome reſpects as an illuſtration of the lateral effect which had been obſerved at the cramps.

But, not to reſt this very material fact upon any doubtful opinion that may poſſibly be entertained reſpecting its exiſtence, other experiments will be produced, that may explain this matter more ſatisfactorily.

(b) Farther Experiments and Obſervations upon Lightning, by BENJAMIN WILSON, publiſhed in 1774.

Before

Before theſe experiments are related, it is proper to mention a material circumſtance, which hath hitherto been unnoticed.

At the time the accident happened at Purfleet, a great quantity of rain fell, by which the walls of the Board-houſe, being made very wet, were diſpoſed to admit more readily the lightning paſſing upon the ſurface, though yet not ſo readily as a covering of metal would have done. This particular circumſtance of the rain will be attended to in its place.

But firſt it may be proper to ſhew the effects of a metallic communication.

To this end, I covered the top of the parapet quite round with tin-foil; and becauſe the copeing projected a little over the parapet, under which no rain could poſſibly get, I left a ſmall interval, proportional to it, uncovered with that metal. This interval was on the oppoſite ſide of the model, which anſwered to the South-ſide of the building where the other ſpouts were fixed. Then from that interval I paſted a broad ſlip of tin-foil down to the bottom of the model, where having fixed a ſmall braſs ſtaple to hook a wire upon, I faſtened the other end of it to the wire which communicated with the well. At the ſame time care was taken to faſten another wire in the ſame manner, which communicated with the oppo-

fite fide of the model. This wire was intended to anfwer the purpofe of a fpout in the building at Purfleet towards the South.

Befides thefe precautions, there remained another circumftance to be attended to, refpecting motion: for feveral gentlemen had obferved, that clouds in a thunderftorm are not always in motion; or at leaft not in the degree of motion which has been reprefented in the preceding experiment. To obviate that objection, I propofed to repeat thofe experiments when the model was at reft. In order to prepare the whole apparatus in the moft proper manner for this purpofe, it was neceffary to attend to the circumftances that are obferved in nature.

For when a cloud comes ready charged, and ftrikes another cloud with the matter of lightning, there muft be a certain diftance at which that effect muft take place; and this we therefore call the greateft, or the ftriking diftance. Now, to produce fimilar effects, our artificial apparatus muft confift of two parts at leaft, to reprefent fuch clouds: and thofe parts muft be fo difpofed as not to exceed the greateft diftance at which they will ftrike, when the largeft part, or fubftitute, is properly charged: nor muft the diftance between them be lefs, for reafons that will appear prefently. And therefore, to make the experiment correfpond with nature, it will require fome

trouble

trouble to adjuſt this diſtance between the two ſubſtitutes. Now the diſtance between them will depend upon the quantity of the charge given, and the method to determine that diſtance may be found by removing one ſubſtitute from the other ſo far as not to cauſe any previous or partial exploſions before the great ſtroke happens; and when it does happen, it muſt not only ſtrike between the ſubſtitutes, but, at the ſame inſtant, between the remote end of the leſs ſubſtitute and the object oppoſed to it.

EXP. XVIII. To each end of a ſlender ſubſtitute made of wood, about eleven feet in length, and ſomething leſs than one inch in diameter, was fixed a ball of the ſame matter. The larger of theſe meaſured three inches in diameter, and the leſſer one inch nine-tenths. The exact meaſure of theſe balls was attended to the more, in ſome of the experiments, becauſe Mr. NAIRNE has given a deſcription of an apparatus in the Phil. Tranſ. vol. LXIV. part I. p. 87 and 88. to which this is nearly ſimilar. Having covered the whole of this ſlender ſubſtitute with tinfoil, it was then ſupported near the center by a ſlender frame of wood upon a pillar of glaſs, to adjuſt it to the height and diſtance required. The larger ball was then brought within one inch and a quarter (and ſometimes at a greater diſtance) of another ball, one inch nine-

tenths

tenths in diameter, that was covered with the ſame metal, and projected from the center of the nearer end of the great cylinder by a kind of ſtem made of wood, and covered with tin-foil alſo, which was ſix or ſeven inches long. Being ſo prepared, the model was ſet upon a table, directly under the ball at the remote end of the leſs ſubſtitute, with the pointed conductor upon it: and all the wires were properly connected, ſo as to make a free communication between the model and the well. Nothing now remained but to put the machine in motion; when, after ten turns of the wheel, the point upon the model was ſtruck at the diſtance of four inches.

In the twelfth experiment, where the model was in motion, the point was ſtruck at the diſtance of five inches nearly from the cylinder. This difference of diſtance in theſe two experiments ſeemed to ariſe chiefly from a difference in the ſtates of the air, it being rather unfavourable when the eighteenth experiment was tried.

EXP. XIX. However, ſome time after, when the ſtate of the air was very favourable, I repeated the laſt experiment, and obſerved that the point was ſtruck at ſix inches and one quarter.

During this laſt experiment, a perſon, ſtanding upon the wire of communication, placed his finger in contact with the pointed conductor (near the bottom) at the time

the

the ſtroke happened, when he received a blow, uncommonly violent, inſomuch that he thought it exceeded greatly what he had ever experienced from the great cylinder only.

EXP. XX. I now repeated the eighteenth experiment; and attending only to the two cramps at the corner, there appeared, at the inſtant when the point was ſtruck, a ſmall ſpark or exploſion between them: which clearly ſhewed, that the ſtream-like exploſion, obſerved in the ſeventeenth experiment, was only this ſmall ſpark accompanied with the circumſtance of the motion of the model.

TENTH OBSERVATION. From what we have now experienced it appears, that thunder-clouds, even at reſt, and that ſtrike each other at a given diſtance with the matter of lightning, occaſion the ſame phænomena nearly which a ſingle cloud produces when motion is introduced.

EXP. XXI. When the diſtance between the two ſubſtitutes was made leſs in any degree than the greateſt ſtriking diſtance (in proportion to its diminution the circumſtances were leſs ſimilar to thoſe in nature) it made a conſiderable difference in the effects; becauſe the fluid in theſe caſes paſſed more freely from the greater to the leſs ſubſtitute: and the freer it paſſed into the latter, the

the nearer they approached to be one ſubſtitute. So that bringing the two ſubſtitutes into contact, occaſioned the ſame phænomena that the great cylinder did alone: that is, the rounded end would cauſe an exploſion at a conſiderable diſtance; and the point little or none, notwithſtanding it was brought almoſt cloſe to the ſubſtitute.

EXP. XXII. But if motion in this caſe was introduced, during the contact of the two ſubſtitutes, the point was ſtruck at nine inches and an half diſtance from the ball. The motion employed upon this occaſion was by the hand only, which held a proper ſtand with the point upon it; and this point communicated by a wire with the well.

ELEVENTH OBSERVATION. Now the nearer the two ſubſtitutes were brought together, the nearer they repreſented one cloud; and, conſequently, as hath been obſerved before, the matter of lightning would paſs from one to the other in theſe caſes more readily, and without permitting ſo great an accumulation to take place as was experienced in the nineteenth experiment, when the ſeparated ſubſtitutes ſtruck at the greateſt diſtance.

To occaſion ſuch a ſtroke, it is not only neceſſary to have an accumulation of the fluid, but that accumulation muſt be kept up, as it were, and increaſed ſuddenly, before a ſtroke in this caſe can poſſibly take place; as

appeared

appeared manifeftly by the motion introduced in the twenty-fecond experiment, where the two fubftitutes were united: for the refiftance at a point being feeble, when in this experiment a point was fuddenly brought towards the ball at the end of the great apparatus in charge, the whole of that charge muft of confequence rufh towards the point in an inftant, to difcharge itfelf into the earth. This, I apprehend, was the true reafon why we obtained an explofion at the diftance of nine inches and an half.

Having now gone through the experiments where points were introduced, we fhall next relate the feveral experiments where other terminations were ufed. By this method of proceeding, we fhall be able to form a proper judgement what kind of conductors are the moft advantageous for fecuring buildings, &c.

EXP. XXIII. On repeating the eighteenth experiment, but with a rounded conductor upon the model, every other circumftance continuing the fame, and the model at reft, the greateft diftance at which it was ftruck (in confequence of ten or eleven turns) was not more than three quarters of an inch.

Now if we compare this diftance with that at which the point was ftruck in the eighteenth experiment, the proportion will be found to be lefs than one to five.

But this and the eighteenth experiment were repeated many times afterwards, for many days together, and when the ſtate of the air at each trial was very different; on which accounts the reſults of thoſe experiments varied as follows.

Sharp point.	Rounded end.
$2\frac{1}{4}$ inches.	$\frac{5}{8}$ of an inch.
4	$\frac{1}{2}$
$3\frac{1}{4}$	$\frac{7}{8}$
$5\frac{1}{2}$	$\frac{4}{10}$
$6\frac{1}{2}$	$\frac{1}{2}$
2	$\frac{1}{10}$

Immediately after relating the nineteenth experiment, mention was made of the ſenſation or blow that was received by a perſon who had brought his finger into contact with the pointed conductor at the time the ſtroke happened. The experiment was repeated here with the rounded end; but the ſenſation or blow received (as well as the exploſion) was conſiderably weaker in this caſe.

EXP. XXIV. When the twenty-third experiment was repeated, and the rounded end ſtruck at three quarters of an inch, the ſame kind of ſpark appeared between the two cramps as in the twentieth experiment when the point was uſed; but in this experiment with the rounded

rounded end the ſpark at the cramps appeared conſiderably leſs to every obſerver.

EXP. XXV. Upon repeating the twenty-firſt experiment where the two ſubſtitutes were brought into contact, every other circumſtance remaining the ſame, the rounded end was ſtruck at the ſame diſtance nearly as when a ſpark was taken by a larger metal ball (ſuppoſe three inches in diameter) from any part of the great cylinder when equally charged: for in this caſe the two ſubſtitutes, being in contact, made in reality but one great ſubſtitute.

EXP. XXVI. I now repeated the twenty-ſecond experiment, where motion was introduced; and without any other change of circumſtances than putting in the place of the point the rounded end. Upon this occaſion, as well as upon the former, the ſame perſon moved the ſtand with the rounded end upon it, and with the ſame velocity (as near as he and others preſent could judge) but not before the connected ſubſtitutes were fully charged by an equal number of turns. The inſtant that the rounded end approached within a certain diſtance of the ball at the end of the leſs ſubſtitute, it ſtruck; but the exploſion ſeemed inferior to that which the point occaſioned at the diſtance of nine inches and an half. In this experiment the diſtance between the rounded end and

ball

ball was not more than ſix inches and an half. From which it appears, that even in thoſe circumſtances the point was ſtruck at a greater diſtance than the rounded end in the proportion of nine and an half to ſix and an half.

EXP. XXVII. It has been obſerved in a former part of this paper, that a great quantity of rain fell when the accident happened at Purfleet; as this circumſtance ſeemed to be material, it was proper to put the model into a ſimilar ſituation; and therefore, after removing the tin-foil upon the parapet, I waſhed the model all over with a ſponge; and, whilſt it continued in this ſtate, the machine was put into motion: after ten turns of the wheel, the point was ſtruck at five inches diſtance. In conſequence of this, a ſmall exploſion (more vivid than in the ſeventeenth experiment) appeared, not only between the cramps; but alſo another was ſeen, ſtill more vivid, at the inner corner of the parapet, neareſt to the cramps, darting, as it ſeemed, from the gutter up to the cramps.

EXP. XXVIII. Upon paſting tin-foil upon the top of the parapet quite round the model, as had been done before, and moiſtening the inner part of the parapet down to the gutter, the ſtroke was again received by the point, when the ſame appearances were obſerved as in the laſt experiment.

EXP. XXIX. I then made a metallic communication between the top of the parapet down to the gutter: after this the experiment was repeated. And though the point was ſtruck at five inches diſtance, there was no ſpark whatſoever, either between the cramps or at the inner corner next to the gutter.

TWELFTH OBSERVATION. From theſe laſt experiments it appeared, that rain contributed to make the *lateral* effect greater at the corner, by forming a better communication between the cramps and the gutter, than the dry materials of brick and ſtone admitted of. But it alſo appeared that, when the communication between the gutter and cramps was rendered more perfect by a ſlip of tin-foil that was interpoſed between them, that *lateral* effect ceaſed at the cramps; becauſe a ſtill freer paſſage was made for the fluid to diſcharge itſelf through; not only along the ſlip of metal communicating with the gutters and the tin-foil quite round the parapet, but alſo along the tin-foil down the ſide of the model, from whence it was conveyed by the wire to the well.

EXP. XXX. An objection having been made, that the wire communicating from the bottom of the model to the well, as it conſiſted of ſeveral diſtinct parts, occaſioned a reſiſtance at each of the junctions, and therefore conſtituted only an imperfect conductor; in order to try the

the validity of this objection, a new communication was now made from the model to the well, confifting of one entire wire, pointed at the end, with which I repeated the experiment of paffing the model; and finding no fenfible difference from the former refults, I then, in order to apply a rounded end to this perfect communication, had the pointed conductor, which was previoufly made ufe of, foldered on to the end of the wire inftead of the other point; and to this conductor fo foldered I occafionally applied the fmall ball which was ufed in the former experiments, and which has hitherto been called the rounded end. It was made to fit upon the point of the foldered conductor by means of a focket, fo as to render the communication perfect. With thefe different terminations, and thus circumftanced, all the experiments were repeated; and it was found, that the refults were rather more in favour of the doctrine hitherto advanced, than before the communication was made fo perfect.

It having been farther objected, that the motion of the model employed in thefe experiments was confiderably greater than the motion of a thunder-cloud, I made the following experiment.

EXP. XXXI. The weight which moved the model in the preceding experiment was gradually reduced till it was nearly balanced by the friction; and when the motion

motion was rendered ſo ſlow as ſeven feet ſeven inches in ſeven ſeconds, it was very little accelerated; and in this ſtate the great cylinder being charged, the model was ſuffered to paſs: and though the velocity was leſs than three quarters of a mile in an hour, the point was ſtruck. This experiment was repeated ſeveral times, with the ſame ſucceſs, in the preſence of ſeveral gentlemen.

Having made two other experiments, reſpecting the different terminations of conductors, and in very different circumſtances from any that have been yet related, I ſhall here give an account of them.

EXP. XXXII. When the great ball of the leſs ſubſtitute was placed at the greateſt ſtriking diſtance from the ball at the end of the great cylinder, I fixed a needle into the under ſide of the remote end of the leſs ſubſtitute, with the point downwards: oppoſite to this point, and upon the ſame ſtand deſcribed in the twenty-ſecond experiment, was fixed another needle; ſo that the two points were oppoſed to each other. The ſpace between them was varied from time to time, in order to find the greateſt diſtance at which the lower one could be ſtruck. Upon charging the great cylinder it appeared, that the greateſt diſtance in this caſe was five inches and a quarter.

EXP. XXXIII. Upon repeating this experiment, whilſt every circumſtance remained the ſame, excepting that, inſtead of the point below, a rounded end was put in its place, and after charging the cylinder again, it appeared, that the greateſt diſtance, at which the lightning (from the needle) ſtruck the rounded end, was not more than two inches and three quarters. And the largeneſs of the ſpark, as likewiſe the loudneſs of the exploſion, appeared to be leſs conſiderable than in the thirty-ſecond experiment.

The reaſon for fixing the needle at the end of the leſs ſubſtitute with the point downward, was to repreſent a fragment or jagged part of a cloud, which ſometimes hangs down towards the earth; and, as Dr. FRANKLIN and others have ſuppoſed, ſerves as a kind of ſtepping-ſtone for the lightning to paſs more readily, and in a ſilent manner, from a charged cloud to a a pointed conductor underneath it. But we ſee, from the two laſt experiments, the lightning does not paſs in a ſilent manner, becauſe the point below, as well as the rounded end, was always ſtruck, and the former at twice the diſtance nearly of that of the latter.

That two points oppoſed to each other, in the manner deſcribed above, ſhould ever occaſion a ſtroke of lightning, may perhaps appear ſtrange to thoſe who are not

very

very well acquainted with this ſubject. However, I have related it not only as it is a fact of a very curious kind, but as the conſequences which may be drawn from it ſeem to be conſiderable.

During the courſe of this inquiry, having occaſion to try ſome experiments in the dark, I obſerved a curious circumſtance, which ſeemed to ſhew, that a point had a far greater influence upon the charged ſubſtitute, in certain circumſtances, than a rounded end had when it was placed in the ſame ſituation.

EXP. XXXIV. The circumſtance alluded to was an appearance of light upon the braſs ball (for ſo I call it, to be more clear in the deſcription) that was fixed at the end of the great cylinder, when the copper ball (for ſo I call *that* for the ſame reaſon) of the leſs ſubſtitute was oppoſed to it at the greateſt ſtriking diſtance, as in the eighteenth experiment, every other circumſtance remaing the ſame; and whilſt the model, with its pointed conductor, ſtood upon the table directly under the tin ball that was fixed at the remote end of the leſs ſubſtitute: for ſoon after ſeven or eight turns of the wheel, a light began to appear on the braſs ball, and continued to increaſe in brightneſs till the moment it burſt forth in an exploſion towards the copper ball. The part of the braſs

on which the light appeared was that next to the copper ball: and the general appearance of it was round, and ſometimes more than half an inch in diameter. It did not ſend forth rays or ſtreams that were luminous; neither did it extend beyond the ſurface; or the diſtance to which it did extend was ſo inconſiderable, as to ſeem incapable of being aſcertained, even at the inſtant before the exploſion, when it was moſt vivid; though at that time there did appear ſomething like a ſmall ſwell towards the centre, as if it was making an effort to get out. Whenever the wheel was ſtopped ſuddenly, or the motion of it decreaſed, the light retired on the inſtant, and totally vaniſhed: but when the motion of the wheel was renewed for a little time, the light returned as before. The whole time that this appearance continued, was never more than five or ſix ſeconds: reckoning from the moment it was firſt ſeen, to the inſtant when the exploſion happened. The diſtance between the point and the tin ball meaſured three inches and a quarter. There was no ſuch appearance on the copper ball; nor is it eaſy to conceive how there ever ſhould, when all the circumſtances are taken into conſideration.

EXP. XXXV. Upon repeating this experiment with a rounded end, inſtead of a point, and at the ſame diſtance from the tin ball, notwithſtanding every other circum-

ſtance

ſtance continued the ſame, there was no ſuch appearance.

EXP. XXXVI. But when the rounded end was moved conſiderably nearer, that is, within ſix-tenths of an inch, a light was viſible; but then it was faint, and not more than one-tenth of an inch in diameter, even at the inſtant before the exploſion happened.

THIRTEENTH OBSERVATION. By the firſt of theſe experiments it appears, that the influence which the point had upon the whole of the fluid contained in the great cylinder, was ſuch as to cauſe a general tendency of it towards the leſs ſubſtitute; but, on account of the reſiſtance which ſeemed to operate at the ſurface of the braſs ball, it was there ſtopped, and by degrees accumulated, till ſuch time as the accumulation was great enough to overcome that reſiſtance. Now, according to this manner of reaſoning, the point did not draw the fluid out of the great cylinder ſilently; but when the accumulation had got to a ſufficient degree, a ſudden exploſion enſued, more or leſs violent, according to the circumſtances which accompanied the experiment.

FOURTEENTH OBSERVATION. From the other experiment it appears, that the rounded end had not ſo great an influence as the point upon the charge in the cylinder; becauſe we were obliged to bring it five times

nearer

nearer before any light could be perceived at all; and even then it was ſo faint and inconſiderable in its diameter (rather leſs than one tenth of an inch) compared with the other light produced by the influence of the point, that it manifeſtly confirmed the truth of the laſt obſervation.

Theſe facts being in my opinion ſo clear and ſatisfactory in regard to the great object we have had in view, I think that any farther experiments reſpecting the nature and uſe of conductors are unneceſſary. I ſhall therefore proceed to make ſome general deductions from what has been already related.

It ſeems to be clear, that in all experiments made with pointed and rounded conductors (provided the circumſtances be the ſame in both) the rounded ones are by far the ſafer of the two, whether the lightning proceed from one or more clouds; that thoſe are ſtill more ſafe, which (inſtead of being, as Dr. FRANKLIN recommends, ten feet high) are very little, if at all, above the higheſt part of the building itſelf; and that this ſafety ariſes from the greater reſiſtance exerted at the larger ſurface.

The luminous appearance at the end of the braſs ball, occaſioned by the point in the thirty-fourth experiment, manifeſtly ſhewed that there was an accumulation of the fluid within that part of the ball, in conſequence of ſome reſiſtance:

refiftance: for when the refiftance at the furface of the brafs ball was at laft overcome by the influence the point had upon the charge, the explofion took place immediately; and that, not only between the two fubftitutes, but alfo between the end of the lefs fubftitute and the point.

A cloud, therefore, that happens to be charged, and within the ftriking diftance of another cloud which is not charged, and alfo equally within the influence of a pointed conductor, muft neceffarily produce fimilar effects with thofe mentioned in the thirty-fourth experiment.

On the other hand, clouds that are circumftanced like thofe above, and not within the influence of a rounded conductor, will pafs quietly over fuch a termination, and without any explofion.

Nor can any one, at all acquainted with fubjects of this kind, want to be reminded, how far the effects of experiments, made within the limited power of fuch an apparatus, muft differ in degree from thofe which are exhibited in the great and wonderful phænomena of nature.

If I could grant to thofe who object to the motion employed in fome of thefe experiments, that a cloud charged with lightning, and motionlefs, may impend

over a building; it muſt nevertheleſs be allowed, that clouds are generally obſerved to move with conſiderable velocity; ſo that when the velocity is from three quarters of a mile or ſomething leſs, to four or five miles or more in an hour, the pointed conductor is always ſtruck by the charge contained in the great cylinder. And the experiments which have been made by the addition of the leſs ſubſtitute ſhew alſo, that no ſecurity can be expected from a pointed conductor when a thunder-cloud is even entirely at reſt.

UPON ACCELERATION, AND ITS EFFECTS.

From conſidering the extraordinary effects which have ſometimes been produced upon groſs matter by lightning, and the diſtance there frequently is between thunder-clouds and the earth, when ſuch effects take place, I ſuſpected that thoſe effects might in ſome degree be owing to an increaſe of the velocity of the fluid which produced them.

To try whether this was really ſo, it ſeemed neceſſary to have an apparatus of a far greater length than the great cylinder: I therefore made uſe occaſionally of the long wire which has been already deſcribed.

EXP.

EXP. XXXVII. Upon connecting one end of this long wire with the farther end of the great cylinder, and the other with one end of the brass drums; I found, that about six uniform turns of the wheel, with a moderate velocity, were required to cause the appearance of a small stream of light at the top of the spout described in the first experiment, when the model, with the pointed conductor upon it, stood directly under the great cylinder, but at the distance of five inches.

EXP. XXXVIII. When the great cylinder was unconnected with the long wire and brass drums, and whilst the model, with the same conductor upon it, remained in its place; about two turns, with the same velocity, were sufficient to charge the great cylinder, so as to cause a similar appearance at the spout.

EXP. XXXIX. On separating the great cylinder from a fourteenth part of it (which fourteenth part for the present I call the little cylinder) the model and machine continuing in their places, it was found, that half a turn of the wheel was sufficient to charge the little cylinder, so as to cause the like appearance at the spout.

FIFTEENTH OBSERVATION. Now these differences in the numbers of turns required for causing similar appearances, when the several charges were given in the thirty-seventh and thirty-eighth experiments, could not arise

arife from a difference in the quantity of metallic matter contained in the refpective fubftitutes; becaufe the tin-foil which covered the great cylinder (independent of the nails and wood it contained) was found to be near three times heavier than the weight of the whole wire.

Neither could thefe differences be owing to a difference in the quantity of furface of the refpective fubftitutes; becaufe the furface of the great cylinder was found to be ten times greater than the furface of the wire.

Thofe feveral differences muft therefore depend upon fome other caufe; and, as a true knowledge of this caufe may be of fome moment in the prefent inquiry, we muft endeavour to find it out by experiments and obfervations.

To this end it may be neceffary, that our inquiry fhould begin from an early part of this fubject, fo that we may proceed regularly, ftep by ftep, as nature directs. By purfuing this method (though it is propofed to be done very generally on account of the length of this paper) we may poffibly arrive at the knowledge of the caufe in queftion, and fhorten the road to the main object in view; I mean, the effects of comparative velocities.

From the nature of this fubtile and elaftic fluid, and its being diffufed throughout the whole earth as well as

the air ſurrounding it, the leaſt violence exerted muſt neceſſarily diſturb it.

And although experience hath taught us how to vary the natural quantity of this fluid in many ſubſtances, in conſequence of violence; yet the ſame experience hath likewiſe taught us, that we cannot increaſe that quantity in any particular ſubſtance, without taking it from the general ſtock contained in the earth or air ſurrounding it: and therefore, when this fluid is ſo increaſed, it may be properly ſaid to be in an unnatural ſtate; and whilſt it remains ſo, muſt (from its elaſtic principle) be always endeavouring to recover its natural one.

But experience hath alſo taught us, that metal, for example, hath a property of receiving the fluid more readily, whenever it is diſturbed, than moſt other ſubſtances: for which reaſon a notion hath prevailed with many, that this property of metal ariſes from a power of attraction, which they ſuppoſe it poſſeſſes in a greater degree than any other ſubſtance.

If this philoſophy were true, it would follow, that the ſame power which attracted the fluid into the metal ought to keep it there: for it cannot be ſuppoſed to attract the fluid at one time, and then let it go at another; this would be abſurd, and contrary to experience. We muſt therefore try to find a better reaſon why metal is poſſeſſed

with the property of receiving this fluid more readily than other ſubſtances.

EXP. XL. When the great cylinder, with the wire and braſs drums, were charged with a very ſmall quantity of this fluid, by the wheel being turned ſomething leſs than a quarter round, there was, the moment after, a viſible exploſion, and a ſenſible effect perceived at the remote end of the wire. When half a turn was given, theſe effects were greater; and, after a whole turn, the quantity of the fluid accumulated in this great apparatus was increaſed conſiderably.

SIXTEENTH OBSERVATION. Now, ſomething muſt have hindered the fluid from getting out of the cylinder and wire all the time they were charging, otherwiſe we ſhould not have been able to have cauſed the leaſt accumulation; for, from the nature of this fluid, there cannot be any accumulation without ſome reſiſtance to occaſion it. And whatever the nature of that reſiſtance may be, experiments ſhew, that there are certain bounds preſcribed to its power of acting, and which in particular circumſtances ſeem to be very eaſily ſurmounted.

From Sir ISAAC NEWTON's obſervations[c], and a great variety of experiments made ſince his time, we collect, that this principle of reſiſtance is probably exerted at, or

(c) NEWTON's Optics, p. 240, 241, and 372.

very

very near, the furfaces of bodies, and extends only to very fmall diftances from them.

It feems then, that it is by this kind of refiftance at the furface of bodies, that the fluid is prevented from efcaping out of the great cylinder and wire, whilft it is accumulating within them: and therefore, when we begin to charge the great apparatus at the nearer end, the moment any part of the charge arrives at the farther end of the wire, it is prevented (in fome degree at leaft) from efcaping, in confequence of the refiftance it meets with at that end. And, if we continue to make the charge greater, the charge itfelf, during its increafe, muft alfo refift every farther effort which any ways tends to make it greater, with a force probably proportional to the quantity accumulated.

EXP. XLI. When the great cylinder and wire with the drums were fully charged, and a perfon, ftanding upon the wire which communicated with the well, fuddenly approached the brafs drums with his hand, an explofion enfued, which indeed was neither fo large, nor did it take place at fo great a diftance, as might have been expected[(d)]; neverthelefs, the perfon received a violent fen-

(d) The circumftances attending the explofion in this experiment were certainly owing to the long wire being not entire, but confifting of feveral pieces twifted together, the ends of which, being very many, muft have caufed a confiderable part of the fluid to efcape, and fo have weakened the general effect.

ſation, not unlike that produced by the Leyden phial, as it affected his body quite through, from the hand that took the diſcharge to the feet that ſtood upon the wire.

EXP. XLII. Upon repeating the above experiment, with the great cylinder only, and when it was fully charged, the exploſion appeared ſtronger, and the diſtance it ſtruck at greater, than in the other caſe; and yet the ſenſation received was not near ſo violent as when the long wire was connected with it.

EXP. XLIII. When the little cylinder by itſelf was fully charged, the effects were very inconſiderable, compared with thoſe from the great cylinder: for, in this caſe, the perſon ſtanding upon the wire of communication was affected in his hand only, and that no farther than the wriſt. Theſe three experiments were repeated many times by different perſons, and the reſults were nearly the ſame.

SEVENTEENTH OBSERVATION. When all the circumſtances in the two laſt experiments are conſidered, we may ſafely conclude, that the difference in the ſenſation, produced by the two cylinders, could ariſe from no other cauſe than a difference in their lengths; the one being fourteen times longer than the other, and both in other reſpects nearly ſimilar; and ſince the ſenſation perceived

in

in the thirty-eighth experiment, where the long wire was employed, was confiderably greater than when the great cylinder alone was charged, we feem to have fufficient reafon to apprehend that the effects of every charge, as to fenfation, will be proportional to the length of the body charged; provided the charge (or accumulation) be uniform from end to end in every experiment.

Apprehending that, if fome of the circumftances employed in producing the charge were varied, we might poffibly obtain a greater charge than we had yet found, I made the following experiments.

EXP. XLIV. Inftead of one machine to charge the great apparatus I made ufe of two. The glafs cylinders belonging to each were of the fame length and diameter nearly. One of thofe machines was continued in its ufual place, which was not far from the nearer end of the great cylinder. The other ftood at the farther end of the brafs drums. After connecting the long wire with the great cylinder and brafs drums, in the manner before defcribed, the wheels of both machines were put into motion, with equal and uniform velocities: and after fix turns of each wheel (for I could not prevail upon any one prefent at the time to take a higher charge), and, after waiting above eight feconds, a perfon fud-

denly

denly approached the braſs drums with his hand; immediately an exploſion took place, and a diſagreeable ſenſation was perceived. The diſcharge was then made at the nearer end of the great cylinder, and there ſeemed to be no difference in the effect.

EXP. XLV. Upon repeating the experiment with one machine only, and after the ſame number of turns of the wheel with the ſame velocity, and after waiting above eight ſeconds alſo, the ſame perſon ſuddenly cauſed an exploſion with the ſame hand. But the ſenſation in conſequence of it was very different from the laſt experiment; for he declared it was little more (if that) than half as violent. Theſe laſt experiments were alſo repeated ſeveral times by another perſon, who gave the ſame account of the reſults[e].

EXP. XLVI. I now charged the long wire only and fully, and with one machine: the exploſion, in this caſe, appeared not very large, but of a reddiſh hue; and the diſtance it ſtruck at was not more than one inch and an half; however, the ſenſation acroſs the body was at that inſtant

(e) On re-conſidering theſe two laſt experiments I find, that inſtead of giving ſix turns to the wheel, in the laſt experiment, I ought to have given twelve, in order to make any compariſon between the effects of the two experiments: I therefore ſimply relate the experiments as they were made, without making any deductions from them.

ſharp

XIX. *Journal of a Voyage to* The East Indies, *in the Ship* Grenville, *Captain* Burnet Abercrombie, *in the Year* 1775. *By* Alexander Dalrymple, *Esq. F. R. S.* *Communicated by the Honourable* Henry Cavendish, *F. R. S.*

Read January 29, 1778.

EXPLANATION OF THE COLUMNS.

1st, THE *date*[(a)].

2d, The *height* of the *thermometer*, according to FAHRENHEIT's scale. This thermometer belonged to Mr. RUSSELL, and hung in the open air in the balcony.

3d, including four columns, contains the *register* of the *marine barometers*, all of which, as well as the thermometers, were made by NAIRNE and BLUNT: those marked R and D are quicksilver, of the kind usually made by them. That marked S is compounded of quicksilver, and of a lighter fluid, for the purpose of making the alterations more visible, which is a very great convenience at sea; a quicksilver thermometer being fixed to it for the sake of correcting its height, the heat by which is set down in the column marked Th. next to that marked S.

4th, The *weather* and *winds* in four lines; 1st line from noon to 6 P.M.; 2d, from 6 P.M. to midnight; 3d, from midnight to 6 A.M.; 4th, from 6 A.M. to noon.

In the column of *weather*, f. denotes *fair*; sq. *squally*; c. *cloudy*; h. *hazy*; r. *rain*; hr. *hard rain*; sr. *small rain*; dr. r. *drizling rain*; sh. *showers*; th. *thunder*; l. *lightning*.

It is proper to remark, that the *winds* are set down according to the *compass*, without any allowance for the *Variation*.

5th in 2, The *difference* between the daily alteration of *latitude* by *account* and *observation*; N. denoting that the *observation* was to the *Northward* of the *account*; S. that it was to the *Southward*[(b)].

(a) Here, for want of room, the day of the month only is expressed; but in the original journal the days of the week, and of the Moon, are also inserted.

(b) Next to these columns in the original journal are the following, which are left out here only for want of room:
Correct course, lee-way variation &c. allowed, and the different courses reduced to one straight course.
Distance on that straight course.
Difference of latitude by account,
Difference of latitude by observation,
The departure,
The difference of longitude by account,
The difference of longitude by the time-keeper,
} in minutes of a degree.

6th in 2, The *difference* between the daily alteration of *longitude* by the *account* and *time-keeper*; W. denoting that the *longitude* by the *time-keeper* was to the *Westward* of *account*; E. that it was to the *Eastward*.

The result of those differences indicates the daily effect of current; however an error in the course sailed, or distance run by log, would make the current appear different from what it really was.

7th, The *longitude* from Greenwich, in seven columns.

1st, The longitude by *account*.

2d, The longitude by the *time-keeper*, which was made by ARNOLD, but without his late improvements.

3d, The *difference* between the *longitudes* deduced from *observations* of the *Moon* and from the *time-keeper uncorrected*; E. denoting the time-keeper to be to the East of ☾; W. denoting time-keeper to West of ☾. This, admitting the *time-keeper* not to be liable to any sudden changes in its rate of going, indicates the precision with which the observations of the Moon may be relied on, all circumstances of weather and of the ship's motion considered.

4th, The *longitude* by *observations* of the *Moon's distance* from the *Sun* or *Stars*, adjusted, by the log, to the noon nearest the time of observation.

5th, The number of *sights* or *distances observed*.

6th, The *object* whose *distance* from the *Moon* was *observed*; ☉ denoting the Sun; ✱ the Star; S. Spica Virginis; R. Regulus; A. Aldebaran; At. Atair; P. Pollux; F. Fomalhaut; An. Antares.

7th, The *extreme difference* between the *highest* and *lowest observation*; expressed in minutes of a degree; when the *seconds* amount to *more* than 30, the next minute *above* is taken, *otherwise* the next minute *below*.

8th, The *latitude* in two columns:

1st, The latitude by *account*, carried on from the *land*, in the same manner as the *longitude* by *account*.

 2d,

2d, The latitude by *obſervation*; and where the *latitude* could not be had by *obſervation*, it is deduced by *account* from the *laſt obſervation*, in which caſe it is included within [ ].

9th, The *correct longitude* from Greenwich deduced from the *time-keeper corrected* by the *ſight* of *lands*, whereof the *longitudes* are known, and by *obſervations* of the *Moon*, taking a *mean* of the ſeveral obſervations of the *Moon* made within a ſhort period of each other. The *error* of the *time-keeper*, between the *longitude corrected* by *ſight of land* or *obſervations of* the *Moon*, is ſuppoſed to have ariſen by the *time-keeper* having altered its rate of going *uniformly* between theſe obſervations; and the *intermediate longitudes* are determined by the *time-keeper* on this ſuppoſition. Where no obſervations of the *time* were made, it is deduced by the account from the *laſt* obſervation of the *time*, and is then included within [ ].

10th, The *magnetical obſervations* of the *variation* and *dip*, in ſeven columns.

1ſt, } The varia- { azimuth, } * *before*, denoting the obſervation to have been in
2d, } tion by { amplitude, } the *morning*; * *after*, denoting the obſervation to have been in the *evening*. The *variation* was obſerved by the officers with the *compaſſes* belonging to the ſhip.

3d, The *dip* with the *face* of the *inſtrument* to the Eaſt.

4th, Ditto, ditto, ditto, Weſt.

5th, The *mean dip* of the foregoing obſervations.

6th, The *mean corrected*, or what is *ſuppoſed* to be the *true dip*.

7th, The *circumſtances* under which the *obſervations* of the *dip* were made.

12th, The *miles* run by *log*.

The *dip* was obſerved with a *dipping-needle* belonging to the hon. Mr. CAVENDISH, made by SISSON.

The following *remarks* on the *dipping-needle* and *obſervations* are by Mr. CAVENDISH.

The ends of the axis of the dipping-needle are made conical, and turn in conical holes of bell-metal, in the manner of Mr. LORIMER's needle, deſcribed in Phil. Tranſ. vol. LXV. p. 79. The dip was conſtantly obſerved both with the face of the inſtrument to the Eaſt and to the Weſt, and the poles were changed twice during the voyage, in order to ſee whether the needle continued well balanced. The uſe of this method of obſerving is explained in Phil. Tranſ. vol. LXVI. p. 396.

The mean dip corrected is what is ſuppoſed to be the *true dip*. The foundation of this correction is as follows.

By the obſervations on July 12th, when the poles were changed, it appears, that the marked end of the needle was too heavy, ſo as to make that end point $\frac{15}{16}$ths of a degree too low at that place; and therefore, if we ſuppoſe that the force of magnetiſm is equally ſtrong in all parts of the earth, the error produced thereby in other places ſhould be to $\frac{15}{16}$ths of a degree as the coſine of the dip to the radius. The obſervations alſo made when the poles were changed at Suez, agree well enough with this ſuppoſition: therefore, as in all the obſervations the marked end of the needle pointed to the North, the mean dip in all that part of the voyage ſubſequent to July 12th is corrected by ſubtracting

$\frac{4}{8}$	of a degree from the mean dip, when the mean dip is between	63° and	52°	North, and adding as much when the dip is as much South.
$\frac{5}{8}$		52	43	
$\frac{6}{8}$		43	30	
$\frac{7}{8}$		30	0	

But as before the needle left London, changing the poles was found to make very little difference in the dip, the correction in the preceding part of the voyage is made not ſo great, *videlicet*,

$\frac{1}{8}$	of a degree is ſubtracted from the mean dip, when that dip is between	70° and	62°
$\frac{2}{8}$		62	52
$\frac{3}{8}$		52	43
$\frac{4}{8}$		43	35
$\frac{5}{8}$		35	30
$\frac{6}{8}$		30	25
$\frac{7}{8}$		25	0

The dip was obſerved on board the Grenville at Deptford, after her return, in the ſame part of the ſhip in which the obſervations were uſually made, and was found not to differ more than 5′ from that obſerved with the ſame needle in a pretty large garden in London, about five miles diſtant; ſo that the obſervations on board the Grenville ſeem to be not much influenced by the iron-work of the ſhip.

ſharp (as it was expreſſed) and violent, but not quite ſo diſagreeable as when the great cylinder was connected with it, and ſimilarly charged.

EXP. XLVII. Having procured an equal quantity of the ſame kind of wire, and of the ſame diameter, with that which was ſuſpended and tried in the laſt experiment, it was placed in the form of coils upon a board, fixed on the top of a long ſtand of glaſs, without having any connection whatſoever with the great apparatus, or any part of it. Theſe coils were then fully charged by the power of one of thoſe machines only. The ſenſation they afforded, in conſequence of cauſing ſparks, was very inconſiderable, compared with what had been obſerved in the laſt experiment.

EXP. XLVIII. The ſeveral coils of wire employed in the laſt experiment, as likewiſe ſeven hundred yards more in coils alſo, were joined together at their ſeveral ends. Theſe coils being then ſtrung upon ſilk lines, were drawn out into a form reſembling that of a ſcrew, and ſeparated from each other in ſuch a manner all along as to occupy one hundred yards of ſilk line. The ſeveral diameters of theſe coils, at a mean, were about fifteen inches. As I had ſo ſhort a time in which to prepare and ſuſpend them properly, the diſadvantage of their touching and interſecting each other in many places

could not be prevented; however, I found that the ſenſation cauſed, after charging this wire, was nearly equal to that which had been experienced from the long wire in the forty-ſixth experiment.

EXP. XLIX. Upon joining the farther end of theſe coils to one end of the long wire, ſo that the whole length was in this experiment about three thouſand nine hundred yards, and afterwards charging the nearer end of the coils, and without the great cylinder (it being at that time taken down) the ſenſation complained of, by two indifferent perſons, was twice as violent as the ſenſation perceived by the ſame perſons when the long wire alone was charged.

It may be now proper to make ſome general obſervations reſpecting the *exploſion* itſelf, and the quantity of the fluid diſcharged in conſequence thereof.

After many experiments we found, that when the great apparatus was fully charged, and the motion of the wheel ſuddenly ſtopped, it appeared, that a ſingle exploſion at either end of it, inſtantly (as to *ſenſe*) diſcharged the fluid contained therein; but never ſo effectually as to leave no remainder: for the quantity which did remain (upon a ſecond application immediately afterwards) was generally ſufficient to cauſe a ſecond explo-

ſion

fion perceptible to the fenfe of feeling, as well as to that of fight.

Now, before the great explofion was caufed, the fluid accumulated in the apparatus muft have been diffufed equally through it, in confequence of its elaftic principle; and, being fo circumftanced, a fudden application of the hand, or any other fubftance, which would open a door for the paffage of the fluid into the earth, was found to difcharge the greater part of that fluid: and whatever part thereof was fo difcharged, the moft diftant particles feemed to have arrived at the point where the explofion took place, at the fame time with thofe that were the neareft to it; becaufe, immediately after the explofion, there was very little of the fluid remaining in the apparatus.

If then the difcharge of the fluid be, as it feems to be, very nearly inftantaneous, the particles of it muft move with velocities, and confequently with forces, very nearly proportional to thofe diftances.

From this confideration I apprehend it will appear, why the fenfation upon the difcharge from the long wire in the forty-fixth experiment was more violent: and upon recollecting the thirty-feventh, thirty-eighth, and thirty-ninth experiments, and the obfervations I made upon them, I am inclined to believe, that the effect

depends more upon the length of the metallic body, than upon the quantity of its matter or furface.

It was upon the idea of the velocity of the fluid being thus increafed, that I apprehended gun-powder might be fired without the leaft appearance of a fpark. The fuccefs of this experiment was an inducement to try KUNKELL's phofphorus, which was made by Dr. HIGGINS. The moment this inflammable fubftance was brought very near the furface of the brafs drums, it burft into a blaze; and common tinder, applied in the fame manner, was fet on fire the inftant when it was brought fo near as to touch the metal: but there was not the leaft appearance of a fpark in any of thefe experiments.

The method taken to fire the gun-powder was this: upon a ftaff of baked wood a ftem of brafs was fixed, which terminated in an iron point at the top. This point was put into the end of a fmall tube of Indian paper, made fomewhat in the form of a cartridge, about one inch and a quarter long, and about two-tenths of an inch in diameter. When this cartridge was filled with common gun-powder (unbruifed) the wire of communication with the well was then faftened to the bottom of the brafs ftem. Being fo circumftanced, and whilft the charge in the great cylinder and wire was continually kept up by the motion of the wheel, the top of the cartridge was brought

fo

ſo near to the drums as frequently to touch the metal. In this ſituation, a ſmall faint luminous ſtream was obſerved between the top of the cartridge and the metal drum.

Sometimes this ſtream would ſet fire to the gun-powder at the inſtant of the application; at others, it would require half a minute or more before it took effect. But this difference in time might probably ariſe from ſome difference in the circumſtances, for any the leaſt moiſture in the ſilk lines, the powder, or in the paper itſelf, was unfavourable to the experiment.

This new method of firing gun-powder by a luminous ſtream of the matter of lightning ſurely merits the moſt ſerious attention; and more eſpecially in thoſe caſes where pointed conductors are fixed to ſecure magazines of gun-powder from ſuch accidents.

In repeating the laſt experiment, there were one or two inſtances where the powder was fired without making uſe of the long wire; but never with the great cylinder alone: for we were obliged to connect it with the braſs drums by means of a wire ten or twelve feet long.

It was, however, a conſiderable time (ten minutes or more) before the experiment ſucceeded even with this laſt apparatus. This was not the caſe with tinder; for it fired

fired pretty readily, and ſometimes with the great cylinder alone.

EXP. L. Before the apparatus was taken down in the Pantheon, attempts were made ſeveral days to fire gun-powder, but without ſucceſs, except in one inſtance, which was attended with ſome difficulty. This failure ſeemed to be owing to a variety of cauſes, the chief of which appeared to be moiſture that affected the ſilk lines, and perhaps the powder itſelf. I might add alſo the duſt, which in ſo long a time had ſettled upon the lines, and rendered them in ſome degree incapable of reſiſting the paſſage of the fluid. However, when the coiled wires upon the ſilk lines were properly joined to the long wire, as in the forty-ninth experiment, it was found that gun-powder could be fired very readily. This is another proof of the increaſe of velocity by increaſing the length of the wire.

The exploſion of gun-powder, in the particular manner related above, without the aſſiſtance of the Leyden charge, and even without a ſpark, is an effect which could not eaſily be deduced from reaſoning upon any experiments hitherto made; for though gun-powder has been frequently fired with the Leyden charge, yet there is a difference in the two caſes, which makes it neceſſary to take ſome farther notice of that experiment.

When

When the Leyden phial is charged we find, by experiment, that the charge is confined in a ſmall compaſs, or very much condenſed, near one ſurface of the glaſs. It is alſo true, that the oppoſite ſurface of the ſame glaſs is as much rarified, or (according to Dr. FRANKLIN, who was the firſt that obſerved it) in a *minus* ſtate. Now, according to this rule, the greater the ſurface of the glaſs is, and of an even and limited thickneſs, the greater charge may be given; and the greater the charge, the greater muſt be the effect (whenever the diſcharge is properly made) to reſtore the natural ſtate of the glaſs on the two oppoſite ſides. In order therefore to fire gun-powder with this kind of apparatus, experience hath taught us hitherto, that the powder ſhould be confined, for example, in a tube or cartridge; and that this cartridge ſhould be placed in ſuch a manner as to make part of the circuit neceſſary for the diſcharge of the fluid from one ſurface of the glaſs, and carry it through the cartridge to the other ſurface. But it is alſo neceſſary, before the powder can be fired, to have part of the wire which forms the circuit in contact with the powder; or, to have one end of the wire, which makes part of the circuit, forced a little way into one end of the cartridge; and another end of a wire, which alſo makes part of the circuit, forced a little way into the other end of the cartridge; but ſo as that the

two

two ends of the wire within the cartridge may be at a proper diſtance from each other; which diſtance will depend upon the ſtrength of the charge. When every thing is thus adjuſted, and the circuit properly made, the gun-powder is generally fired; I ſay generally, becauſe it ſometimes happens, that the charge is not great enough to produce the effect required. For this effect does not appear to proceed from the ſpark or the exploſion produced by the fluid (becauſe flame of a certain denſity will not fire gun-powder); but it muſt be from the ends of the two wires, or from one of them at leaſt, within the cartridge, being rendered hot enough to fire the gun-powder, in conſequence of the very great quantity of the fluid, and the velocity with which it paſſed through the wire at that moment: it being well known that wire, even of a conſiderable thickneſs, has been frequently made red-hot, and even melted, by the Leyden charge.

Dr. LIND, a gentleman who is well acquainted with this ſubject, favoured me with a ſight of a very curious experiment, which ſeems to ſhew more clearly what has been advanced above, reſpecting the immediate cauſe of the gun-powder's taking fire in the Leyden experiment. He procured for the purpoſe ſome extraordinary fine threads or ſhavings of ſteel; one of which was ſo diſpoſed, where gun-powder was properly lodged and confined, as

to

to be in contact with the powder. After having charged a ſmall phial, which did not exceed a pint in meaſure, with only a few turns of the wheel, he made the diſcharge, and fired the gun-powder. The advantage he gained by having ſo ſmall a thread of metal in this experiment was, that it could be made red-hot with a leſs charge than what is neceſſary when a thicker wire is made uſe of; and the ſmallneſs of the charge he employed to fire the powder was a certain proof that the ſteel thread had been made red-hot.

Upon the whole we find, that this method of firing gun-powder is totally different from the other, where the great apparatus was employed; becauſe the faint luminous ſtream, obſerved at the braſs drums where the gun-powder was applied, I found was abſolutely incapable of making the point of metal within the cartridge red-hot. Beſides, there did not appear to be any exploſion whatſoever between the apparatus and the powder at the inſtant it was fired.

Before I conclude this paper, it may not be amiſs if I take notice of certain atmoſpheres, which bodies have round them when they are properly charged with this elaſtic fluid; becauſe the nature of them may not perhaps be perfectly underſtood by every one who attends to enquiries of this kind.

Every charge, from the nature of the fluid which produces it, muſt, while ſuch charge remains, continually act upon the air ſurrounding it; and of conſequence upon the fluid alſo, which ſtands diffuſed therein, and in the intervals between its parts by its repulſive principle. This appears to be true, not only from theory but experiment.

The ſtate of the fluid in the air neareſt to the cylinder that is charged, muſt therefore be in an oppoſite ſtate to that which is in the cylinder; that is, in the one caſe it will be condenſed, and in the other rarified.

Now, becauſe the power ariſing from the charge within the cylinder, which cauſed the rarefaction beyond its outward ſurface, is limited; the diſtance to which that rarefaction extends, muſt be limited alſo: and beyond that diſtance the fluid muſt be condenſed more or leſs, according to the power of the charge which cauſed the rarefaction.

Whenever therefore we are diſpoſed to open a door to let the charge out of the cylinder, the rarified fluid near the outſide of the cylinder muſt neceſſarily promote the diſcharge at that inſtant.

As to the diſtance to which this rarefaction extends, it will always depend upon the ſtrength of the charge, and

the ſtate of the air in the place where the experiment is made.

When I ſuſpended the 1600 yards of wire in the Pantheon, the ſeveral lengths thereof were purpoſely hung (as hath been obſerved before) at five or ſix feet diſtance from each other, entirely upon this idea; leſt the reſpective atmoſpheres might interfere with each other, or with the charge contained in the wire, and by that means diſturb the experiment.

It is now time to put an end to this inquiry; in which, however defective I may have been in ability, neither attention nor impartiality have been wanting; and although it may be true that, even before theſe experiments were tried, I was inclined more to one ſide of the queſtion than the other, it was becauſe I many years ago grounded my perſuaſions upon the philoſophy of Sir ISAAC NEWTON. Without thoſe perſuaſions, and the advantage of that moſt gracious encouragement which is never wanting to honeſt and candid endeavours in the purſuit of philoſophical truth, I ſhould hardly have felt ſufficient zeal to engage in ſo conſiderable an undertaking: nor indeed, without powerful aſſiſtance, could the great object of this inquiry have been in any degree attained. For my own part, I can boaſt of very little more in this inveſtigation than patience and induſtry.

I ſhall only add, that if any one, who may be diſpoſed to try the preceding experiments, will uſe an apparatus of ſufficient dimenſions, and faithfully attend to every circumſtance as I have done, I have no doubt they will find the ſame reſults; but they muſt alſo apply their obſervation to much ſmaller circumſtances than have been here ſpecified, as the intervention of a ſingle hair, a fibre of down, or even a little vapour ariſing from perſpiration or otherwiſe, will, where great exactneſs is required, ſometimes prevent the ſucceſs of an experiment; and by that means miſlead the obſerver, or afford a ſubterfuge for a miſtaken hypotheſis.

Great Ruſſel-ſtreet, Bloomſbury,
November 12, 1777.

N. B. The different diſtances at which the point and ball were ſtruck being expreſſed in fractions, p. 278. I thought it would be better to put theſe diſtances in whole numbers, that the ratio between them may more clearly appear.

Sharp point.	Rounded end.
90	25
160	20
130	35
220	16
260	20
80	4

Forty of thoſe parts are equal to one inch.

Meaſure-

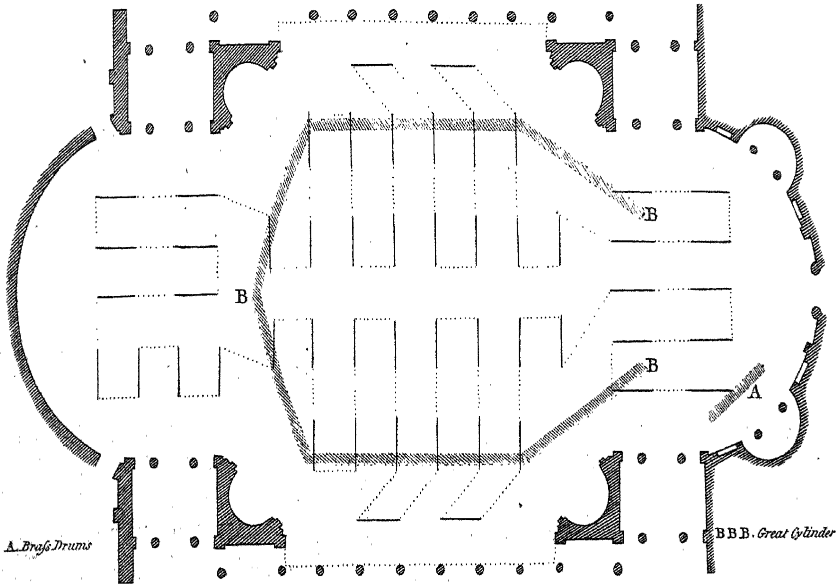
Meaſurements, &c. *of the great Apparatus and Machinery.*

	Feet.	In.	lbs.
Length of the wire ſuſpended, which conſiſted of many pieces that were connected by twiſting the ſeveral ends together, - -	4800		
Weight of the wire, - -			30
Twenty-one pieces of this wire, laid parallel and cloſe to each other, meaſured exactly, - -		1	
Surface of the whole wire in ſquare feet,	57		
Length of coiled wire, which was ſuſpended alſo, but in a form ſomewhat reſembling a ſcrew and of the ſame thickneſs with the wire above,	6900		
Length of great cylinder, including the braſs drums above, -	155		
Diameter of this cylinder, -	1	4	
Weight of tin-foil, which covered 112 drums, - -			87
Surface of 112 drums in ſquare feet (including the ſix ends) meaſured,	592		

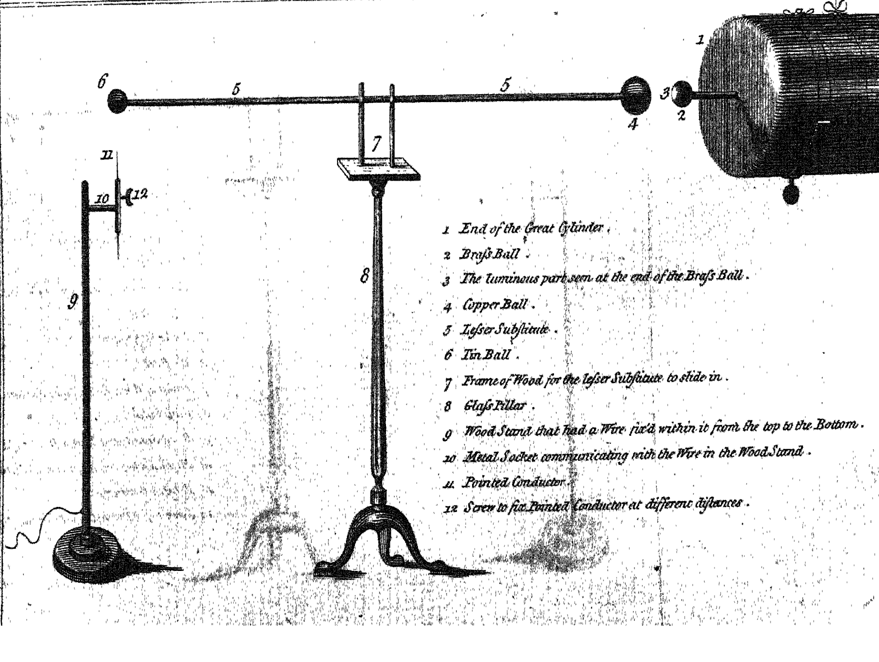
	Feet.	In.	lbs.
Space through which the conductor upon the model paſſed from its beginning to move, to the inſtant nearly of its being ſtruck, -	7	7	
Heighth of the frame from the floor,	3	1$\frac{1}{2}$	
Breadth of the frame, - -	1	3	
Heighth of the two poſts, on the top of which were fixed two wheels,	10	6	
Diameter of the great wheel, -	1	11	
Diameter of the ſmaller wheel, fixed upon the ſame axis, -		11$\frac{3}{4}$	
Length of frame on which the model moved, - - -	18	2	
Diameter of the pulley that was fixed between the two poſts, which delivered the line from the model to the greater wheel, the center of which pulley was two inches and an half above the center of reſiſtance in the model, -		5	
Greater weight to draw the model,			32
Smaller weight, - - -			4
Weight of the model, - -			12

Upon

Disposition of the long Wire, Brass Drums & Great Cylinder, as suspended in the Pantheon.

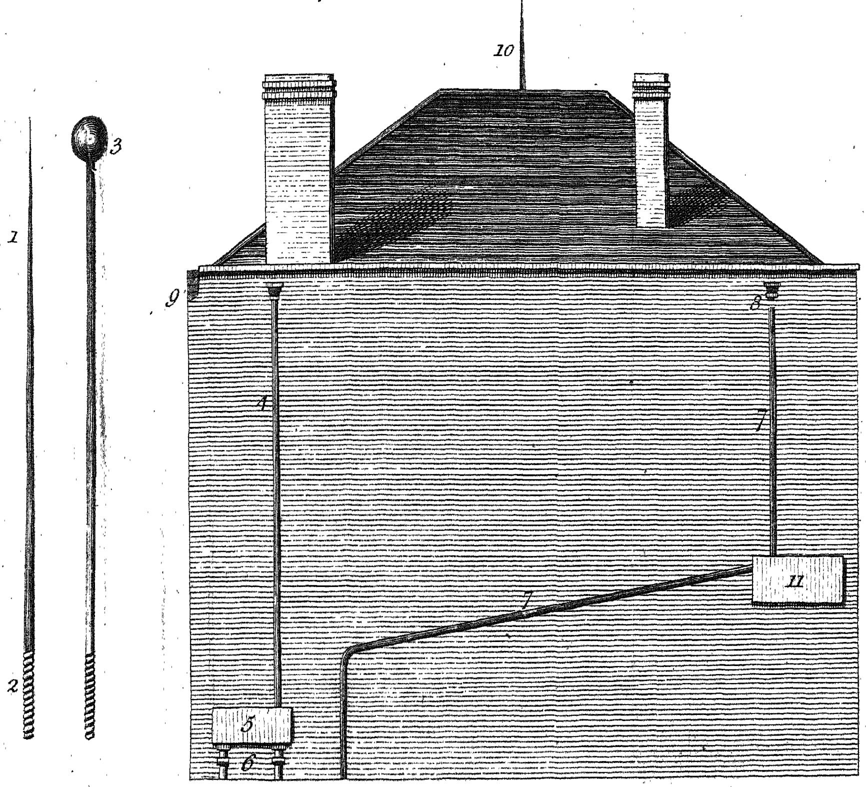


The dotted lines represent the lower, the black lines the upper horizontal parts of the Long Wire.
The perpendicular parts could not be represented in this plate and must therefore be supplied by the Readers Imagination.



1 *End of the Great Cylinder.*
2 *Brass Ball.*
3 *The luminous part seen at the end of the Brass Ball.*
4 *Copper Ball.*
5 *Lesser Substitute.*
6 *Tin Ball.*
7 *Frame of Wood for the lesser Substitute to slide in.*
8 *Glass Pillar.*
9 *Wood Stand that had a Wire fix'd within it from the top to the Bottom.*
10 *Metal Socket communicating with the Wire in the Wood Stand.*
11 *Pointed Conductor.*
12 *Screw to fix Pointed Conductor at different distances.*

The North Elevation of the House in which the Board of Ordnance meet call'd, for that reason, the Board House.



1. Size of pointed Conductor as on the Model.
2. The part Inserted at the top of the Roof.
3. Size of rounded end, as on the Model.
4. Short Spout.
5. Cestern.
6. Two Posts.
7. Long Spout.
8. Little discontinuation of Metal not in the House but left in the Model for the purpose of making Experiments.
9. The Corner damag'd by Lightning.
10. Pointed Conductor. 11. Cestern.

Upon a ſcale of one-third of an inch to a foot.

	Feet.	In.	lbs.
Length of the model, -	1	$9\frac{1}{4}$	
Breadth, - -	1	$4\frac{1}{6}$	
Heighth to the top of the parapet,	1	$1\frac{11}{12}$	
Roof above the parapet, -		$4\frac{2}{3}$	
Chimnies exceeded the higheſt part of the roof, about		$\frac{1}{6}$	
Pointed conductor when fixed upon the roof, -		$3\frac{1}{3}$	
Length of ſhort ſpout, -		10	
Length of long ſpout in its bent ſtate, - -	1	$10\frac{3}{4}$	
The two poſts or pillars to ſupport the ciſtern (which ciſtern was lined with lead) belonging to the ſhort ſpout,		$\frac{5}{8}$	

5. *A Report of the Committee, appointed by the Royal Society, to conſider of the moſt effectual method of ſecuring the Powder Magazines at* Purfleet *againſt the Effects of lightning; in compliance with the Requeſt of the Board of Ordnance.*

IT being referred to us to conſider, whether any thing more than what was formerly directed by the committee of the Royal Society in the year 1772 can be done,

done, for the preſervation of His Majeſty's magazines at Purfleet: we, having attentively examined the experiments and obſervations of Mr. WILSON, contained in a paper referred to the Society by the Board of Ordnance; and having maturely conſidered the ſubject at large, ſubmit it as our opinion.

1. That it is very improbable, that the powder magazines, guarded in the manner in which they are at preſent, ſhould receive any damage from lightning.

2. That they would be ſtill leſs liable to be injured if three other elevated pointed rods, ſimilar to thoſe already erected, were to be fixed upon the roof of each of the five magazines, between the extreme rods, at equal diſtances from each other, with three ſtrips of lead, about one foot in breadth, ſtrongly connected with them, and carried down the roof, from the ridge to the eaves, on each ſide of the building; thence two of them to be continued into the earth, and to terminate at the bottom of wells; one of which ſhould be dug for that purpoſe nearly in the middle of each of the intervals between the magazines, deep enough to contain at leaſt four feet of water. The middle ſtrip ſhould be connected with the iron rod over the door, hereafter to be mentioned.

We alſo adviſe, that other high pointed rods be erected; one at each of the four corners, and one over each

each of the metal doors in the middle of the fides; which latter fhould be bent, fo as to avoid the doors, in the fame manner as thofe which are already placed upon the outward fide of the outermoft magazines. All which rods fhould be continued into the earth, and be made to communicate with the bottom of the water of the neareft wells, by means of leaden pipes, clofely connected with thefe iron rods. Likewife, that ftrips of lead be put upon the copeing of the end walls, and be made to communicate with the rods to be placed at the feveral corners as above directed.

3. But that the greateft degree of fecurity would be attained by covering the whole roof, and the tops of the end walls of each of the five magazines, with lead; erecting all the additional conducting rods above directed; and forming a communication between the leaden covering of the roof, and the bottom of the wells as before mentioned.

As to the other buildings belonging to the magazines we recommend:

1. That a pointed rod, fimilar to the reft, be erected at each end of the proof-houfe, and be united with the lead already there: alfo, that the lead on the roofs of the two low buildings, deftined for the reception of the empty powder cafks, &c. be connected with the wells by means

of one ſtrip of lead in the middle of each building, of the ſame breadth as thoſe above mentioned.

2. That a pointed rod of copper, about three quarters of an inch in diameter, be erected on each of the four chimnies of the Board-houſe, reaching five feet above them; and be connected, by ſtrips of lead, with the other lead upon the roof of the building.

We do alſo adviſe in general: 1. That the lead and rods, upon the ſeveral buildings, be reſpectively connected with the neareſt wells, by the ſhorteſt metallic communication that can conveniently be formed; in particular, that the two leaden ſpouts of the Board-houſe, which do not reach the ground, but terminate in ciſterns, be connected, by ſtrips of lead, with thoſe ſpouts that do already communicate with water. 2. That the different pieces of which the iron rods may be compoſed, be ſtrongly ſcrewed together in cloſe joints, having a thin plate of lead between them, as directed by the firſt committee. 3. That theſe rods be firmly fixed, and cloſely connected with the lead upon the roofs. 4. That all the ſtrips and pieces of lead be well faſtened and ſoldered together, ſo as to make a perfect metallic communication with the bottom of the wells. 5. That the iron rods be painted, except in thoſe places where they are to be in contact with the lead; that they be all ten feet high;

 and

and that they be terminated with pieces of copper eighteen inches long, like thofe already erected: and, 6. that thefe copper terminations be very finely tapered, and as acutely pointed as poffible.

We give thefe directions, being perfuaded, that elevated rods are preferable to low conductors terminated in rounded ends, knobs, or balls of metal; and conceiving, that the experiments and reafons, made and alledged to the contrary by Mr. WILSON, are inconclufive.

March 12, 1778.

J. PRINGLE, P. R. S.
W. WATSON.
H. CAVENDISH.
W. HENLY.
S. HORSLEY.
T. LANE.
MAHON.
EDW[D]. NAIRNE.
JOSEPH PRIESTLEY.

XVI. *On the Arithmetic of Impoſſible Quantities. By the Rev.* John Playfair, *A. M. Communicated by the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read Feb. 26, 1778.

THE paradoxes which have been introduced into algebra, and remain unknown in geometry, point out a very remarkable difference in the nature of thoſe ſciences. The propoſitions of geometry have never given riſe to controverſy, nor needed the ſupport of metaphyſical diſcuſſion. In algebra, on the other hand, the doctrine of negative quantities and its conſequences have often perplexed the analyſt, and involved him in the moſt intricate diſputations. The cauſe of this diverſity, in ſciences which have the ſame object, muſt no doubt be ſought for in the different modes which they employ to expreſs our ideas. In geometry every magnitude is repreſented by one of the ſame kind; lines are repreſented by a line, and angles by an angle. The genus is always ſignified by the individual, and a general idea by one of the particulars which fall under it. By this means all contradiction is avoided, and

the geometer is never permitted to reaſon about the relations of things which do not exiſt, or cannot be exhibited. In algebra again every magnitude being denoted by an artificial ſymbol, to which it has no reſemblance, is liable, on ſome occaſions, to be neglected, while the ſymbol may become the ſole object of attention. It is not perhaps obſerved where the cennection between them ceaſes to exiſt, and the analyſt continues to reaſon about the characters after nothing is left which they can poſſibly expreſs: if then, in the end, the concluſions which hold only of the characters be transferred to the quantities themſelves, obſcurity and paradox muſt of neceſſity enſue. The truth of theſe obſervations will be rendered evident by conſidering the nature of imaginary expreſſions, and the different uſes to which they have been applied.

2. Thoſe expreſſions, as is well known, owe their origin to a contradiction taking place in that combination of ideas which they were intended to denote. Thus, if it be required to divide the given line AB (fig. 1.) $= a$ in C, ſo that $AC \times CB$ may be equal to a given ſpace b^2, and if $AC = x$, then $x = \frac{1}{2}a \pm \sqrt{\frac{1}{4}a^2 - b^2}$; which value of x is imaginary when b^2 is greater than $\frac{1}{4}a^2$; now to ſuppoſe that b^2 is greater than $\frac{1}{4}a^2$, is to ſuppoſe that the rectangle $AC \times CB$ is greater than the ſquare of half the line AB,

which

which is impoſſible. The ſame holds wherever expreſſions of this kind occur. Thus, when it is aſſerted that unity has the three cube roots 1, $\frac{-1+\sqrt{-3}}{2}$, $\frac{-1-\sqrt{-3}}{2}$, no more is meant than that when the general equation $x^3-ax^2+bx-r=0$ is, by a change in the data, reduced to the particular ſtate $x^3-1=0$, x is then equal to unity only, and admits not of any other value, as it does in more general forms of the equation. The natural office of imaginary expreſſions is, therefore, to point out when the conditions, from which a general formula is derived, become inconſiſtent with each other; and they correſpond in the algebraic calculus to that part of the geometrical analyſis, which is uſually ſtyled the determination of problems.

3. This, however, is not the only uſe to which imaginary expreſſions have been applied. When combined according to certain rules, they have been put to denote real quantities, and though they are in fact no more than marks of impoſſibility, they have been made the ſubjects of arithmetical operations; their ratios, their products, and their ſums, have been computed, and, what may ſeem ſtrange, juſt concluſions have in that way been deduced. Nevertheleſs, the name of reaſoning cannot be given to a proceſs into which no idea is introduced.

Accordingly

Accordingly geometry, which has its modes of reaſoning that correſpond to every other part of the algebraic calculus, has nothing ſimilar to the method we are now conſidering; for the arithmetic of mere characters can have no place in a ſcience which is immediately converſant with ideas.

But though geometry rejects this method of inveſtigation, it admits, on many occaſions, the concluſions derived from it, and has confirmed them by the moſt rigorous demonſtration. Here then is a paradox which remains to be explained. If the operations of this imaginary arithmetic are unintelligible, why are they not alſo uſeleſs? Is inveſtigation an art ſo mechanical, that it may be conducted by certain manual operations? or is truth ſo eaſily diſcovered, that intelligence is not neceſſary to give ſucceſs to our reſearches? Theſe are difficulties which it is of ſome importance to reſolve, and on which much attention has not hitherto been beſtowed. Two celebrated mathematicians, BERNOULLI and MACLAURIN, have indeed touched on this ſubject; but being more intent on applying their calculus, than on explaining the grounds of it, they have only ſuggeſted a ſolution of the difficulty, and one too by no means ſatisfactory. They alledge[(a)], that when imaginary expreſſions are put to

(*a*) Op. J. BERN. tom. I. N° 70. MACLAUR. Flux. art. 699—763.

denote

denote real quantities, the imaginary characters involved in the different terms of ſuch expreſſions do then compenſate or deſtroy each other. But beſide that, the manner in which this compenſation is made, in expreſſions ever ſo little complicated, is extremely obſcure, if it be conſidered that an imaginary character is no more than a mark of impoſſibility, ſuch a compenſation becomes altogether unintelligible: for how can we conceive one impoſſibility removing or deſtroying another? Is not this to bring impoſſibility under the predicament of quantity, and to make it a ſubject of arithmetical computation? And are we not thus brought back to the very difficulty to be removed? Their explanation cannot of conſequence be admitted; but, on attempting another, it behoves us to obſerve, that a more extenſive application of this method, than had been made in their time, has now greatly facilitated the inquiry. We begin then with conſidering the manner in which the imaginary expreſſions, ſuppoſed to denote real quantities, are derived; and the caſes in which they prove uſeful for the purpoſes of inveſtigation.

4. Let a be an arch of a circle of which the radius is unity, and let c be the number which has unity for its hyperbolic logarithm, then the ſine of the arch a, or

ſin.

ſin. $a = \frac{c^{a\sqrt{-1}} - c^{-a\sqrt{-1}}}{2\sqrt{-1}}$; and coſ. $a = \frac{c^{a\sqrt{-1}} + c^{-a\sqrt{-1}}}{2}$. Theſe exponential and imaginary values of the ſine and coſine are already well known to geometers; and the inveſtigation of them, according to the received arithmetic of impoſſible quantities, may be as follows.

Let ſin. $a = z$, then $\dot{a} = \frac{\dot{z}}{\sqrt{1-z^2}}$. To bring this fluxion under ſuch a form that its fluent may be found by logarithms, both numerator and denominator are to be multiplied by $\sqrt{-1}$; then $\dot{a} = \sqrt{-1} \times \frac{\dot{z}}{\sqrt{z^2-1}}$, and (by form. 6. HARM. Men.) $a = \sqrt{-1} \times \log. \frac{z+\sqrt{z^2-1}}{\sqrt{-1}}$. Hence $\frac{a}{\sqrt{-1}}$, or $1 \times \frac{a}{\sqrt{-1}} = \log. \frac{z+\sqrt{z^2-1}}{\sqrt{-1}}$, and becauſe 1 is the log. of c, $c^{\frac{a}{\sqrt{-1}}} = \frac{z+\sqrt{z^2-1}}{\sqrt{-1}}$; wherefore, if both parts of the fractional index of c be multiplied by $\sqrt{-1}$, $c^{-a\sqrt{-1}} = \frac{z+\sqrt{z^2-1}}{\sqrt{-1}}$. Again, if the arch a be conſidered as negative, its ſine becomes alſo negative, and therefore $-a = \sqrt{-1} \times \log. \frac{-z+\sqrt{z^2-1}}{\sqrt{-1}}$, or, $-a\sqrt{-1} = -\log. \frac{-z+\sqrt{z^2-1}}{\sqrt{-1}}$, and $a\sqrt{-1} = \log. \frac{-z+\sqrt{z^2-1}}{\sqrt{-1}}$; whence alſo, $c^{a\sqrt{-1}} = \frac{-z+\sqrt{z^2-1}}{\sqrt{-1}}$. If from this equation the former be taken away, there remains $-\frac{2z}{\sqrt{-1}} = c^{a\sqrt{-1}} - c^{-a\sqrt{-1}}$, whence dividing by $2\sqrt{-1}$ we have $z = \text{ſin. } a = \frac{c^{a\sqrt{-1}} - c^{-a\sqrt{-1}}}{2\sqrt{-1}}$. By adding together the

 equations

equations a value of the cofine may be found in the fame imaginary terms which were affigned above. Now by means of thefe expreffions many theorems may be demonftrated; it may, for example, be fhewn, that if a and b are any two arches of a circle, of which the radius is unity, then fin. $a \times$ cof. $b = \frac{\text{fin.}\ \overline{a+b}}{2} + \frac{\text{fin.}\ \overline{a-b}}{2}$. For fin. $a = \frac{c^{a\sqrt{-1}} - c^{-a\sqrt{-1}}}{2\sqrt{-1}}$, and cof. $b = \frac{c^{b\sqrt{-1}} + c^{-b\sqrt{-1}}}{2}$, therefore, fin. $a \times$ cof. $b = \frac{c^{\overline{a+b}\times\sqrt{-1}} - c^{\overline{-a-b}\times\sqrt{-1}} + c^{\overline{a-b}\times\sqrt{-1}} - c^{\overline{b-a}\times\sqrt{-1}}}{4\sqrt{-1}} = \frac{\text{fin.}\ \overline{a+b}}{2} + \frac{\text{fin.}\ \overline{a-b}}{2}$.

5. Now it may be obferved, that the imaginary value which has been found for fin. a was obtained by bringing a fluxion, properly belonging to the circle, under the form of one belonging to the hyperbola. It may, therefore, be worth while to inquire, whether a fimilar expreffion may not be derived from the hyperbola itfelf.

Let BAD be a rectangular hyperbola (fig. 2.) of which the center is C, and the femi-tranverfe axis AC $=1$; let B be any point in the hyperbola, join BC, and let BE be an ordinate to the tranfverfe axis. Then, if the fector ACB $= \frac{1}{2}a$, and BE $= z$, it is known that $\dot{a} = \frac{\dot{z}}{\sqrt{1+z^2}}$; whence $a = \log.\ z + \sqrt{1+z^2}$, and $c^a = z + \sqrt{1+z^2}$. But if the fector be taken on the other fide of the tranfverfe axis, a and z become

become negative, and $c^{-a} = -z + \sqrt{1+z^2}$. Hence $z = \frac{c^a - c^{-a}}{2}$; in like manner the abſciſs belonging to ACB, that is $CE = \frac{c^a + c^{-a}}{2}$. Theſe values of the ordinates and abſciſſæ differ in nothing from thoſe of the ſines and coſines already found, except in being free from impoſſible quantities; for it is evident, that the quantity a is related in the ſame manner both to the circular and hyperbolic ſectors. If now ord. a and abſ. b denote the ordinate and abſciſs belonging to the ſectors $\frac{1}{2}a$, $\frac{1}{2}b$ reſpectively, ord. $a \times$ abſ. $b = \frac{c^a - c^{-a}}{2} \times \frac{c^b + c^{-b}}{2} = \frac{c^{a+b} - c^{-a-b} + c^{a-b} - c^{b-a}}{4} = \frac{\text{ord. } \overline{a+b}}{2} + \frac{\text{ord. } \overline{a-b}}{2}$.

6. The concluſions in both the foregoing caſes are perfectly coincident, and the methods by which they have been obtained are ſimilar; though with this difference between them, that in the firſt all the ſteps are unintelligible, but in the laſt ſignificant. If then, notwithſtanding a difference which might be expected ſo materially to affect their concluſions, they have been equally ſucceſsful in the diſcovery of truth, it can be aſcribed only to the analogy which takes place between the ſubjects of inveſtigation; an analogy ſo cloſe, that every property belonging to the one may, with certain reſtrictions, be transferred to the other. Accordingly,

 every

every imaginary expreſſion, which has been found to belong to the circle in the preceding calculation, is by the ſubſtitution of real for impoſſible quantities, or of $\sqrt{1}$ for $\sqrt{-1}$, converted into a propoſition which holds of the hyperbola. The operations, therefore, performed with the imaginary characters, though deſtitute of meaning themſelves, are yet notes of reference to others which are ſignificant. They point out indirectly a method of demonſtrating a certain property of the hyperbola, and then leave us to conclude from analogy that the ſame property belongs alſo to the circle. All that we are aſſured of by the imaginary inveſtigation is, that its concluſion may, with all the ſtrictneſs of mathematical reaſoning, be proved of the hyperbola; but if from thence we would transfer that concluſion to the circle, it muſt be in conſequence of the principle which has been juſt now mentioned. The inveſtigation, therefore, reſolves itſelf ultimately into an argument from analogy; and, after the ſtricteſt examination, will be found without any other claim to the evidence of demonſtration. Had the foregoing propoſition been proved of the hyperbola only, and afterwards concluded to hold of the circle, merely from the affinity of the curves, its certainty would have been preciſely the ſame as when a proof is made out by the intervention of imaginary ſymbols.

8. Though it might readily be coucluded, that the ſame principle on which the foregoing inveſtigation has been found to proceed, extends itſelf to all thoſe in which imaginary expreſſions are put to denote real quantities, it may yet be proper to make trial of its application in ſome other inſtances.

Let AB, AC, AD, AE (fig. 3.) be any arches of a circle in arithmetical progreſſion, and let m be their number; it is required to find the ſum of the ſines BC, CH, &c. of thoſe arches. Let the radius $AF=1$, $AB=a$, and the common difference of the arches, or $BC=x$: the ſum of the ſeries ſin. $a+$ ſin. $\overline{a+x}$ + ſin. $\overline{a+2x}$ + ... (m) is to be found. Now, becauſe ſin. $a=\frac{c^{a\sqrt{-1}}-c^{-a\sqrt{-1}}}{2\sqrt{-1}}$, and ſin. $a+x=\frac{c^{\overline{a+x}\times\sqrt{-1}}-c^{\overline{-a-x}\times\sqrt{-1}}}{2\sqrt{-1}}$, &c.; the ſeries ſin. $a+$ ſin. $a+x+$ ſin. $a+2x$ . . . . . $(m)=\frac{c^{a\sqrt{-1}}}{2\sqrt{-1}}\times$ $\overline{1+c^{x\sqrt{-1}}+c^{2x\sqrt{-1}}}$ . . . . . $(m)-\frac{c^{a\sqrt{-1}}}{2\sqrt{-1}}\times$ $\overline{1+c^{-x\sqrt{-1}}+c^{-2x\sqrt{-1}}}$ . . . . . (m). But theſe ſeries are both geometrical progreſſions, and the ſum of the firſt is $\frac{c^{a\sqrt{-1}}}{2\sqrt{-1}}\times\frac{1-c^{mx\sqrt{-1}}}{1-c^{x\sqrt{-1}}}$; and of the ſecond, $\frac{c^{-a\sqrt{-1}}}{2\sqrt{-1}}\times\frac{1-c^{-mx\sqrt{-1}}}{1-c^{-x\sqrt{-1}}}$. The ſum of the propoſed ſeries therefore

$$=\frac{c^{a\sqrt{-1}}}{2\sqrt{-1}}\times\frac{1-c^{mx\sqrt{-1}}}{1-c^{x\sqrt{-1}}}-\frac{c^{-a\sqrt{-1}}}{2\sqrt{-1}}\times\frac{1-c^{-mx\sqrt{-1}}}{1-c^{-x\sqrt{-1}}}=\frac{1}{2\sqrt{-1}}\times$$
$$\frac{c^{a\sqrt{-1}}-c^{\overline{a+mx}\times\sqrt{-1}}-c^{\overline{a-x}\times\sqrt{-1}}+c^{\overline{a+mx-x}\times\sqrt{-1}}}{1-c^{x\sqrt{-1}}-c^{-x\sqrt{-1}}+1}+\frac{1}{2\sqrt{-1}}\times$$
$$-c^{-a\sqrt{-1}}$$

$$\frac{-c^{-a\sqrt{-1}}+c^{-\overline{a-mx}\times\sqrt{-1}}+c^{-\overline{a+x}\times\sqrt{-1}}-c^{-\overline{a-mx+x}\times\sqrt{-1}}}{1-c^{x\sqrt{-1}}-c^{-x\sqrt{-1}}+1};$$

in which expreſſion, if the ſines be ſubſtituted for their imaginary values, we have

$$\frac{\text{ſin.}\, a - \text{ſin.}\, \overline{a+mx} - \text{ſin.}\, \overline{a-x} + \text{ſin.}\, \overline{a+mx-x}}{2\times\overline{1-\text{coſ.}\, x}} =$$

$\text{ſin.}\, a + \text{ſin.}\, \overline{a+x} + \text{ſin.}\, \overline{a+2x} \ldots\ldots (m)$. Q. E. I.

When AB = BC, or $a = x$, the propoſed ſeries becomes $\text{ſin.}\, x + \text{ſin.}\, 2x + \text{ſin.}\, 3x \ldots\ldots (m)$, and its value $= \frac{\text{ſin.}\, x - \text{ſin.}\, \overline{m+1}\times x + \text{ſin.}\, mx}{2\times\overline{1-\text{coſ.}\, x}}$.

In like manner it will be found, that the ſum of the coſines of the ſame arches, or $\text{coſ.}\, a + \text{coſ.}\, \overline{a+x} + \text{coſ.}\, \overline{a+2x} + \ldots\ldots (m) = \frac{\text{coſ.}\, a - \text{coſ.}\, \overline{a+mx} - \text{coſ.}\, \overline{a-x} + \text{coſ.}\, \overline{a+mx-x}}{2\times\overline{1-\text{coſ.}\, x}}$; and when $a = x$, $\text{coſ.}\, x + \text{coſ.}\, 2x + \text{coſ.}\, 3x \ldots\ldots (m) = \frac{\text{coſ.}\, mx - \text{coſ.}\, \overline{m+1}\times x}{2\times\overline{1-\text{coſ.}\, x}} - \frac{1}{2}$.

9. To ſolve the ſame problem, in the caſe of the hyperbola, we muſt follow the ſteps which have been traced out by theſe imaginary operations. Let ABE be an equilateral hyperbola (fig. 4.) of which the center is F, and the tranſverſe axis AF = 1; let ABF, ACF, ADF, &c. be any ſectors in arithmetical progreſſion, and let m be their number; it is required to find the ſum of all the ordinates BG, CH, DK, &c. belonging thoſe ſectors. Let the ſector AFB $= \frac{1}{2}a$, and the ſector BFC, which is the

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common difference of the ſectors, $=\frac{1}{2}x$: then BG, or ord. $a = \frac{c^a - c^{-a}}{2}$, and CH, or ord. $\overline{a+x} = \frac{c^{a+x} - c^{-a-x}}{2}$, by art. 5. Therefore the ſeries of ordinates, that is, $\text{BG}+\text{CH}+\text{DK}+\ldots\ldots(m) = \frac{c^a}{2} \times \overline{1 + c^x + c^{2x} + \ldots\ldots(m)} - \frac{c^{-a}}{2} \times \overline{1 + c^{-x} + c^{-2x} + \ldots\ldots(m)} = \frac{c^a}{2} \times \frac{1 - c^{mx}}{1 - c^x} - \frac{c^{-a}}{2} \times \frac{1 - c^{-mx}}{1 - c^{-x}}$

$$= \frac{1}{2} \times \frac{c^a - c^{a+mx} - c^{a-x} + c^{a+mx-x} - c^{-a} + c^{-a-mx} + c^{-a-x} - c^{-a-mx+x}}{1 - c^x - c^{-x} + 1} =$$

$$\frac{\text{ord. } a - \text{ord. } \overline{a+mx} - \text{ord. } \overline{a-x} + \text{ord. } \overline{a+mx-x}}{2 \times \overline{1 - \text{abſ. } x}}.$$

When $a = x$, ord. x + ord. $2x$ + ord. $3x + \ldots\ldots(m) =$

$$\frac{\text{ord. } x - \text{ord. } \overline{m+1} \times x + \text{ord. } mx}{2 \times \overline{1 - \text{abſ. } x}}.$$

In like manner it is proved, that the ſum of the abſciſſæ, that is, $\text{FG}+\text{FH}+\text{FK}+\ldots\ldots(m) =$

$\frac{\text{abſ. } a - \text{abſ. } \overline{a+mx} - \text{abſ. } \overline{a-x} + \text{abſ. } \overline{a+mx-x}}{2 \times \overline{1 - \text{abſ. } x}}$; and when $a = x$, this expreſſion becomes $\frac{\text{abſ. } mx - \text{abſ. } \overline{m+1} \times x}{2 \times \overline{1 - \text{abſ. } x}} - \frac{1}{2}$.

10. The coincidence of the theorems deduced in the two laſt articles is obvious at firſt ſight, and if the methods by which they have been obtained be compared, it will appear, that the imaginary operations in the one caſe were of no uſe but as they adumbrated the real demonſtration, which took place in the other. This will be rendered more evident by conſidering that the reſolution of the ſeries of hyperbolic ordinates, into two others

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of continual proportionals, can be exhibited geometrically. For, from the points A, B, C, and D, let AM, BN, CO, DP, be drawn at right angles to the affymptote FP; let GB produced meet FP in Q, and let BR be perpendicular to the conjugate axis FR. Then, becaufe the triangles FRS, FMA, are equiangular, AF : FM :: FS : FR; hence $FR = \frac{FM}{FA} \times FS = \frac{FM}{FA} \times \overline{FN - NB}$. For the fame reafon $CH = \frac{FM}{FA} \times \overline{FO - OC}$, and $DK = \frac{FM}{FA} \times \overline{FP - PD}$. Therefore, $BG + CH + DK = \frac{FM}{FA} \times \overline{FN + FO + FP} - \frac{FM}{FA} \times \overline{BN + CO + DP}$; now, FN, FO, FP, are continual proportionals, and fo alfo are BN, FO, FP, becaufe the fectors FBC, FCD, are equal. But in the circle no fuch refolution of the propofed feries of fines can take place, that feries being fubject to alternate increafe and diminution; on which account it is, that imaginary characters enter into the exponential value of the fine. Thofe characters are therefore fo far from compenfating each other in the prefent cafe, as they ought to do, on the fuppofition of BERNOULLI and MACLAURIN, that they manifeftly ferve as marks of impoffibility. There remains, of confequence, the affinity between circular arches and hyperbolic areas, or between the meafures of angles and of ratios, as the only principle on which the imaginary inveftigation can proceed. It need fcarcely be obferved,

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that the exponential value of the hyperbolic ordinate may be deduced from what has been proved in this article.

11. But as the arithmetic of impossible quantities is no where of greater use than in the investigation of fluents, it is of consequence to inquire, whether the preceding theory extends also to that application of it.

Let it then be required to find the fluent of the equation $\frac{\ddot{y}}{\dot{z}^2} \mp a^2 y = Q$, where Q denotes any function whatever of x. For this purpose, the following lemma is premised: let x be any arch, and p any flowing quantity; then, if the sign $\int$, be taken to denote the fluent of the quantity to which it is prefixed, sin. $x \int \dot{p}$ cos. $x -$ cos. $x \int \dot{p}$ sin. $x =$ $\frac{c^{x\sqrt{-1}}}{2\sqrt{-1}} \int \dot{p} c^{-x\sqrt{-1}} - \frac{c^{-x\sqrt{-1}}}{2\sqrt{-1}} \int \dot{p} c^{x\sqrt{-1}}$; or if $\frac{1}{2}x$ be a hyperbolic sector, ord. $x \int \dot{p}$ abs. $x -$ abs. $x \int \dot{p}$ ord. $x =$ $\frac{c^x}{2} \int \dot{p} c^{-x} - \frac{c^{-x}}{2} \int \dot{p} c^x$.

Because sin. $x \int \dot{p}$ cos. $x = \frac{c^{x\sqrt{-1}} - c^{-x\sqrt{-1}}}{2\sqrt{-1}} \int \dot{p} \times \frac{c^{x\sqrt{-1}} + c^{-x\sqrt{-1}}}{2}$, by separating the terms we have sin. $x \int \dot{p}$ cos. $x =$ $\frac{c^{x\sqrt{-1}}}{4\sqrt{-1}} \int \dot{p} c^{x\sqrt{-1}} + \frac{c^{x\sqrt{-1}}}{4\sqrt{-1}} \int \dot{p} c^{-x\sqrt{-1}} - \frac{c^{-x\sqrt{-1}}}{4\sqrt{-1}} \int \dot{p} c^{x\sqrt{-1}} -$ $\frac{c^{-x\sqrt{-1}}}{4\sqrt{-1}} \int \dot{p} c^{-x\sqrt{-1}}$, for the same reason $-$ cos. $x \int \dot{p}$ sin. $x =$ $-\frac{c^{x\sqrt{-1}}}{4\sqrt{-1}} \int \dot{p} c^{x\sqrt{-1}} + \frac{c^{x\sqrt{-1}}}{4\sqrt{-1}} \int \dot{p} c^{-x\sqrt{-1}} - \frac{c^{-x\sqrt{-1}}}{4\sqrt{-1}} \int \dot{p} c^{x\sqrt{-1}} +$

 $\frac{c^{-x\sqrt{-1}}}{4\sqrt{-1}}$

$\frac{c^{-x\sqrt{-1}}}{4\sqrt{-1}}\int \dot{p}c^{-x\sqrt{-1}}$. Wherefore, by collecting the sum of all the terms, we have sin. $x\int \dot{p}$ cos. x − cos. $x\int \dot{p}$ sin. x = $\frac{c^{x\sqrt{-1}}}{2\sqrt{-1}}\int \dot{p}c^{-x\sqrt{-1}} - \frac{c^{-x\sqrt{-1}}}{2\sqrt{-1}}\int \dot{p}c^{x\sqrt{-1}}$.

The demonstration in the case of the hyperbola is free from imaginary expressions; but, in other respects, is exactly similar to that which has now been given in the case of the circle.

12. Let the co-efficient of y in the proposed equation be first supposed negative, that is, let $\frac{\ddot{y}}{\dot{x}^2} - a^2y = Q$, and if we multiply by $c^{nx}\dot{x}$, n being a constant but indeterminate quantity, it becomes $\frac{c^{nx}\ddot{y}}{\dot{x}} - a^2c^{nx}y\dot{x} = c^{nx}Q\dot{x}$. Let $c^{nx} \times \overline{\frac{A\dot{y}}{\dot{x}} - By}$ be assumed for the fluent, A and B being indeterminate, and let its fluxion be taken, then,

$$\frac{Ac^{nx}\ddot{y}}{\dot{x}} + nAc^{nx}\dot{y} - nBc^{nx}y\dot{x} = c^{nx}Q\dot{x}.$$
$$-Bc^{nx}\dot{y}.$$

Hence, by comparing the terms, we get $A = 1$, $nA - B = 0$, $nB = a^2$; therefore, $n = \pm a$, and $B = \pm a$: for n and B let the value $+a$ be substituted, and for A, its value, unity; and the assumed equation becomes $\overline{\frac{\dot{y}}{\dot{x}} - ay} \times c^{ax} = \int c^{ax}Q\dot{x}$, or $\frac{\dot{y}}{\dot{x}} - ay = c^{-ax}\int c^{ax}Q\dot{x}$. Let this equation be multiplied by $c^{mx}\dot{x}$, m being indeterminate as before, and

$c^{mx}\dot{y}$

$c^{mx}\dot{y} - a c^{mx} y \dot{x} = \overline{c^{m-a} \times x} \dot{x} \int c^{ax} Q \dot{x}$. The fluent of the first member of this equation is evidently of the form $D c^{mx} y$, the fluxion of which, *viz.* $D c^{mx}\dot{y} + D m c^{mx} y \dot{x}$ being compared with the former gives $D = 1$, and $m = -a$; wherefore, $c^{-ax} y = \int c^{-2ax} \dot{x} \int c^{ax} Q \dot{x}$, or $y = c^{ax} \times \int c^{-2ax} \dot{x} \int c^{ax} Q \dot{x}$. Let $c^{ax} Q \dot{x} = \dot{z}$, and $c^{-2ax} \dot{x} = \dot{v}$; then $\int c^{-2ax} \dot{x} \int c^{ax} Q \dot{x} = \int z \dot{v} = z v - \int v \dot{z}$; but $v = \frac{1}{2a} - \frac{c^{-2ax}}{2a}$, supposing that v and x vanish at the same time; therefore $v z - \int v \dot{z} = \frac{1}{2a} \int c^{ax} Q \dot{x} - \frac{c^{-2ax}}{2a} \int c^{ax} Q \dot{x} - \frac{1}{2a} \int c^{ax} Q \dot{x} + \frac{1}{2a} \int c^{-ax} Q \dot{x} = \frac{1}{2a} \int c^{-ax} Q \dot{x} - \frac{c^{-2ax}}{2a} \int c^{ax} Q \dot{x}$. Hence $y = \frac{c^{ax}}{2a} \int c^{-ax} Q \dot{x} - \frac{c^{-ax}}{2a} \int c^{ax} Q \dot{x}$. This value of y is sufficient for the construction of the fluent, because the quantities $\int c^{-ax} Q \dot{x}$, and $\int c^{ax} Q \dot{x}$ depend on the quadrature of the hyperbola; but if we would introduce into it the ordinates and abscisses of that curve, we need only have recourse to the foregoing lemma, from which it appears, that $y = \frac{1}{a}$ ord. $ax \int Q \dot{x}$ abs. $ax - \frac{1}{a}$ abs. $ax \int Q \dot{x}$ ord. ax.

13. Let the co-efficient of y be now supposed affirmative, or let $\frac{\ddot{y}}{\dot{x}^2} + a^2 y = Q$. In this case imaginary expressions are introduced into the fluent, and the construction by

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the hyperbola becomes impoffible. For we have then, $n = \pm a\sqrt{-1}$, from which, by proceeding as above, we get $y = \frac{c^{ax-\sqrt{-1}}}{2a\sqrt{-1}}\int c^{-ax\sqrt{-1}} Q\dot{x} - \frac{c^{-ax}}{2a\sqrt{-1}}\int c^{ax\sqrt{-1}} Q\dot{x}$; hence alfo, by the lemma, $y = \text{fin. } ax \int Q\dot{x} \text{ cof. } ax - \text{cof. } ax \int Q\dot{x} \text{ fin. } ax$. Here the quantities, $\int Q\dot{x} \text{ cof. } ax$, and $\int Q\dot{x} \text{ fin. } ax$, are affignable by the quadrature of the circle, in the fame manner as $\int Q\dot{x} \text{ abf. } ax$, and $\int Q\dot{x} \text{ ord. } ax$, by the quadrature of the hyperbola; but the method of inveftigating them, though an illuftration of the principles which we have laid down, is too well known to need to be inferted here. In like manner might the fluents of innumerable fluxionary equations, comprehended under the general form $Q = y + \frac{a\dot{y}}{\dot{x}} + \frac{b\ddot{y}}{\dot{x}^2} + \frac{d\dddot{y}}{\dot{x}^3} + \text{\&c.}$ be deduced, and all of them would tend to prove that the arithmetic of impoffible quantities is no more than a method of tracing the analogy between the meafures of ratios and of angles. M. M. EULER[(b)] and D'ALEMBERT[(c)] were the firft to integrate fuch equations as the preceding, and the method employed here differs from theirs only by being better adapted to illuftrate the principle which is common to them all.

(b) Nov. Com. Petrop. tom. III. (d) Theorie de la Lune.

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14. The forms in the *Harmonia Mensurarum* might alſo be brought to confirm this theory; but, without accumulating inſtances any farther, it may be ſufficient to remark two conſequences that follow from it: 1. That the only caſes in which imaginary expreſſions may be put to denote real quantities, are thoſe in which the meaſures of ratios or of angles are concerned. 2. That the property of either of thoſe meaſures, ſo inveſtigated, might have been inferred from analogy alone. Now both theſe concluſions are agreeable to experience. It does not appear, that any inſtance has yet occurred where imaginary characters ſerve to expreſs real quantities, if circular arches or hyperbolic areas are not the ſubjects of inveſtigation; and if the concluſion obtained may not be transferred from the one to the other, by a mere ſubſtitution of correſponding magnitudes; that is, of ſines for ordinates, coſines for abſciſſes, and circular arches for the doubles of hyperbolic ſectors. The affinity between the circle and hyperbola is not however ſo cloſe, but that it is ſubject to certain limitations, from conſidering which, the truth of what is here aſſerted will be rendered more evident.

1. Any propoſition demonſtrated of hyperbolic ſectors may be transferred to circular arches by ſubſtitution alone, without any change in the ſigns, when only

abſciſſæ and their products enter into the enunciation, and converſely. Thus abſ. $a \times$ abſ. $b = \frac{\text{abſ. } \overline{a+b}}{2} + \frac{\text{abſ. } \overline{a-b}}{2}$; and coſ. $a \times$ coſ. $b = \frac{\text{coſ. } \overline{a+b}}{2} + \frac{\text{coſ. } \overline{a-b}}{2}$. The ſame holds when the ſimple power of the ordinate is combined with any power whatever of the abſciſs: ſo in the theorems of art. 5. and 4. ord. $a \times$ abſ. $b = \frac{\text{ord. } \overline{a+b}}{2} + \frac{\text{ord. } \overline{a-b}}{2}$; and ſin. $a \times$ coſ. $b = \frac{\text{ſin. } \overline{a+b}}{2} + \frac{\text{ſin. } \overline{a-b}}{2}$.

2. When an expreſſion containing any property of hyperbolic ſectors, involves in it the rectangle of two ordinates, the value of that rectangle muſt have a contrary ſign, when a tranſition is made to the circle. Thus ord. $a \times$ ord. $b = \frac{\text{abſ. } \overline{a+b}}{2} - \frac{\text{abſ. } \overline{a-b}}{2}$; but ſin. $a \times$ ſin. $b = -\frac{\text{coſ. } \overline{a+b}}{2} + \frac{\text{coſ. } \overline{a-b}}{2}$. The difference which according to this rule is found between the powers of ordinates and of ſines may be ſeen in the following examples. If $\frac{1}{2}x$ denote any hyperbolic ſector, then, by involving $\frac{c^x - c^{-x}}{2}$, and again ſubſtituting for the exponential quantities as in art. 5. we have,

$$\overline{\text{ord. } x}^2 = \frac{\text{abſ. } 2x - 1}{2};$$

$$\overline{\text{ord. } x}^3 = \frac{\text{ord. } 3x - 3 \text{ ord. } x}{4};$$

$$\overline{\text{ord. } x}^4 = \frac{\text{abſ. } 4x - 4 \text{ abſ. } 2x + 3}{8};$$

2 ord.

$\overline{\text{ord. } x}^5 = \frac{\text{ord. } 5x - 5 \text{ ord. } 3x + 10 \text{ ord. } x}{16}$; and universally, if n be any number; a the co-efficient of the second term of a binomial raised to the power n, b the co-efficient of the third, &c. and p the greatest co-efficient: when n is an even number,

$$\overline{\text{ord. } x}^n = \frac{\text{abs. } nx - a \text{ abs. } \overline{n-2} \times x + b \text{ abs. } \overline{n-4} \times x \ldots\ldots \mp p.}{2^{n-1}} \pm \frac{p}{2^n};$$

but when n is an odd number,

$$\overline{\text{ord. } x}^n = \frac{\text{ord. } nx - a \text{ ord. } \overline{n-2} \times x + b \text{ ord. } \overline{n-4} \times x \ldots\ldots\ldots \mp p \text{ ord. } x}{2^{n-1}}.$$

If now x denote an arch of a circle, by substituting and changing the signs as oft as $\overline{\text{ord. } x}^2$ occurs in any of the preceding expressions, we get

$$\overline{\text{sin. } x}^2 = \frac{1 - \text{cos. } 2x}{2};$$

$$\overline{\text{sin. } x}^3 = \frac{3 \text{ sin. } x - \text{sin. } 3x}{4}:$$

$$\overline{\text{sin. } x}^4 = \frac{3 - 4 \text{ cos. } 2x + \text{cos. } 4x}{8};$$

$\overline{\text{sin. } x}^5 = \frac{10 \text{ sin. } x - 5 \text{ sin. } 3x + \text{sin. } 5x}{16}$: and universally, if n be any number, p the greatest co-efficient of a binomial raised to the power n, A the co-efficient next less than p, B the co-efficient next less than A, and so on: when n is an even number,

$$\overline{\text{sin. } x}^n = \frac{\frac{1}{2}p - \text{A cos. } 2x + \text{B cos. } 4x - \text{\&c.}}{2^{n-1}};$$

but when n is an odd number,

$$\overline{\text{sin. } x}^n = \frac{p \text{ sin. } x - \text{A cos. } 3x + \text{B cos. } 5x - \text{\&c.}}{2^{n-1}}.$$

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These ſeries differ from the former only in the ſigns, and the arrangement of the terms; and when either n, or $n-1$, is diviſible by 4, the ſigns remain the ſame in both.

16. The reaſon of the foregoing rule for changing the ſigns is, that the rectangle under two ordinates to the hyperbola is always expreſſed by the difference of two abſciſſæ: and that if from the abſciſs belonging to a greater ſector, be ſubtracted the abſciſs belonging to a leſs, the remainder will be affirmative; whereas, if from the coſine of a greater arch be ſubtracted the coſine of a leſs, the remainder will be negative. Therefore, that the rectangles, expreſſed by theſe remainders, may have the ſame ſign, in both caſes, the ſigns of the remainders muſt be different.

It appears then, that the ſecond rule, as well as the firſt, is founded on the principle of analogy when taken with the neceſſary limitations, and it is likewiſe evident from the inſtances which have been produced, that thoſe rules lead to the very ſame concluſions which are obtained from the imaginary values of the ſine and coſine.

There are, however, inſtances in which the analogy between the circular and hyperbolic areas being wholly interrupted, neither the foregoing rules, nor any of the ſame kind, can be applied; but this occaſions no ambiguity,

guity for the conftruction required in fuch cafes is by its nature reftricted to one of the curves only. Of this kind is the Cotefian theorem, which requires the whole circle to be divided into a given number of equal parts, and therefore cannot be extended to the hyperbola where a fimilar divifion is impoffible. Others of a like nature may be derived from the general theorems already inveftigated; for the circle, by returning into itfelf, often reduces them to a fimplicity to which there is nothing analogous in the hyperbola. Many examples of this might be adduced, but the two following may fuffice.

1. Let ABCDE (fig. 5.) be a regular polygon infcribed in a circle, and let m be the number of its fides; it is required to find the fum of the lines FA, FB, FC, &c. drawn from any point F in the circumference, to all the angles of the polygon. By the method which in art. 8. was employed to obtain the fum of the fines of a feries of arches in arithmetical progreffion, it will be found, that the fum of the chords of the arches a, $\overline{a+x}$, $\overline{a+2x}$, ... (m), that is, (making FA$=a$, and AB$=x$) the fum of the chords of the arches FA, FB, FC, &c. $=$ $\dfrac{\text{cho.}\,a - \text{cho.}\,\overline{a+mx} - \text{cho.}\,\overline{a-x} + \text{cho.}\,\overline{a+mx-x}}{2\times\overline{1-\text{cof.}\,\frac{1}{2}x}}$; but, in the prefent cafe, mx is equal to the circumference, and therefore $-\text{cho.}\,\overline{a+mx} = +\text{cho.}\,a$ (the chord of an arch greater than

the

the circumference being negative); and, for the ſame reaſon, cho. $\overline{a+mx-x}=-$cho. $\overline{a-x}=+$cho. $\overline{x-a}$. Hence the general expreſſion becomes $\frac{\text{cho. } a+\text{cho. } \overline{x-a}}{1-\text{coſ. } \frac{1}{2}x}=\text{FA}+\text{FB}+\text{FC}+\ldots\ldots(m)$. If therefore GK be drawn from the center, biſecting the chord AB in H, and meeting the circumference in K, the ſum of the chords, that is, $\text{FA}+\text{FB}+\text{FC}+\text{FD}+\text{FE}=\frac{\text{AF}+\text{FB}}{\text{FK}}\times\text{GK}$.

2. Let n be an even number, the reſt remaining as above, and let it be required to find the ſum of the n powers of the chords, that is, the ſum of $\overline{\text{FA}}^n+\overline{\text{FB}}^n+\overline{\text{FC}}^n \ldots\ldots(m)$. By reaſoning, as in the caſe of the ſines, it will appear that, if p be the greateſt co-efficient of a binomial raiſed to the power n; A the co-efficient next leſs than p; B the co-efficient next leſs a. and ſo on, then,

$$\overline{\text{cho. } a}^n=p-2\text{A coſ. } a+2\text{B coſ. } 2a+2\text{D coſ. } 3a+\text{ \&c.}$$

$$\overline{\text{cho. } \overline{a+x}}^n=p-2\text{A coſ.} \overline{a+x}+2\text{B coſ. } 2\times\overline{a+x}+2\text{D coſ. } 3\times\overline{a+x}, \text{ \&c.}$$

$$\overline{\text{cho. } \overline{a+2x}}^n=p-2\text{A coſ. } \overline{a+x}+2\text{B coſ. } 2\times\overline{a+2x}+2\text{D coſ. } 3\times\overline{a+2x}, \text{ \&c.}$$

&c.

Each of theſe vertical columns is to be continued downward, till the number of terms be equal to m, and therefore the ſum of the ſecond is mp. The ſum of the third, or of $-2\text{A}\times\overline{\text{coſ. } a+\text{coſ. } \overline{a+x}+\text{coſ. } \overline{a+2x}} \ldots\ldots(m)$, by art. 8. is $-2\text{A}\times\frac{\text{coſ. } a-\text{coſ. } \overline{a+mx}-\text{coſ. } \overline{a-x}+\text{coſ. } \overline{a+mx-x}}{2\times\overline{1-\text{coſ. } x}}=$ (becauſe $mx=$ the circumference)

$-\text{A}\times$

$-A \times \frac{\text{cos. } a - \text{cos. } a - \text{cos. } \overline{a-x} + \text{cos. } \overline{a-x}}{1 - \text{cos. } x} = 0$. In like manner do the sums of all the subsequent columns vanish; and therefore, cho. $a +$ cho. $\overline{a+x} +$ cho. $\overline{a+2x} \ldots\ldots (m) = mp$. But when n is an even number, $p = \frac{\frac{1}{2}n+1}{\frac{1}{2}n-1} \times \frac{\frac{1}{2}n+2}{\frac{1}{2}n-2} \ldots\ldots \times \frac{n}{\frac{1}{2}n}$ $= \frac{1.3.5.7 \ldots\ldots \overline{n-1}}{1.2.3.4 \ldots\ldots \frac{1}{2}n} \times 2^{\frac{1}{2}n}$. If therefore the radius be put $= r$, and the expression made homogeneous, we have $\overline{FA}^n + \overline{FB}^n + \overline{FC}^n \ldots\ldots (m) = m \times \frac{1.3.5.7 \ldots\ldots \overline{n-1}}{1.2.3.4 \ldots\ldots \frac{1}{2}n} \times 2^{\frac{1}{2}n} r^n$.

Q. E. I.

This last coincides with the forty-first of the curious and difficult propositions published by Dr. STEWART, under the title of general theorems: it is given there without a demonstration, but appears plainly to have been investigated, in a manner altogether rigorous, by that profound geometer. It may therefore be regarded as one of the instances, in which the conclusions of this imaginary arithmetic are verified by the geometrical analysis.

17. The two foregoing propositions being confined to the circle, and yet having been investigated by the help of imaginary expressions, may, at first sight, seem exceptions to the rule, which we have been endeavouring to establish. But it needs only to be remarked, that they are particular cases of certain theorems belonging both

to the circle and hyperbola, and that it was into the investigation of thoſe theorems, that the imaginary expreſſions were introduced.

The concluſions therefore from the whole are theſe: that imaginary expreſſions are never of uſe in inveſtigation but when the ſubject is a property common to the meaſures both of ratios and of angles; that they never lead to any conſequence which might not be drawn from the affinity between thoſe meaſures; and that they are indeed no more than a particular method of tracing that affinity. The deductions into which they enter are thus reduced to an argument from analogy, but the force of them is not diminiſhed on that account. The laws to which this analogy is ſubject; the caſes in which it is perfect, in which it ſuffers certain alterations, and in which it is wholly interrupted, are capable, as may be concluded from the ſpecimens above, of being preciſely aſcertained. Supported on ſo ſure a foundation, the arithmetic of impoſſible quantities will always remain an uſeful inſtrument in the diſcovery of truth, and may be of ſervice when a more rigid analyſis can hardly be applied. For this reaſon, many reſearches concerning it, which in themſelves might be deemed abſurd, are nevertheleſs not deſtitute of utility. M. BERNOULLI has found, for example, that if r be the radius of a circle, the circumference

Fig: 1.

Fig: 2.

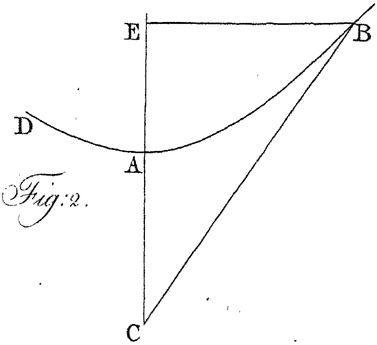


Fig: 3.

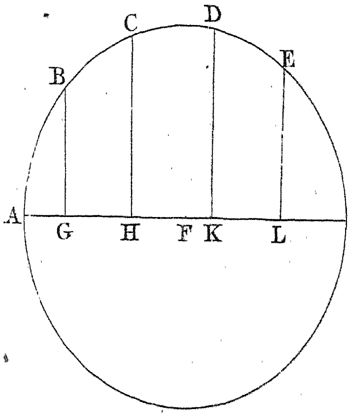


Fig: 4.

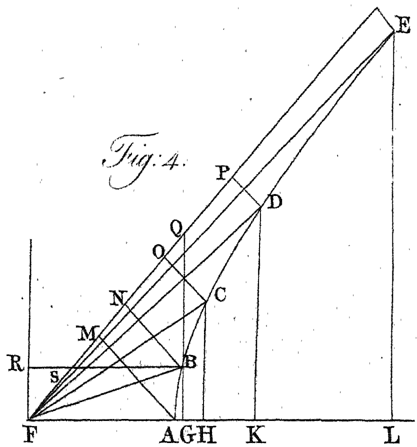
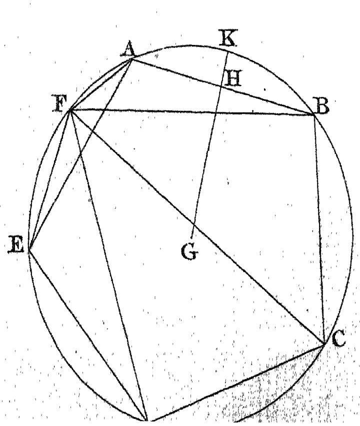


Fig: 5.



$= \frac{4 \log. \sqrt{-1}}{\sqrt{-1}} r$; and the ſame may be deduced from art. 4. Conſidered as a quadrature of the circle, this imaginary theorem is wholly inſignificant, and would deſervedly paſs for an abuſe of calculation; at the ſame time we learn from it, that if in any equation the quantity $\frac{\log. \sqrt{-1}}{\sqrt{-1}}$ ſhould occur, it may be made to diſappear, by the ſubſtitution of a circular arch, and a property, common to both the circle and hyperbola, may be obtained. The ſame is to be obſerved of the rules which have been invented for the transformation and reduction of impoſſible quantities[(e)]: they facilitate the operations of this imaginary arithmetic, and thereby lead to the knowledge of the moſt beautiful and extenſive analogy which the doctrine of quantity has yet exhibited.

(e) The rules chiefly referred to are thoſe for reducing the impoſſible roots of an equation to the form $A + B\sqrt{-1}$.

XVII. *Reflections on the Communication of Motion by Impact and Gravity. By the Rev.* Isaac Milner, *M. A. Fellow of* Queen's College, Cambridge. *Communicated by* Anthony Shepherd, *D. D. F. R. S. and Plumian Professor at* Cambridge.

Read Feb. 26, 1778.

THE theory of moving bodies was little understood by the philosophers who lived in the sixteenth century. They observed, that a body, once put into motion, continued to move for some time after the force was impressed; but they argued very strangely from this ordinary phænomenon. Far from considering the air as a resisting medium; they supposed with ARISTOTLE and the ancients, that it was the perpetual influx of the parts of the atmosphere which continued to urge the body forward and preserve its motion. When a body is projected in any direction inclined to the horizon, the gravity of its parts is always observed to bend the direction of its motion into a curve line; and because this gravity remains invariably the same, whatever the force of projection be, in very swift motions, the figure described may approach very nearly to a right line.

This

This laſt circumſtance induced ſome of the philoſophers we are ſpeaking of to believe, that a cannon ball, for inſtance, always moves in the ſame ſtrait line till its velocity is entirely deſtroyed; and that afterwards it deſcends towards the earth in a direction perpendicular to the horizon. Others thought they mended the matter by ſuſpending the action of gravity for a certain period only; by allowing the latter part of the path to be curvilinear; and laſtly, the body to deſcend to the earth in a ſtraight line, as in the former caſe. We, in theſe days, who have ſeen the gradual improvements in mechanics from time to time, are not ſurprized, that men, in the infancy of that ſcience, ſhould have embraced abſurd and ridiculous principles: we rather wonder, how the author [(a)] of the notion juſt mentioned was able to form any juſt eſtimate of the horizontal ranges of projectiles, and to diſcover their maxima. Whether by conjecture, or probability of induction, we are unable to determine; but ſo it was, TARTALEA affirmed, what has ſince been found true upon unexceptionable evidence, that the amplitudes of projectiles upon the horizon are always greateſt when the angles of projection are equal to 45°. But the praiſe of this diſcovery, as well as whatever elſe relates to the accelerated motions of bodies near the ſurface of the earth, is

(a) NICH. TARTALEA.

is juſtly due to the incomparable GALILEO. The theory of mechanics had received no conſiderable improvement ſince the time of ARCHIMEDES, when this ſurprizing genius appeared in the former part of the ſeventeenth century. He diſcarded the peripatetic philoſophy; explained the whole doctrine of accelerated motion and of projectiles: in a word, he ſo much exhauſted the ſubject, that the beſt treatiſes we have at this day are little more than a repetition of GALILEO's diſcoveries.

This philoſopher, as far as we know, never attempted to inveſtigate the laws by which motion is communicated from one body to another. The celebrated DES CARTES is the firſt we hear of who gave any attention to the ſubject; and the reſult of his enquiries is what might reaſonably be expected from ſo whimſical and romantic a genius; he blundered in this, as in all other caſes, where he was not confined to pure mathematical reaſonings. Our countryman, Dr. WALLIS, made a real progreſs in this ſcience, by diſcovering that fundamental law in the communication of motion, *viz.* that action is equal to re-action, and always in contrary directions: WREN, HUYGENS, confirmed the ſame thing; and the whole theory of the colliſion of bodies, and their mutual actions upon one another, ſeemed to be advancing faſt towards perfection.

But

But a new opinion was now ftarted by M. LEIBNITZ concerning the forces of bodies in motion. The force of a body in motion and its momentum had hitherto been confidered as fynonymous terms, and had alike been meafured by the quantity of matter and velocity conjointly. On the contrary, LEIBNITZ and his followers affirmed, that the force was proportional to the quantity of matter in the moving body and the fquare of its velocity. It is needlefs to relate all that paffed on both fides: fo material an oppofition in fentiment neceffarily produced very warm contention; and, as it generally happens in other difputes, we do not hear of any conviction being produced on either fide.

After furveying the arguments of the difputants, it is not eafy to fay, whether the agitation of the queftion before us has contributed to retard or advance the progrefs of truth and fcience. On the one hand, many ingenious experiments have been made, many curious problems invented and refolved, which probably would never once have been thought of by men who were in the purfuit of truth in a more cool and deliberate way: and, on the other hand, it may juftly be affirmed, that the violence of prejudice and party-fpirit has fo much clouded the reafonings of the beft writers, that we fenfibly feel their influence to this day. I need not diffemble: it

it is a ſerious perſuaſion, that the laws by which motion is communicated are ſtill very materially miſtaken by ſenſible perſons, that induced me to throw together the following hints, and to lay them before the Royal Society. The right underſtanding of theſe laws is of the laſt importance in practice: the good or bad ſucceſs of ſome very expenſive projects has depended upon it; and certain excellent artiſts have been diſappointed in the execution of their plans, and unable to reconcile the apparent contradiction between theory and experiment. From the length of time, which has elapſed ſince LEIBNITZ firſt advanced his new opinions, and the abilities of the philoſophers who engaged in the conteſt, one might have expected, that the whole matter would long before this have been cleared up in a ſatisfactory manner; eſpecially when we conſider, that the communication of motion from one body to another is what every moment happens before our eyes, and that particular experiments are made in this doctrine with the greateſt ſimplicity and convenience. This part of rational mechanics however is not yet generally underſtood, as we may fairly preſume from the difference of opinion which ſtill ſubſiſts among the learned. I freely own, it appears to me, that no new experiments are wanting; no new geometrical reaſonings or conſtructions: the improved parts of geo-

metry

metry have been already applied to the theory of motion in numberlefs cafes, and a variety of well attefted experiments have been clearly explained to us by authors. The laws of motion, in certain cafes, are inconteftable, and no author of eminence contradicts them: it is from a miftaken application of thefe laws that a difference of opinion has arifen. It is obvious, that the laws of motion, as defcribed by Sir ISAAC NEWTON, may, in a certain fenfe, be founded on experiment; and yet, if they are extended to cafes where they cannot be applied, the conclufions muft ftill be erroneous. My defign in thefe pages is to point out diftinctly what is real in this difference of opinion from what is merely verbal, and to explain the caufes of it. This, which perhaps will appear to have never been done with fufficient precifion, feems to be the moft effectual way of preventing miftakes. Geometry and algebra will lead us wrong, if our principles are ill founded: experiment itfelf, if we are not extremely careful, will deceive us in forming a general deduction, or what is called a law of nature. The controverfial writings of the moft able authors will embarrafs and perplex our judgements; but when we have once difcovered the grounds of their mutual miftakes and mifapprehenfions, there is reafon to think, that we

ſhall both underſtand the ſubject better than we did before, and be more on our guard for the future.

The firſt law of motion, as expreſſed by Sir ISAAC NEWTON, is unexceptionable: nobody denies that a body perſeveres in a ſtate of reſt or uniform motion in a right line, till affected by ſome external influence. It is the third law of motion which has produced all this confuſion and perplexity. "Actioni contrariam ſemper et "æqualem eſſe reactionem: ſive corporum duorum ac"tiones in ſe mutuo ſemper æquales et in partes con"trarias dirigi." Theſe words of Sir ISAAC NEWTON convey to us as clear an idea as can poſſibly be conceived with ſo much conciſeneſs. It muſt however be confeſſed, that his illuſtration is not ſo very perſpicuous[(b)]. To ſay, that when a man preſſes a ſtone with his finger, his finger is equally preſſed; and when a horſe draws a ſtone by a cord, the horſe is drawn equally backwards towards the ſtone; is a moſt indiſtinct and popular way of ſpeaking, and can never make evident what was before not underſtood.

(b) Quicquid premit vel trahit alterum, tantundem ab eo premitur vel trahitur. Si quis lapidem digito premit, premitur et hujus digitus a lapide. Si equus lapidem funi alligatum trahit, retrahitur etiam et equus (ut ita dicam) æqualiter in lapidem: nam funis utrinque diſtentus eodem relaxandi ſe conatu urgebit, equum verſus lapidem, ac lapidem verſus equum; tantumque impediet progreſſum unius quantum promovet progreſſum alterius, &c. NEWTON Princip.

Some

Some useful writers, who have copied after Sir ISAAC NEWTON, have talked in the same way; and only increased the ambiguity by being more diffuse. Mr. MACLAURIN himself, who engaged very warmly in this debate with the foreign mathematicians, and who, to say the truth, seems to have understood the nature of the controversy better than any one else, is frequently unguarded in his expression. In chap. II. book 2. of his account of NEWTON's discoveries, he is describing the laws of motion for the first time, and one naturally expects a more than ordinary precision and exactness. There he blames, very justly, the opposers of the Newtonian definition of motion for mistaking the direction in which the motion, lost or communicated, ought always to be estimated. But in p. 122[(c)], he thus expresses himself: "When two "bodies meet, each endeavours to persevere in its state, "and resists any change; and because the change, which "is produced in either, may be equally measured by the "action, which it exerts upon the other, or by the re-"sistance, which it meets with from it, *it follows*, that "the changes produced in the motions of each are "equal; but are made in contrary directions." I cannot possibly conceive, that so skilful and accurate a philosopher could believe, that the third law of motion was an

(c) Octavo edition.

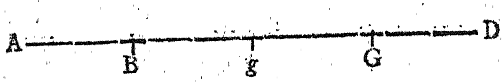
inference

inference of reaſon, excluſive of all experiment; and yet, if words have any meaning at all, the above quotation inclines us to think ſo. It is true, the change which is produced in either body may be meaſured by the action which it exerts upon the other, or by the reſiſtance which it meets with from the ſame: but what are we to underſtand by action or reſiſtance, until they are explained by more intelligible terms? or, when they are explained by terms which do not neceſſarily imply the ſame thing, how do we know that their meaſures are equal, or that they are made in contrary directions, until theſe truths be eſtabliſhed by experiments? A law of nature is not merely a deduction of reaſon: it muſt be proved, either at once and directly, by ſome ſimple and deciſive experiments; or if that cannot be done, by ſuch experiments as enable us to collect its exiſtence by the aſſiſtance of geometry. However obvious theſe reflections may appear, I thought it neceſſary to take notice of MACLAURIN's aſſertion; becauſe in conſequence of that and ſimilar expreſſions, young philoſophers are extremely puzzled in the beginning of their ſtudies, and becauſe I have known ſome, who are more experienced, affirm, that the third law of motion is nothing more than a definition. I now proceed to the conſideration of particular caſes.

CASE THE FIRST. Suppose A and B to represent the magnitudes of two spherical bodies, and *a* and *b* their respective velocities in the same direction; suppose *a* to be greater than *b*, and A will overtake B; and if the bodies are non-elastic, they will proceed together in the same direction as one mass: if they are perfectly elastic, whatever effect has already been produced by the collision, will be repeated; and, because in the first case there is no relative velocity after the stroke, in the second the relative velocity before and after the stroke will be the same, and in contrary directions; and in either case, the motion lost by the striking body is found to be always equal to the motion communicated to B, and in a contrary direction. In this sense action is equal to re-action; and every experiment which has yet been produced, where a clear judgment could be formed of the effect, has confirmed the same thing. All the experiments which are usually brought to determine the impressions made upon soft bodies, as snow, clay, &c. are absolutely unfit for the purpose. The circumstances, which take place in the production of these effects, are such as we can never discover. The directions in which the particles recede, the velocities they acquire, their mutual actions upon one another, and lastly, the time, in which these effects are performed, are all beyond the reach of compu-

computation. The other principle, that the relative velocity of A and B is not altered by the ſtroke, is neither to be demonſtrated nor confirmed by experience; it is a direct conſequence of the definition of elaſticity. Again, ſuppoſe α and β to repreſent the reſpective velocities of A and B after the ſtroke, and from theſe *data* it is eaſily inferred, that $A\alpha^2+B\beta^2=Aa^2+Bb^2$: for $a-b$ is equal to $\beta-\alpha$, becauſe $a-b$ is the relative velocity before, and $\beta-\alpha$ the relative velocity after, the ſtroke. And $Aa+Bb$ is equal to $A\alpha+B\beta$, becauſe theſe quantities repreſent the ſum of the motions before and after the ſtroke reſpectively; and from theſe equations the above equation is deduced, ſhewing, that in elaſtic bodies the ſum of the two bodies multiplied by the ſquares of their abſolute velocities, is not altered by the ſtroke.

The ſame theorem may be demonſtrated



geometrically in the following manner. Let the velocities of A and B be repreſented by AD, AB, reſpectively; and let G be their center of gravity, when placed at B and D; the velocity of A after the ſtroke will be repreſented by Bg, if Gg be taken equal to GD, and the velocity of B by AB + 2BG. From the nature of the center of gravity A × GD = B × BG, and A × GD × 4AG = B × BG × 4AG =

B ×

$B \times \overline{4BG^2 + 4BG \times AB}$. Add to both ſides $A \times Ag^2 + B \times AB^2$, and we ſhall have $A \times AD^2 + B \times AB^2 = A \times Ag^2 + B \times \overline{AB + 2BG}^2$.

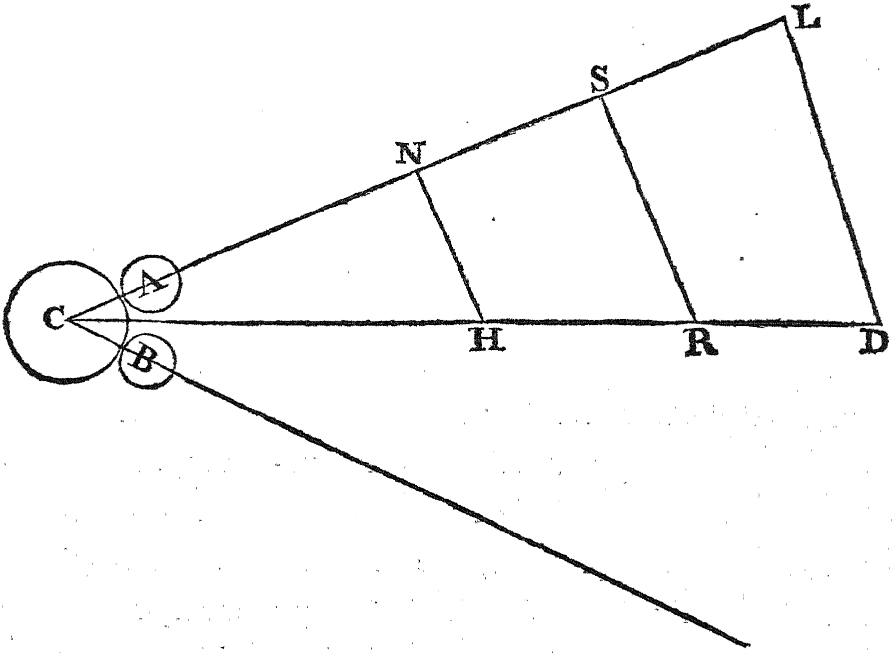
We are not to wonder, therefore, upon making trials with perfectly elaſtic bodies, if any ſuch exiſted, were we always to find their *vires vivæ*, as the foreigners expreſs themſelves, neither increaſed nor diminiſhed by the ſtroke. They define the force of bodies in motion, or their *vis viva*, to be in a compound ratio of their quantities of matter, and the ſquares of their velocities; and certainly ſuch a definition implies no contradiction or impoſſibility. The term force, in a looſe and ordinary way of ſpeaking, conveys to us no determinate idea at all, and therefore, until it be defined, is incapable of being uſed to any good purpoſe in philoſophy: whether this or that definition come nearer to the general ſenſe in which it is uſed indiſtinctly enough in common language, is entirely another queſtion. We may go farther, and add, that in their uſe of the words, becauſe the ſum of the forces of elaſtic bodies is never affected by the ſtroke, it is not unnatural to ſay, that action is therefore equal to re-action, and that no force is loſt by one body but what is communicated to the other. But if we will go ſo far, and thereby change the meaning of the terms action and re-action and their meaſures, we ought at leaſt to guard our readers from miſtaking us, however

convenient ſuch modes of expreſſion may appear. Becauſe $A\alpha^2 + B\beta^2$ is equal to $Aa^2 + Bb^2$, it is true that no force is loſt by A but what is communicated to B; but not in the ſame ſenſe in which it was affirmed that no motion is loſt by A but what is communicated to B. In that caſe the ſquares of their abſolute velocities are underſtood; in this, their velocities reduced to the ſame direction. However, no material ill conſequence can poſſibly ariſe from ſuch a notion of action and re-action, as long as the queſtion is ſuppoſed to concern only elaſtic bodies: but real miſchief is done, and the debate ceaſes to be verbal, whenever the law of the equality of action and re-action is ſaid to take place in the colliſions of all ſorts of bodies.

CASE THE SECOND. But the truth of theſe remarks, and the neceſſity of attending to the preciſe uſe of terms, will appear in a ſtill ſtronger light, if we conſider the ſolution of a problem given us by J. BERNOULLI[(d)].

Suppoſe that two equal and ſpherical bodies, A and B, ſtruck at once in the direction CD perpendicular to the line joining the centers of A and B with a velocity repreſented by *a*. Let the quantity of matter in C be called *m*, and the quantity of matter in A or B, *n*: let the velo-

(*d*) Diſcours ſur le mouvement.



city of C after the ſtroke be repreſented by x, and that of A or B in the direction AC or CB by y, and ſuppoſe $p : q ::$ rad. : coſin. LCD. Then, becauſe ma the quantity of motion before the ſtroke is equal to $mx + \frac{2qny}{p}$, the quantity of motion after the ſtroke, and ma^2 is equal to $mx^2 + 2ny^2$, becauſe the quantity of force is not altered by the colliſion; he eaſily finds $x = \frac{p^2ma - 2q^2na}{p^2m + 2q^2n}$ and $y = \frac{2pqma}{p^2m + 2q^2n}$.

There is no problem which deſerves to be more conſidered than this by a perſon deſirous of having a clear

 idea

idea of the grounds of that contention which has ſubſiſted ſo many years. We here ſee BERNOULLI taking it for granted, that the quantity of force in elaſtic bodies is no ways affected by their mutual actions, whether direct or oblique; and the moſt ſurprizing circumſtance is, that he ſhould not ſo much as hint at any apparent difficulty in the preſent caſe, after he had been ſo very diffuſe in illuſtrating others which were much more ſimple. No doubt he believed this principle to be a direct conſequence of the equality of action and re-action, and therefore it is plain he could not mean the ſame things by thoſe terms as we do at preſent. He believes no force is gained or loſt by impact; he defines force by quantity of matter and ſquare of the velocity conjointly; and in eſtimating the velocity, he pays no regard to the direction in which the bodies are moved. Let us not cavil at his words: we cannot miſtake his meaning. The queſtion is, how far theſe notions are agreeable to experience; how far they are conſiſtent with ſome other principles which are inconteſtable, and which he himſelf has admitted: for inſtance, he admits it as an undoubted principle, that the quantity of motion in any ſyſtem of bodies is preſerved invariable, when eſtimated in a given direction, in all their colliſions and mutual actions upon one another; and in this he entirely agrees with the followers of Sir

ISAAC

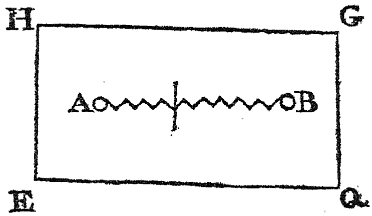
ISAAC NEWTON. Let us attend to the conſequences of theſe two different principles in the very caſe propoſed by J. BERNOULLI. And firſt, becauſe $ma=mx+\frac{2qny}{p}$, by tranſpoſition we have $m\times\overline{a-x}=\frac{2qny}{p}$, which is ſaying no more than that the motion loſt by C is equal to the ſum of the motions gained by A and B, eſtimated in the ſame direction CD. By a ſimilar proceſs from the ſecond equation, we deduce $m\times\overline{a+x}\times\overline{a-x}=2nq^2$; and therefore the compariſon of the two equations gives $\frac{q\times\overline{a+x}}{p}=y$. The quantity y therefore, or the velocity of A or B after the ſtroke, muſt neceſſarily be equal to the ſum of the two quantities $\frac{qa}{p}$ and $\frac{qx}{p}$. In the figure, let CD repreſent the velocity of C before the ſtroke, and CH the velocity after it, and let fall the perpendiculars Hn, DL, upon the direction AC. It eaſily appears, that Cn is equal to $\frac{qx}{p}$ and CL equal to $\frac{qa}{p}$, becauſe CH : CN :: CD : CL :: rad. : coſ. LCD :: $q:p$. And now the whole controverſy is reduced into a narrow compaſs; for whether the two principles aſſumed by this author be conſiſtent with experience or not; it is impoſſible they ſhould be conſiſtent with one another, unleſs Cn+CL ſhall be found to meaſure the velocity of A in the direction CL. Suppoſe Cr to be the velocity of C after impact, when all the bodies are perfectly

perfectly hard, and letting fall the perpendicular rs; cs will be the velocity acquired by A in that case; and, universally, the velocity acquired by A will be equal to $cs+\frac{cs}{m}$, if the elasticity of the bodies be to perfect elasticity as $1:m$. In order to determine, therefore, when $cn+cL$ can possibly be equal to $cs+\frac{cs}{m}$, or, which is the same thing, $Ls+cn$ equal to $\frac{cs}{m}$, we are to consider that $ns:Ls::1:m$: and because cn is equal to $cs-sn$, $cn=cs-\frac{Ls}{m}$, and it is obvious that $cs+Ls-\frac{Ls}{m}$ can never be equal to $\frac{cs}{m}$, unless m be taken equal to unity, and BERNOULLI's hypothesis is plainly impossible in all cases where the bodies are not supposed perfectly elastic.

But though we confess the learned author, who first solved the problem we have been considering, deserves no commendation for proposing in a general form what ought to have been restrained to a particular case, yet it will by no means follow, that every argument which has been advanced against this doctrine is either intelligible or satisfactory. Of all the objections and experiments which have been started and contrived to refute the new opinions of the German philosophers, there is none which carries a greater degree of plausibility along with it, than a celebrated invention of Mr. MACLAURIN. It is extremely

extremely ſimple, eaſy to be deſcribed; and I do not find that it has ever been anſwered by any of the advocates for the new doctrine of forces.

" Let A and B be two equal " bodies that are ſeparated from " each other by ſprings inter- " poſed between them, in a " ſpace EFGH, which in the



" mean time proceeds uniformly in the direction BA (in " which line the ſprings act) with a velocity as 1; and " ſuppoſe that the ſprings impreſs on the equal bodies A " and B equal velocities, in oppoſite directions, that are " each as 1. Then the abſolute velocity of A (which " was as 1) will now be as 2; and according to the new " doctrine its force as 4: whereas the abſolute velocity " and force of B (which was as 1) will now be deſtroyed; " ſo that the action of the ſprings adds to A a force as 3, " and ſubducts from the equal body B a force as one " only; and yet it ſeems manifeſt, that the actions of the " ſprings on theſe equal bodies ought to be equal, and " M. BERNOULLI expreſsly owns them to be ſo[(e)]." I ſhall only juſt obſerve, that if M. BERNOULLI expreſsly owns, that ſprings, interpoſed between two bodies in a ſpace, which is carried uniformly in the direction in which the

(e) Book II. chap. 2. Account of NEWTON's diſcoveries.

ſprings

ſprings act, will always generate equal forces in the bodies according to his own definition of that term, he talks more inconſiſtently than I have obſerved him to do: on the contrary, if I could find that he has anſwered this famous argument (which Dr. JURIN propoſed over again in Phil. Tranſ. vol. XLIII. with a conditional promiſe of embracing the Leibnitzian doctrine) by ſimply ſaying, that ſprings he conſiders as motive forces, or, when the bodies are equal, as accelerating forces; and that their actions are equal, when in equal times they generate equal velocities, but not neceſſarily equal forces, in the equal bodies; I ſhould not make the leaſt ſcruple to own that I thought his reaſoning ſolid and concluſive, and his diſtinctions a full anſwer to every objection of that ſort(f).

CASE THE THIRD. The two preceding caſes are curious examples of the force of prejudice and party-ſpirit. In the latter particularly it does not appear that J. BERNOULLI

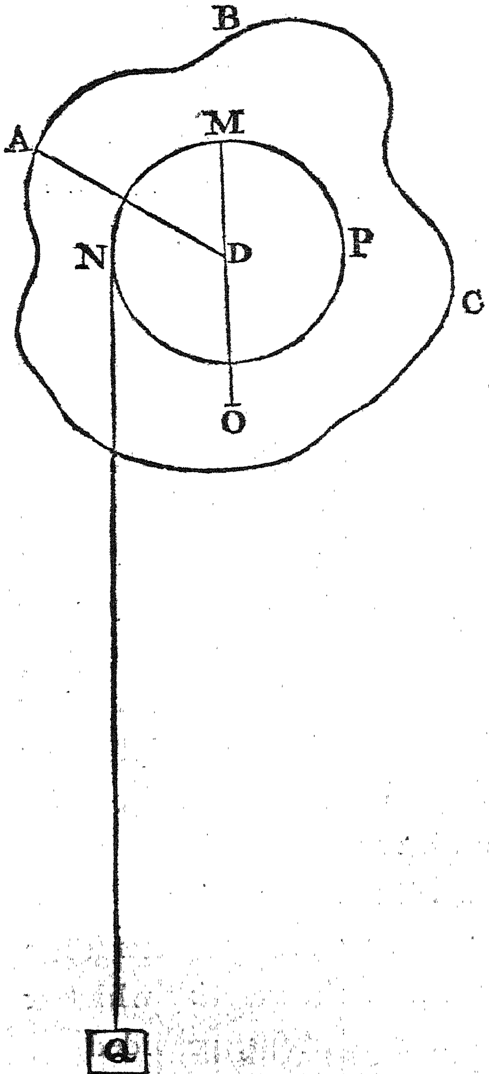
(f) No doubt MACLAURIN refers to the following paſſage of BERNOULLI, "La force du choc, ou de l'action des corps les uns ſur les autres, depend uniquement de leurs viteſſes reſpectives; or il eſt viſible que les viteſſes reſpectives des corps ne changent pas avant le choc, ſoit que le plan ou l'eſpace qui les contient ſoit ſans mouvement, ſoit qu'il ſe meuve uniformement, ſuivant une direction donnée, les viteſſes reſpectives ſeront donc encore les mêmes après le choc."

This quotation puts the matter beyond diſpute. It is plain, BERNOULLI, though he does make uſe of the word action, is only ſpeaking of the motion loſt or communicated, and the relative velocities of the bodies: there is not the moſt diſtant hint at the change in their abſolute forces.

NOULLI knew the preſervation of the *vires vivæ* to be an infallible conſequence of perfect elaſticity in bodies; or indeed that he had any other reaſon for taking that principle for granted, but becauſe he was not able to prove it. All the inſtances that are uſually brought on both ſides are to be treated in a ſimilar way. The meaning of the terms muſt firſt be defined; then the principles aſſumed explained; and if we cannot tell at firſt ſight, whether they are agreeable to experience or not, as is frequently the caſe; we muſt examine into their conſequences by the aſſiſtance of geometry, and we ſhall at laſt arrive at ſome ſimple principle, the exiſtence of which is neceſſarily implied in the original hypotheſis. The colliſion of ſpherical bodies is the moſt ſimple way of communicating motion from one to another; and therefore ſuch examples are better adapted to throw light on a diſputable queſtion, than where the ſuppoſitions are more perplexed with mechanical contrivances. Beſides, when the theory of mechanics is well underſtood, and the foundations of error diſcovered, the ſame reaſonings are eaſily transferred to other caſes, and ſimilar precautions applied. Indeed practical artiſts have little to do with the ſudden communication of motion by impact. The colliſions of bodies are too violent operations to enter into the com-

pofition of ufeful machines, in which motions are rather to be preferved by the gradual effects of weights and preffures. An accurate knowledge therefore of thefe effects is more effential to the interefts of fociety; and the only way of arriving at fuch a knowledge is always to diftinguifh thofe principles which nobody denies, from thofe others which are found to take place only in fome particular circumftances. The following problem was propofed, and a folution given to it long ago, by D. BERNOULLI [g].

(g) Comment. Petrop. tom. II.



"Sit grave aliquod
"cujuſcunque figuræ
"CBA, cujus centrum
"gravitatis ſit D; ex quo
"et radio DM deſcriptus
"intelligatur circulus
"MNP, cui filum cir-
"cumvolutum eſt PMN,
"cujus fili extremitati
"appenſum ſit pondus
"Q, quod deſcenſu ſuo
"grave CBA in gyrum
"agit circum centrum
"gravitatis D, dico velo-
"citatem corporis Q ſe-
"quentem in modum
"determinari poſſe. Sit
"MD $= a$; conſideretur
"corpus ſuſpenſum ex
"puncto M oſcillari, eſſe-
"que centrum oſcilla-
"tionis in O, ſitque
"DO $= b$, pondus gravis totius CBA $=$ P, pondus corporis
"appenſi $= p$; altitudo ex quâ corpus Q delapſum eſt $=$ R;
"altitudo quæſita per quam grave aliquod cadendo ac-

 "quirere

" quirere poſſit velocitatem corporis $Q=z$; dico fore $z=$
" $\frac{ap R}{ap+bP}$, et ſi tempus quo corpus naturaliter cadit, per
" altitudinem R dicatur t, erit tempus inſumptum a cor-
" pore $Q=t\sqrt{\frac{ap+bP}{ap}}$, id quod experientiæ conforme eſſe
" plurimis inſtitutis experimentis ſemper inveni."

Both theſe concluſions are derived by this author from the principle, which they call the *conſervatio virium vivarum*; but as he has not given us the ſeveral ſteps of his reaſoning, it may be uſeful to ſupply them here, before we proceed to make any remarks upon his ſolution. And firſt, ſuppoſe the axis at D to be perpendicular to the plane of the figure, and conceive the whole body to be reſolved into an indefinite number of priſmatic particles, each of which is perpendicular to the ſame plane. Let E repreſent the ſum of all the particles multiplied by the ſquares of their reſpective diſtances from the axis; and E ſhall be equal to $P\times ab$, as is demonſtrated by all the writers who treat of the center of gyration. Let v be the velocity which is actually acquired by Q after it has deſcended through the ſpace R; V the velocity which it would have acquired by the ſame deſcent, provided the body had fallen freely by its gravity; and becauſe the *vires vivæ* are incapable of diminution or increaſe, we

have

have $pV^2 = pv^2 + \frac{Pabv^2}{a^2}$. For ſince v is the velocity of Q at a certain period of its deſcent, and is to the velocity of any priſmatic particle in the body as the diſtance mD from the axis to the diſtance of that particle from the ſame, it is evident that $\frac{Pabv^2}{a^2}$ will truly repreſent the ſum of all the particles multiplied by the ſquares of their velocities. V^2 is therefore to v^2 as $ap+bP$ to ap, and the whole force of gravity is to the force which accelerates the motion of Q in the ſame ratio, becauſe in uniformly accelerated motions, when the ſpaces deſcribed are the ſame, the accelerating forces are in the duplicate ratio of the velocities. It is obvious, that the motion of Q is uniformly accelerated, becauſe the velocity acquired by any deſcent is to the velocity of any point in the body always in the ſame ratio; and therefore the action of Q upon the body is the ſame as if both were at reſt. Farther, the altitude z through which a heavy body muſt fall to acquire the velocity v is plainly equal to $R \times \frac{ap}{ap+bP}$, for the altitudes Z and R are inverſely, as the forces which generate the equal velocities. Laſtly, the time of Q's deſcent is equal to $t \times \sqrt{\frac{ap+bP}{ap}}$; becauſe the times are always in the ſub-duplicate ratio of the ſpaces directly, and forces inverſely.

It

It is now extremely eaſy to trace theſe expreſſions back again in a contrary order, and to ſhew, that if theſe laſt equations are true, the original one muſt be true alſo; that $p \times V^2$ muſt neceſſarily be equal to $\frac{Pabv^2}{a^2} + pv^2$, or, which is the ſame thing, that the body Q multiplied into the ſquare of its velocity, and added to the ſum of all the products which ariſe by multiplying every particle into the ſquare of its reſpective velocity, is equal to the body Q multiplied by the ſquare of the velocity which it would have acquired by the ſame deſcent *in vacuo*.

Now this is to give the argument its full force; and ſince the concluſions are confirmed by repeated experiments, as the author himſelf aſſures us, it is preſumed, that the premiſes can be liable to no juſt exception. If we do not think with the advocates for this doctrine, that the *vires vivæ* muſt always remain the ſame from the thing itſelf, they will force our aſſent by the teſtimony of experience, and oblige us to admit their principles when we find it impoſſible to deny the conſequences.

A prudent philoſopher is always afraid to pronounce generally concerning the exiſtence of cauſes, which are attended with a variety of circumſtances, and are complex in their operations. To ſay that the quantity of force in bodies remains invariably the ſame, ſeems to be

a propo-

a propofition of this kind. The mutual actions of bodies upon one another, efpecially when their gravity is taken into the queftion, depends upon fo many confiderations, and the cafes which may be put are capable of fuch an infinite variation, that it is impoffible almoft to draw a general inference of this nature. Even when experiments are produced, which feem to prove the point, one is apt to fufpect the univerfality of the conclufion, and to imagine that it may poffibly be owing to fome particular circumftance which we have not attended to, or been able to diftinguifh from others not fo effential. In the example we are confidering it is clearly proved from experience, that $p \times V^2$ is equal to $pv^2 + \frac{Ev^2}{a^2}$; but whether that be true in every other cafe that may be conceived, can never be determined from fuch an experiment; nor is it poffible to make any diftinctions about it, until we have demonftrated its connexion with fome other principle, which is more fimple and lefs contefted.

Retaining the fame fymbols, let F reprefent the force of gravity, and f the force which accelerates the body Q in its motion. From what has been already fhewn it appears, that $F : f :: ap + bP : ap$ and $F - f : f :: bP : ap :: \frac{abPv}{a^2} : pv$; and becaufe pv is the motion generated in Q

by the force f, $\frac{abPv}{a^2}$ will be the motion loſt in the ſame body Q by the diminution of its gravity. Let A be any priſmatic particle of the body, and AD its diſtance from the axis; the velocity of this particle will be $\frac{v \times AD}{a}$; its motion $\frac{A \times AD \times v}{a}$, and, by the nature of the lever, the motion which Q muſt loſe to generate ſuch an effect in A muſt be $\frac{A \times AD^2 \times v}{a^2}$. The quantity $\frac{vabP}{a^2}$ repreſents the ſum of all the quantities $\frac{A \times AD^2 v}{a^2}$; and therefore the motion, which Q has loſt by its action on the body, is preciſely equal to the motion gained by the different parts of that body after a proper allowance is made for the lengths of the levers, AD, &c.

Thus it appears, that there is no neceſſity in accounting for the time of Q's deſcent and the velocity it acquires, of having recourſe to the *conſervatio vis vivæ*, or any ſuch perplexed hypotheſis. By purſuing the analytic method far enough, we have been led directly to that fundamental law of motion, that action is equal to re-action, and in the contrary direction.

A diſtinction, however, is always to be made between the actions of bodies when at liberty, and when they revolve about a center or axis. In the firſt caſe the motion loſt

loſt is always equal to the motion communicated in an oppoſite direction: in the ſecond the motion loſt is to be increaſed or diminiſhed in the ratio of the levers before it will be equal to the motion communicated. The properties of the lever are well underſtood and eaſily applied, and becauſe their evidence depends upon experience, and is as firmly eſtabliſhed as the third law of motion itſelf, it is always beſt to make uſe of thoſe two univerſal principles, inſtead of others which are more liable to deceive us(b).

In all caſes concerning the motion of a ſingle body, or ſyſtem of bodies, where there is any rotatory motion, the conſideration of the lever becomes requiſite, and that, with a juſt application of the laws of motion, is ſufficient for the reſolution of the moſt arduous problems. It is

(b) It is acknowledged, that the experiments which have been made to determine the effects of wind and water-mills do not agree with the computations of mathematicians; but this is no objection to the principles here maintained. Writers generally propoſe ſuch examples with a view rather of illuſtrating the methods of calculation by algebra and fluxions, than of making any uſeful improvements in practice. They ſuppoſe the particles of the fluid to move in ſtraight lines, and to ſtrike the machine with a certain velocity; and after that, to have no more effect. As ſuch ſuppoſitions are evidently inconſiſtent with the known properties of a fluid, we are not at a loſs to account for a difference between experiment and theory; and therefore it ſhould ſeem unreaſonable to aſſert, that certain authors of reputation have neglected the collateral circumſtances of time, ſpace, or velocity, in the reſolution of theſe problems, unleſs we were able to point out ſuch omiſſions.

now pretty well agreed upon, that the neglect of this circumftance is one caufe of that material error, which Sir ISAAC NEWTON himfelf is fuppofed to have fallen into in the thirty-ninth propofition of the third book of his Principia.

I had feveral reafons for infifting fo particularly on the demonftration of this third cafe. It is in itfelf one of the moft neat and elegant problems we have; and, what is of more confequence, it admits of an experimental proof and illuftration. It is obvious, that the motion of the body AMB may be made fo flow, that the time of Q's defcent through any affignable fpace may be meafured to the greateft exactnefs. The velocity of Q may alfo be inferred with the fame eafe by obferving the velocity of any particular point in the body to which the velocity of Q always bears an invariable ratio. Such experiments, it muft be owned, feem very unfit for the firft difcovery of the laws of nature; though, as I have fhewn, it is not impoffible to collect them that way; but after they are difcovered, the application of them to the folution of fuch intricate problems is both entertaining and inftructive, and then the agreement of the experiments themfelves with the theory becomes a folid argument for the certainty of our principles.

We have ſhewn, that in this caſe at leaſt BERNOULLI's hypotheſis is founded upon, and coincides with, the commonly received doctrine of motion, and therefore we can hardly entertain a doubt of the ſucceſs of the experiment, ſuppoſing it had never been tried. The author himſelf, in the paſſage above quoted, tells us, that he found it ſo; but we need not reſt upon his authority: a ſimilar experiment has been lately made by Mr. SMEATON, and is deſcribed at length in the Philoſophical Tranſactions, vol. LXVI.

It does not appear, that D. BERNOULLI attempted to meaſure any thing but the time of Q's deſcent through any particular ſpace: Mr. SMEATON has given us both the times of Q's deſcent, and the proportions of the velocities acquired, in a variety of caſes. By moving the weights he makes uſe of nearer to, or farther from, the center D, he alters the lengths of the levers at which the particles act, without increaſe or diminution of their number: he does the ſame with the circle or axis NMP, and conſequently the lever MD; and in every caſe, from the known character of that ingenious gentleman, we may preſume that his numbers are ſafely to be relied upon.

His concluſions may receive ſome illuſtration from the preceding theory.

From the proportion $F : f :: ap + bP : ap$, it appears, that the force which accelerates the motion of Q, or in Mr. SMEATON'S figure, the weight in the ſcale is to the natural force of gravity in a conſtant and invariable proportion as long as the quantities a, b, P, and p, remain the ſame; and therefore let Q deſcend ever ſo ſlowly, its motion will be uniformly accelerated throughout, and the ſpaces through which it deſcends will be as the ſquares of the velocities acquired, and the times will be as the velocities themſelves; and this is agreeable to what Mr. SMEATON found them in his ſecond and third, fifth and ſixth, eighth and ninth experiments.

The general expreſſion for the force which accelerates the weight in the ſcale is $\frac{ap+bP}{F \times ap}$, and will be different according as the quantities a, p, or b, are altered; but is always eaſy to be determined as ſoon as thoſe quantities are known. But it is impoſſible to determine the magnitude of the quantity b in the different caſes, unleſs we have given the preciſe dimenſions of the whole machine, and the ſpecific gravity of the wood made uſe of; and therefore I confeſs myſelf to have been puzzled in endeavouring to reconcile the firſt and ſecond and other experiments with the theory: for though I could not doubt a moment, that the general expreſſion for the force was rightly

rightly aſſigned, and would always be found conſonant to experience, yet I was extremely ſurprized to find, that when the quantity a in the ſecond experiment was made exactly one-half of what it was in the firſt, the time of deſcending through the ſame ſpace came out nearly double of what it was before, and the velocity the ſame. Now this I knew could never happen unleſs the force in the firſt caſe was to the force in the ſecond as 4 to 1; for when the ſpaces deſcribed are the ſame, the accelerating forces are always as the ſquares of the velocities, or inverſely, as the ſquares of the times. This conſideration led me to inquire farther into the ratio of thoſe forces in the caſe deſcribed, in order to diſcover, if poſſible, whether they came any thing near that ratio, which of neceſſity they ought to do.

I conſidered, that the weight of the axis and arms of the machine was inconſiderable, compared with the weight of the two cylinders of lead, and alſo that the quantity a bore a very ſmall proportion to the length of the cylindrical arms of fir. And ſince the accelerating force is always as $\frac{ap}{ap+bP}$, or as $\frac{a^2p}{a^2p+abP}$, and the quantity abP or E expreſſes the ſum of all the particles multiplied by the ſquares of their diſtances from the axis of motion, it is plain that E muſt far exceed a^2p; and, laſtly, ſince the quantity E is

6 the

the ſame both in the firſt and ſecond experiment, it follows, that the forces are very nearly to one another as a^2p to $\frac{a^2p}{4}$, or as 4 to 1: and in the ſame way the other experiments are ſhewn to be conſiſtent with the theory.

I choſe to premiſe a ſhort account of the opinions which the philoſophers before GALILEO entertained concerning the motions of bodies; becauſe their miſtaken ideas of the effects of gravity are analogous to ſome opinions of a later date, which indeed ſuggeſted the neceſſity of reſuming theſe inquiries.

And as nothing in controverſial matters ſo completely ſatisfies the mind as an exact knowledge of that particular which produces the diſpute; I have ſhewn, that the terms made uſe of to expreſs the third law of motion were taken in two very different ſenſes: that Sir ISAAC NEWTON's explication of them is at beſt ambiguous, and MACLAURIN's abſolutely falſe.

1ſt. In the demonſtration of the firſt caſe we ſee that the aſſertion of LEIBNITZ is true in one particular inſtance. When two elaſtic balls move in the ſame ſtraight line, the ſum of their forces is not altered by colliſion; and it is more than probable, that this ſingle circumſtance was the cauſe of affixing new ideas to the terms action and re-action. For,

2d. In

2d. In the ſecond caſe, the ſame principle is taken for granted by J. BERNOULLI. We have examined into the conſequences of this author's ſolution, and ſhewn that his hypotheſis will prove all bodies to be perfectly elaſtic. As the ſteps by which he deceived himſelf are here expoſed, whoever carefully attends to theſe two examples cannot eaſily miſtake in any caſe that may occur. It is plain, that if any one contends for the equality of action and re-action, and explains thoſe terms by the changes produced in the abſolute forces of the bodies, the diſpute is not merely verbal.

3d. When a concluſion, agreeable to experience, is deduced from any hypotheſis, it does not therefore neceſſarily follow, that the hypotheſis is univerſally true, not even ſuppoſing the converſe of the propoſition to hold. In this third caſe it is ſhewn, what kind of anſwer we are to give ſuch reaſoning. The *conſervatio virium vivarum* is never to be admitted, unleſs its connexion with ſimple facts, which are inconteſtable, be firſt made out. The ſolution of this problem depends on this, that the motion loſt is equal to the motion communicated in a contrary direction after the property of the lever is taken into the account; and therefore the nice agreement of Mr. SMEATON's experiments with the

theory

theory cannot fail to add frefh evidence to thefe efta-blifhed laws of nature.

I fhall conclude thefe remarks with obferving that fince it is perhaps impoffible to give one general anfwer to all the arguments which are brought in favour of the new doctrine of forces, it feemed very defireable that we fhould have a general rule to direct us in judging of the cafes that occur in practice. It is of more confequence to the improvement of fcience and the good of the public, to point out the fource of miftakes, and the wifeft means of avoiding them for the future, than merely to confute and filence our adverfaries. Some writers have con-fidered this queftion as entirely verbal, and have affected to treat the advocates on both fides with the greateft con-tempt. Such perfons fave themfelves a great deal of trouble, and have the credit of feeing farther into the controverfy than others; but after all, I am afraid the practical mechanic will receive little information or fecurity from fuch fpeculations. Propriety of expreffion in thefe matters is not all we want. When a plan is pro-pofed for execution, and a certain effect predicted, the grand object is, how to form a fure judgement before-hand of the event, in order to prevent unneceffary ex-pences; and I fhall think my time well employed, if thefe

confi-

conſiderations appear to have the leaſt tendency to promote ſo uſeful an end, in the opinion of that Society to whoſe learned and zealous endeavours we owe the very firſt important diſcoveries in the year 1668, concerning the colliſions of bodies.

XVIII. *Obſervations on the Limits of Algebraical Equations; and a general Demonſtration of* Des Cartes's *Rule for finding their Number of affirmative and negative Roots. By the Rev.* Iſaac Milner, *M. A. Fellow of* Queen's College, Cambridge. *Communicated by* Anthony Shepherd, *D. D. F. R. S. and Plumian Profeſſor at* Cambridge.

Read February 26, 1777.

§ 1. THE inveſtigations of the limits of equations is conſidered as one of the moſt important problems in algebra. The knowledge of them not only enables us to demonſtrate many uſeful theorems in that ſcience, but is alſo of material ſervice in diſcovering the roots themſelves. Mr. MACLAURIN has treated this ſubject very fully, both in his Algebra and in the Philoſophical Tranſactions.

The ſubſtance of what he has delivered may be briefly expreſſed in the two following propoſitions.

1ſt. That any equation $x^n - px^{n-1} + qx^{n-2} - \&c. = 0$ being propoſed, if you take the fluxion of this equation, and

and divide it by x, the reſulting equation will have all its roots limits of the roots of the given equation.

2dly, If the terms of the propoſed equation be multiplied into the terms of any arithmetical ſeries, the reſulting equation will alſo have its roots limits of the roots of the original equation.

§ 2. This ſecond propoſition, though admitted by all the eminent authors whom I have had an opportunity of conſulting, certainly requires ſome reſtrictions. For example, the roots of the quadratic equation $x^2-2x-3=0$ are 3, &c. -1; multiply the terms of this equation into the terms of the arithmetical progreſſion 1, 2, 3, reſpectively, and the reſulting equation is $1\times x^2-2\times 2x-3\times 3=0$, the roots of which are $2\pm\sqrt{13}$, neither of which are between the roots of the given quadratic.

Again, ſuppoſe the roots of the cubic equation $x^3-px^2+qx-r=0$ to be a, b, $-c$, and it is poſſible that the equation $\overline{l+3m}\times x^3-\overline{l+2m}\times px^2+\overline{l+m}\times qx-lr=0$ may have no root between the quantities b and $-c$; and in general, if the roots of the equation (A) $x^n-px^{n-1}+qx^{n-2}$, &c. $=0$ be ſuppoſed a, b, c, $-d$, $-e$, $-f$, &c. where a is the greateſt root, b the next, and ſo on in order, the equation (B) $\overline{l+nm}\times x^n-\overline{l+n-1}.mpx^{n-1}+\overline{l+n-2}.mqx^{n-2}$, &c. $=0$ will not neceſſarily have any of its roots between the roots c and $-d$ of the original equation.

§ 3. It will not be difficult to see the reason of this, if we examine the demonstration, which is usually given us of this second proposition.

The roots of the biquadratic equation $x^4 - Ax^3 + Bx^2 - Cx + D = 0$ are supposed to be a, b, c, d, and the results which arise by successively substituting them for x in $4x^3 - 3Ax^2 + 2Bx - C$ are supposed to be $-R$, $+S$, $-T$, $+Z$. From which MACLAURIN concludes, that when $abcd$ are substituted for x in the quantity $\overline{l+4m}\times x^4 - \overline{l+3m}\times Ax^3 + \overline{l+2m}Bx^2 - \overline{l+m}Cx + lD$, the quantities that result will become $-mRx$, $+mSx$, $-mTx$, $+mZx$, where, says he, the signs being alternately negative and positive, it follows, that a, b, c, d, must be limits of the equation $\overline{l+4m}\times x^4 - \overline{l\times 3m}Ax^3 + \&c. = 0$.

Here it is taken for granted, that the quantities $-mRx$, $+mSx$, $-mTx$, $+mZx$, are alternately negative and positive, which is not true, unless the roots a, b, c, d, be either all positive or all negative.

For suppose a, b, c, to be positive [(a)] quantities, and d a

(*a*) Philosophical Transactions, vol. XXXVI. Mr. MACLAURIN, who is here very diffuse upon this subject, never mentions any exception of this sort.

In his Algebra, art. 44. part 2. he says, he shall only treat of such equations as have their roots positive; but it may be observed, that his reasoning from art. 45. to 50. holds in all equations, the roots of which are real. The theorem in p. 182. of that treatise is not general, though applied in the eleventh chapter to the demonstration of NEWTON's rule for finding impossible roots in all equations.

negative

negative one; and then the four refults will be $-m$ R a, $+m$ S b, $-m$ T C, $-m$ Z d.

§ 4. In general, the roots of the equation $nx^{n-1}-\overline{n-1}\cdot px^{n-2}+\overline{n-2}\cdot qx^{n-3}$, are always between the roots of the equation (A) becaufe the roots of this laft equation fubftituted fucceffively for x in $nx^{n-1}-\overline{n-1}\cdot px^{n-2}+$ &c. always give the refulting quantities alternately negative and pofitive; but when the leaft of the affirmative roots, and the greateft of the negative roots of the equation (A) are fubftituted in (B) the quantities that refult will neceffarily have the fame fign, and therefore it is poffible, that no root of the equation (B) may lie between the leaft of the affirmative and the greateft of the negative roots of the equation (A).

§ 5. It is poffible even, that the equation (B) may have imaginary roots, at the fame time that all the roots of the equation A are real, which is contrary to what all algebraical writers have thought. For inftance, the roots of the equation $x^2+6x-7=0$, are 7 and -1, and if the terms of this equation be multiplied by 1, -1, 3 (an arithmetical feries where the common difference of the terms is equal to 2) the refulting equation will be $x^2-6x+21$, the roots of which are evidently impoffible.

§ 6. However, the equation (B) can never have more than two imaginary roots, when the roots of the equation (A)

are real. For fuppofe thefe laft roots to be $+a$, $+b$, $+c$, $+d$, $-e$, $-f$, &c. in their order from the greateft to the leaft, and fince the refults which arife from the fucceffive fubftitution of thefe quantities are always alternately negative and pofitive, that cafe only excepted where d and $-e$ are fubftituted, it is manifeft, that we fhall always have $n-2$ of the roots of the equation (B) which will be limits of the equation (A).

§ 7. It is remarkable, that whenever the equation A has all its terms complete, its roots real, and fome of them pofitive, and others negative, if $l+nm$ be affumed equal to o, the equation B will always have one of its roots either greater than the greateft affirmative root, or lefs than the leaft negative root of the equation (A). Thus, in the quadratic $x^2+6x-7=o$, affume any arithmetical progreffion 0, 1, 2, the firft term of which is equal to nothing, and the equation B in this cafe is $6x-14=o$ and $x=\frac{14}{6}$, which is greater than 1, the greateft affirmative root of the affumed equation.

§ 8. The roots of the equation (A) being ftill fuppofed a, b, c, d, $-e$, $-f$, &c. let m be taken equal to unity, and l any pofitive integer whatfoever, and in that cafe, two of the roots of the equation B will lie between the roots d and $-e$, and one of them will be pofitive, and the other negative.

For

For example, the quadratic equation $x^2+6x-7=0$ has its roots 1 and -7; and if the terms of this equation be multiplied into 3, 2, 1; 4, 3, 2; or 5, 4, 3, ſucceſſively, the reſulting quadratic in every caſe will have its two roots between the roots of the given equation, and one of them will be poſitive, and the other negative.

§ 9. The equation B, which in the laſt article was deduced from the equation A by taking m equal to 1, and l any poſitive integer, may itſelf be treated in the ſame way, and the reſulting equation will, *à fortiori*, have two of its roots between the roots d and $-e$ of the original equation, and one of them will be poſitive, and the other negative.

§ 10. Let $x^2-px+q=0$ repreſent any quadratic equation, the real roots of which are α and β; ſuppoſe $x=\frac{1}{y}$, and we ſhall have $1-py+qy^2=0$, the roots of which equation are $\frac{1}{\alpha}$, $\frac{1}{\beta}$. Let the root of the equation $2qy-p=0$ be equal to $\frac{1}{A}$, and $\frac{1}{A}$ will always lie between the quantities $\frac{1}{\alpha}$, $\frac{1}{\beta}$, and therefore one would think at firſt ſight that the quantity A muſt always lie between α and β. But this would be contrary to what is proved in art. 7. In the preſent caſe A can never lie between α and β, unleſs theſe two quantities have the ſame ſign, and it is obvious,

obvious, that the ſame reaſoning holds in equations of higher dimenſions.

Theſe obſervations, as far as I know, are intirely new. The fundamental propoſition (§ 4.) was, in the year 1775, communicated to Dr. WARING, Lucaſian profeſſor of mathematics in this univerſity, and by him inſerted among the additions to his *Meditationes Algebraicæ*[(c)].

§ 11. M. EULER, at the concluſion of his 13th chap. *Calcul. Different.* has given a demonſtration of DES CARTES's rule for finding the number of affirmative and of negative roots in any equation, the roots of which are real. From what I have already ſaid, his reaſonings will appear inconcluſive, though I freely own, that what he has done ſuggeſted the following different method.

Suppoſe (D) $L+mx+nx^2+Px^3 \ldots +x^n=0$, and the roots of the equation (E) $m+2nx \ldots\ldots +nx^{n-1}=0$ will be limits of the roots of the equation (D); and therefore there muſt be at leaſt as many poſitive roots in the equation (D) as there are in the equation (E). The ſame may be ſaid of the negative roots: for ſince every root of the equation (E) lies between the different roots of the equation (D), it is impoſſible that the number of roots ſhould be leſs in either caſe. Suppoſe L and Mx to be both poſitive, and ſince the laſt term in

(c) See the end of Proprietates Curv.

any

any equation is always the product of all the roots with their ſigns changed, the number of poſitive roots in each of the equations (D) and (E) muſt be even: therefore, the number of poſitive roots in (D) cannot exceed the number of thoſe in (E) by unity; but there is in (D) one root more than in (E), and conſequently it muſt be negative.

If both the terms L and mx are negative, becauſe then the number of poſitive roots in (E) and (D) are even, it follows in the ſame way, that there is one negative root more in (D) than there is in (E).

And laſtly, if the terms L and mx have different ſigns, for the ſame reaſons there muſt be one poſitive root more in the equation (D) than there is in (E).

DES CARTES' rule is, that there are as many poſitive roots in any equation as there are changes in the ſigns of the terms from + to −, or from − to +, and that the remaining roots are negative. From what has been demonſtrated it appears, that if this rule be true in the equation (E), it muſt hold alſo in the next equation (D) of ſuperior dimenſions; and as we know that it is true in ſimple and quadratic equations, it muſt therefore be true in cubics, in biquadratics, and ſo on.

This is one of the beſt rules we have in algebra. Dr. SAUNDERSON [c] ſaw ſuch an infinity of caſes in equa-

(c) Vol. II. p. 683. Algebra.

tions of high dimenſions, that he ſcarcely hoped for a general proof. MACLAURIN's[d] method is plainly impracticable when the roots are numerous, and therefore this conciſe demonſtration will perhaps be acceptable to mathematicians.

(*d*) Page 145. Algebra.

1775	Th.	Marine Barometer R.	Marine Barometer S.	Marine Barometer Th.	Marine Barometer D.	Weather and Winds.	D.L. A.O. N	D.L. A.O. S	D.L. A.T.K. W	D.L. A.T.K. E	Longitude from Greenwich A.	Longitude from Greenwich TK.	Longitude from Greenwich D.☽ T.K.	Longitude from Greenwich ☽	S.	O.	E D.	Latitude A.	Latitude O.	Cor. Long.	Magnetical Variation Az.	Magnetical Variation Amp.	Magnetical Dip Face E.	Magnetical Dip Face W.	Magnetical Dip MD.	Magnetical Dip C.D.		ML.	N. B. Log-line 51 feet to 29″$\frac{1}{2}$.
	°	In.Dec.	In.Dec.	°	In.Dec.		′	′	′	′	° W ′	° W ′	W ′	° W ′			′	° N ′	° N ′	° W ′	° W ′	′ W °	°	°	°	°			
Apr. 28						f. h. f. SE. ESE. S by E. E by S. ESE. SE.													49.43				72.4	73.6	73.1	73.1			Between Ram-head and the Lizard.
29						f.h. SE.V.WbS.WSW.SWbW.SW.WSW.V.C.V.WSW.													49.50	5.35			72.4	73.7	73.1	73.1			Lizard Point E$\frac{1}{4}$N=ENE$\frac{1}{2}$N. 7 or 8 leagues off ſhore.
30			30.2$\frac{1}{2}$	55		h. r. SW by S. WSW. W. h. WNW. NW by W. h. r. WNW. C. NNW. r. f. NNW. NW by N. NW.		6		7	6.19	6.12						49.25	49.19	6.12			73.	74.	73.4	73.4	Great ſwell.	37	At 1 P.M the Lizard Point bore by compaſs E$\frac{1}{4}$N. Variation allowed ENE$\frac{1}{2}$N 7 leagues. The Land's End N by E=N by W. Take a departure from the Lizard in 49° 57′ 30″ N lat. Long. 5° 15′ W. Firſt and middle part, light airs and calm. Latter, pleaſant breezes. Great ſwell from W.
May 1	59		30.4$\frac{1}{2}$	61		f. NW by N. NW by W. WNW. f. NW by W. NW. f. WNW. W by N. f. W. WSW. W by S.		11		14	6.40	6.19						48.42	48.25	6.19			72.	73.4	72.6	72.6		51	Light breezes; great ſwell from Weſtward.
2	57			56		h. fr. WSW. SW by W. hr. r. SW by S. SW. W by S. r. h. fr. W. WNW. h. r. f. h. NW by W. WNW. NW by W.		9	33		7.22	7.34						48.51	48.43	7.34			72.4	73.	72.6	72.6	Great ſwell.	47	Major part light breezes; great ſwell from Weſtward.
3	54$\frac{3}{4}$		30.0$\frac{1}{2}$	54		c. NW by N. C. c. C. c. C. E. c. h. E. E by N.					7.29							48.34	[48.26]	[7.41]			72.	72.4	72.2	72.2		18	Moſtly calm. A very large ſwell from Weſtward.
4	56$\frac{1}{2}$		30.0$\frac{3}{4}$	56$\frac{1}{4}$		h. ENE. h. ENE. h. c. ENE. V. c. NE. V. C.		31	9		7.57	8.18						47.51	47.20	8.18			71.4	72.6	72.1	72.1	Very ſteady.	48	Light breezes and a great ſwell.
5	57$\frac{1}{2}$		30.0	57$\frac{1}{2}$		h. C. W by S. c. h. W. WSW. SW. h. r. SW. WSW. h. fr. c. W by S. WSW. NE by N.		13		7	7.13	7.27						46.56	46.12	7.27			71.	72.4	71.6	71.6	Great motion.	68	Firſt part, calm. Middle, an increaſing breeze from Weſtward, with rain. Latter, a fine breeze from the Northward.
6	55$\frac{3}{4}$		30.2	55$\frac{1}{2}$		h. N. NNE. h. NNE. NE. ENE. h. c. ENE. ESE. E by N. c. f. E by N. E by S.		19		12	8.36	8.38						45.58	44.55	8.38		18.42*	71.	72.	71.4	71.4	Good deal of motion.	83	Moſtly light breezes, with a ſwell from NW.
7	57		30.2$\frac{1}{4}$	56$\frac{1}{2}$		f. ESE. f. E. f. E. f. E. E by N. ENE.		21	4		10.13	10.19						44.14	42.50	10.19			69.6	71.	70.3	70.3	Very ſteady.	126	An increaſing gale from the Eaſtward.
8	61$\frac{1}{4}$		30.2$\frac{3}{4}$	60$\frac{1}{4}$		h. NE by E. NNE. h. NNE. h. NNE. h. NNE. NE.		39	13		10.22	10.43	3 14	10.40 10.29	4 4	○	27 46	41.52	40. 9	10.43			68.4	69.2	68.7	68.6	Ditto.	142 620	Firſt part, freſh gales. Latter, moderate.

T.S. denotes the temperature of the sea-water taken up in a bucket about noon, and the thermometer immersed in this bucket for some minutes.

1775	Th.	Marine Barometer R.	Marine Barometer S.	Marine Barometer Th.	Marine Barometer D.	Weather and Winds	D.L. A.O. N	D.L. A.O. S	D.L. A.T.K. W	D.L. A.T.K. E	Longitude from Greenwich A.	Longitude from Greenwich TK.	D.☽ T.K.	☽	S.	O.	E D.	Latitude A.	Latitude O.	Cor. Long.	Magnetical Variation Az.	Magnetical Variation Amp.	Dip Face E.	Dip Face W.	MD.	C.D.		M L.	
	°	In.Dec.	In.Dec.	°	In.Dec.		′	′	′		° ′ W	° ′ W	′ W	° ′ W			′	° ′ N	° ′ N	° ′ W	° ′ W	′ ° W	°	°	°	°		1273	
May 18	69	30.2	30.1¾	68½	30.2	h. ENE. h. ENE. E. h. E. ENE. h. E. ENE.		15		10	17.9	16.46						29. 4	28.33	16.46	*18.23		61.2	62.6	62.	61.6		53	Light breezes and smooth water. At noon, Peak of Teneriff S 50° E.
19	68	30.2½	30.2¼	67¾	30.2¼	h. E by N. h. NE by N. h. c. N by W. C. N by W. NNE. c. f. NNE.		6	1		17.17	16.55						27.53	27.16	16 55	16.53* *16.39	17. 6*	60.2	61.2	60.6	60.4		74	Pleasant breezes from the Eastward. At noon, Peak of Teneriff (variation allowed) bore N 24° 30′ E, which bearing, and the difference of latitude places it 27′ to the Eastward of our situation at noon, when in 16° 55 long. by time-keeper, and consequently by time k. 16 28 is the longitude of Peak of Teneriff, only 4′ less than Pere FEUILLE makes it; but, by the observations of the Moon, on the 21st and following days, it appears, the ship was 1° farther West than the time-keeper represented, which difference arose since the 9th of May, if the Lunar observations were to be relied on.
20	68	30.1¼	30.1½	68	30.1¼	c. NNE. NE. c. NE. N by E. NE by N. N by E. c. N by E. NNE. c. NNE.		24		3	17.42	17.17						26.46	25.45	17.20	16.37* 15.42*	16.17* 15.42*	59.2	60.2	59.6	59.4	Very steady.	70	Light breezes and smooth water throughout. Several petrills in the wake.
21	68¼	30.1¼	30.1	68	30.1	f. c. NE by N. N by E. c. N by E. N by W. c. f. N by W. N by E. f. N by E. NNE.		1	4		18.18	17.57	57	18.54	4	☉	29	25. 5	24. 3	18. 3		15 36*	58.2	59.2	58.6	58.4		106	Pleasant trade. In Captain EKEBERG's chart there being soundings laid down not far to the Eastward of our situation, sounded at 8 P.M. but had no ground 40 fath. Several petrills in the wake of ship.
22	70	30.1½	30.1¼	70¼	30.1½	f. N. f. c. N. NNE. c. NNE. c. NNE. NE by N. NE.		18			18.49	18.28	66	19.34	3	☉	28	23.43	22.25	18.37	15.13*	*15. 2	56.4	57.4	57.	56.6		87	Light breezes with smooth water throughout. Many petrills. N.B. There was apparently an error in this day's long. by time-keeper. The succeeding days diff. long. was as much less as this day's was more than account. I have here corrected it as conformable to the Lunar observations. T.S. 69°.
23	68	30.2	30.1¾	68	30.2	f. NE. NE by N. f. NE by N. f. NE by N. f. NE by N. NE.				1	19.28	19.6	41 65	19 47 20.11	4 3	☉ ☉	47 71	21.54	[20.36]	19.18	14.35* *13. 9	*13. 7	54.	55.2	54.5	54.3		115	In the evening, the water appeared of a muddy green, like soundings; but did not sound. An increasing trade with fair weather, the water continuing of same colour. T.S. 69°.
24	68¼	30 1	30.0½	68¼	30.0¾	f. NE. NE by N. f. NE by N. f. c. NE by N. c. h. NE by N.		14	8		19.21	19.7	79	20.26	3	☉	37	19.21	17.49	19.22	13.56*	13.55*	51.	52.4	51.6	51.3	Much motion.	153	These 24 hours a fresh trade, and mostly fair weather, the water remaining the same as before. T.S. 68°½.
25	71¾	30.0	29.9½	72	29.9¾	h. N by E. h. N by E. h. N by E. h. NNE.		1	4		18 33	18.23						17. 1	15.28	18.41	13.18*	12.32*	48.6	49.4	49.1	48.6	Very steady.	150	Fresh trade and hazy weather. In the afternoon the sea remarkably confused, breaking different ways. Sounded in the morning, but had no ground 40 fath. In the morning the water appeared blue again. T.S. 71°.
26	76½	30.0½	30.0	76½	30 0	h. NE by N. h. V. WNW. NW. h. NW. V. NNE. h. c. NNE. E by S. ESE.		8		2	18.37	18.25						16. 3	14.22	18.46			46.4	48.	47.2	46.7		59	Light breezes chiefly. In the night, no ground 70 fath. There were many blubbers in the ship's wake, which made a very luminous appearance: they seemed in shape like a sausage about 8 or 9 inches long. In the day, sea was covered with Portugueze men of war, of which took up several; also some pilot-fish and skip-jacks. T.S. 74°.
																												2140	

1755	Th.	Marine Barometer, R.	S.	Th.	D.	Weather and Winds.	D.L. A.O. N	S	D.L. A. T.K. W	E	Longitude from Greenwich A.	TK.	D.☽ T.K.	☽	S.	O.	E.D.	Latitude A.	O.	Cor. Long.	Magnetical Variation Az.	Amp.	Dip Face E.	W.	MD.	C.D.		M.L.	
		In.Dec.	In.Dec.	°	In.Dec		′	′	′	′	° ′	° ′	′	° ′				° ′	° ′	° ′	° ′	° ′	°	°	°	°		2577	
											W	W	E	W				N.	N.	W	W	W							
June 5	$80\frac{3}{4}$	$30.0\frac{3}{4}$	$30.0\frac{1}{2}$	$80\frac{1}{4}$	$30.0\frac{1}{2}$	h. c. C. W by S. c. L. all rd. r. W by S. WSW. V. NNW. r. h ſq. and hr. ſq. r. NNW. SE by E. E by S. V. h. V. S by E. SSE.	10			20	15.43	14.28						9.43	8.40	15.43			38.2	38.4	38.3	37.7		48	Moſtly light breezes. Paſt 2 P.M. a hard ſquall, with great deal of thunder, lightning, and rain. P.M. a boat came on board from the Polly, a whaling veſſel, from *Boſton* in New England; the veſſel was hull down when they came aboard. A.M. ſpoke with the Mary, Captain JONES, from Liverpool to Sierra Leon. P.M. 40 to 36 fath. coarſe ſand. A.M. no ground 55 fath. T.S. 83°¼.
6	83	$30.0\frac{3}{4}$	$30.0\frac{3}{4}$	83	$30.0\frac{3}{4}$	f. C. V. f. L. to Eaſtw. SW by W. h ſq. r. L. all rd. r. c. E by S. SE. E. E by N. c. f. E by N. C. SSE. S by E.	11		8		15.41	14.34						9.12	8.20	15.53	*13.33		36.6	37.2	37.	36.4	Very ſteady.	38	Moſtly light airs and calms. At midnight, a violent guſt of wind came off: its violence did not continue above a minute or two; but brought much rain, and was attended with thunder and lightning. A.M. no ground 88 fath. T.S. 82°.
7	$82\frac{1}{2}$	$30.1\frac{1}{2}$	$30.1\frac{1}{4}$	$82\frac{1}{4}$	$30.1\frac{1}{4}$	h. SE by S. S by E. C. h. c. V. SSE. SE by S. c. ſq. S by W. V. C. ſq. c. C. S by E. SSE. S.	11		19		15.41	14.53						8.56	8.15	16.16		12.28*	36.6	37.6	37.2	36.6		31	Light airs chiefly: a long ſwell from the SW. T.S. 82°½.
8	83	$30.1\frac{1}{4}$	$30.1\frac{1}{4}$	83	$30.1\frac{1}{4}$	c. hr. and L. to s.w. S. C. V. SW. ſq. c. ſq. SW. SW by W. SW. c. C. c. f. C.	23			12	15.22	14.22						8.33	8.15	15.49			36.6	37.6	37.2	36.6	Very unſteady, great ſwell, and no ſail.	30	Firſt, light airs. Middle, freſh breezes and ſqually. Latter, calm. At midnight, carried away the main and fore-top maſt by the badneſs of the rigging. Had, for ſome hours, a pretty freſh SW wind; but not enough to have endangered the top-maſts had the rigging been good. At 11 got fore-top-maſt up. T.S. 83°.
9	84	$30.0\frac{1}{2}$	$30.0\frac{1}{2}$	84	$30.0\frac{1}{2}$	h. C. h. L. to weſtw. C. h. c. C. W by S. c. W by S.		3		3	15.18	14.14						8.29	8. 8	15.46			36.4	37.6	37.1	36.5	Pretty ſteady.	6	Moſtly calm, with ſwell from Southward. At 9 A.M. got up main-top-maſt. T.S. 82°.
10	$81\frac{1}{4}$	$30.0\frac{1}{2}$	$30.0\frac{1}{2}$	$80\frac{3}{4}$	30.0	f. c. ſq. r. W. NNE. ſq. r. c. NNE. V. C. c. L. to Ed. hr. C. V. C. ENE. r. c. ENE. V. SW by W.	3		19		15.0	14.15	53	15.8	5	*S	48	8. 1	7.43	15.51			35.6	36.6	36.2	35.6	Very ſteady.	35	Firſt, fair. Middle, ſqually, with hard rain. Latter, variable, with calms. T.S. 81°½.
11	$77\frac{1}{4}$	$30.0\frac{1}{2}$	$30.0\frac{1}{2}$	$76\frac{1}{2}$	$30.0\frac{1}{2}$	c. ſq. r. SW. SW by S. c. f. c. ſq. r. SW. ſq. r. SSW. SW. WSW. hr. r. WSW. V.					14.19							7. 9	[7.14]	[14.53]			34.6	35.6	36.2	35.6	remarkably ſteady.	64	Cloudy and rainy, with a gentle breeze: great ſwell from Southw. T.S. 81°.
12	82	$30.0\frac{3}{4}$	$30.0\frac{1}{4}$	82	$30.0\frac{1}{4}$	hr. V. SW. fr. r. c. SW. SW by S. SSW. c. r. SSW. C. V. c. C. V.	30			24	14.4	12.55	58	13.53	3	*S	39	6.53	7. 5	14.39			35.2	35.4	35.3	34.7	Very ſteady.	22	Calm and cloudy: a great long ſwell from Southward. A.M. a very large ſhark was ſtruck with the harpon; but it broke the harpon, and got off. Many fiſh and birds. T.S. 82°.
13	$81\frac{1}{2}$	$30.0\frac{1}{2}$	$30.0\frac{1}{2}$	$81\frac{1}{2}$	$30.0\frac{1}{2}$	c. WNW. NW. c. L. to weſtw. NW. c. NW. NW by N. c. th. L. to SW. V. NNE.				2	13.50	12.39	115	14.34	3	*S	15	6.25	6.37	14.27	13.18		32.4 32.0	33.4 32.6	32.6	32.1	Tightened the ſcrews.	31	Light airs and cloudy through. Many fiſh and birds. P.M. no ground 70 fath. A.M. hooked a ſhark. T.S. 81°½.
																												2882	

1775	Th.	Marine Barometer R.	S.	Th.	D.	Weather and Winds		D.L. A.O. N	S	D.L. A.T.K. W	E	Longitude from Greenwich A.	TK.	D.☽ T.K.	☽	S.	O.	E.D.	Latitude A.	O.	Cor. Long.	Magnetical Variation Az.	Amp.	Dip Face E.	W.	MD.	C.D.		M.L.	
	°	In.Dec.	In.Dec.	°	In.Dec.			′	′	′	′	° ′ W	° ′ W	′	° ′ W			′	° ′ N	° ′ N	° ′ W	° ′ W	′ ° W	°	°	°	°		3210	
June 23	82	30.0	29.9¾	81¾	30.0	r. c. c. sq. r. c. c. h.	SSE. S. S by W. S by W. S. S by E. S by E. SSE. S. S by W. S.	12		1		11.13	9.16						3. 0	4.32	11. 6			28.	28.4	28.2	27.4	Very unsteady.	54	Moderate breezes. T.S. 81°.
24	80½	30.0	29.9¾	80	29.9¾	h. sq. r. sq. r. c. c. sq. c. c. sq. r.	S. S by W. SSW. S by W. S by W. S by W. S.	3		3		11.42	9.48						2.53	4.28	11.37			28.4	29.4	29.	28.2		73	Most part a fresh gale from Southward, with squalls and rain T.S. 81°.
25	77¼	30.0½	30.0½	76½	30.0	h. c. c. c. c. r.	S. S by W. SSW. SSW. SSW. SSW. V. C. W by S.					11.16							2.35	[4.21]	[11.11]			27.4	27.6	27.5	26.7	Pretty steady.	58	Most part fresh breezes. At 8 P.M. Richard Jones, the surgeon's lad, fell over-board. Hove the ship to, and sent jolly boat and yawl in search of him; but they could not find him. No observation for latitude or time, not having one glimpse of the Sun all the forenoon. P.M. no ground 50 fath. T.S. 80°½.
26	80	30.0½	30.0¼	80½	30.0½	hr. c. sq. sq. r. r. r. c.	V. SSW. SW by S. SW by S. S by W. SSW. S by W.	21			52	10. 2	7.40						2.23	4.16	9.26			26.4	27.4	27.	26.2		58	A fresh breeze with much rain. P.M. no ground 70 fath. T.S. 80°½.
27	80¼	30.0½	30.0½	80¼	30.0½	c. sq. c. sq. sq. c. c.	SSW. SW by S. SSW. SSW. S by W. S by W. S. S. S by W. S.		24		44	10. 6	7. 0						2.13	3.42	8.45			25.4	26.	25.6	25.		73	Mostly fresh breezes. At 2 P.M. from mast-head *land*, making like islands, was seen bearing E by N=ENE¾N, supposed Cape Palmas. P.M. no ground 70 fath. At 8 P.M. spoke with a French ship from *Havre* to the coast of *Angola*. T.S. 80°.
28	80½	30.0¾	30.0¼	80¼	30.0¾	h. h. sq. f. c. sq. c. c. f.	S. S by E. S by E. S. S. S by E.		13		2	11.10	8. 2						1.34	2.50	9.45	14.42* *14.11		24.4	25.2	24.7	24.		76	Fresh breeze throughout, and a large Southerly swell. T.S. 80°¼.
29	79½	30.1	30.1	79	30.1	c. c. c. f. f.	S. S by E. S. S by E. S by E. S. S by E.		20	39		12.18	9.48						1. 0	1.56	11.29	14.37* *13.44	14.25*	23.6	24.4	24.1	23.2	Very unsteady.	79	Fresh breeze throughout. P.M. cloudy. A.M. fair. T.S. 79°½.
30	78½	30.2	30.1½	78½	30.1	f. f. c. c. f. f.	S by E. S by E. S by E. S by E. S.	1		77		13.21	12.8						0.14	1.11	13.48	13. 6* *12. 7		23.6	24.4	24.1	23.2	Pretty steady.	78	Fresh breezes with fair weather. P.M. many porpoises. Some bottle noses, and vast flocks of flying fish. A.M. a man of war bird. T.S. 76°¾.
July 1	77½	30.1½	30.1½	76¾	30.1	c. c. c. c. f.	S. S. S by E. S. S. S by E. S.	21		76		12.35	12.38						0.16	1.34	14.17	13.17*		24.6	24.4	24.5	23.6		66	Major part moderate and cloudy. T.S. 78°.
2	78½	30.1¼	30.1	78	30.1	h. h. c. c. c.	S by E. S. S by E. S by E. SSE. S by E. S. S. S by E. S.	14		62		13.33	14.38						S 0.22	1.10	16.16	12.41*	12.47*	24.6	25.4	25.1	24.2	N.B. going to *Westward* encreases dip.	70	Pleasant breeze, with fair weather and smooth water. T.S. 76°.
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1775	Th.	Marine Barometer,				Weather and Winds		D.L. A.O.		D.L. A.T.K		Longitude from Greenwich				S.	O.	E.D.	Latitude		Cor. Long.	Magnetical Variation		Magnetical Dip Face					ML.	
		R.	S.	Th.	D.			N	S	W	E	A.	TK.	D.☽ T.K.	☽				A.	O.		Az.	Amp.	E.	W	MD.	C.D.			
		In.Dec.	In.Dec.	°	In.Dec			′	′	′	′	° ′	° ′	′	° ′				° ′	° ′	° ′	° ′	° ′	°	°	°	°		4942	
												W	W	E	W				S.	S.	W	W	W							
July 13	76	30.2¾	30.2½	76	30.2½	h. ſq. r. ſq. r. c. c. c.	SE by E. SE by S. SE by E. E by S. SE. SE. SE by S. SSE. SE by S. S by E. SSE.	2		6		23.30	25.47						16.17	15.33	27.37	*2.43	*2.42	3.4	3.4	3.4	3.4		111	First part, freſh gales and ſqually. Latter, moderate and fair. A.M. Fog Bank exactly like an iſland, the outline perfectly well defined, except the extremities: I never before ſaw any cloud ſo like land. T.S. 74°. Changed the poles of the dipping-needle. Mean of the various trials changed and re-changed 3½ S. viz. E. W. MD. Changed the poles, marked end down, 4.4 4. 4.2 Re-changed, ditto up, 2.6 3. 2.7 Ditto, ditto down, 4.4 4.6 4.5 Ditto, ditto up, 2.4 2.4 2.4 Mean 3.4 3.4 3.4
14	76	30.3¼	30.3	76	30.2½	f. ſq. ſq. f. c. ſq. c. c. f.	SE by S. SE. SE by E. SE by S. SE by S. SE. SE. SE by E. SE.	3		9		24.27	26.53						17.46	16.59	28.45	1.48* *1.45	1.36* *1.49	5.2	5.4	5.3	6.2	Not very ſteady.	104	Conſtant trade wind. T.S. 74°.
15	76¼	30.2½	30.2½	76½	30.2¼	f. c. ſq. c. r. c. ſq. r. ſq. r. c. c. h.	SE. SE by S. SE. SE by E. SE by S. SSE V. SE. SE by E.				1	25.20	27.45						18.58	18.11	29.39	*1.21		7.4	8.	7.6	8.5		88	First part, moderate. Middle, hard ſqualls and rain. T.S. 74°½.
16	74½	30.3½	30.3	74½	30.2¾	c. c. c. c.	SE. SE by E. SE by E. ESE. E by S. SE by E. E by S SE by E. SE. ESE.		1	1		25.42	28. 8						19.47	19. 1	30. 4	0.42*		9.2	9.6	9.4	10.3	Pretty ſteady.	54	A.M ſaw a bird like a booby, but ſhorter winged and necked, called by ſailors, poor John. Light breezes, ſwell much abated. T.S. 74°.
17	73	30.4	30.3¾	73	30 3½	c. c. fr. ſq. ſq. fr. ſq. fr. c. ſq.	SE by E. V. SSE. V. ESE. SE by E. V. SE. SE by E. V. SE. E by S. ESE.	2		5		26. 2	28.33	105	30.18	3	*At.	31	20.47	19.59	30 31			11.	11.2	11.1	12.		69	P.M. to the NE. the clouds very remarkable, being in white curved rays, like the variation chart. At 7 A.M. ſaw Trinidada from maſt-head SSE. At 9 viſible from deck. At noon, Trinidada S 36° E 11 leagues. Major part, moderate breezes and cloudy weather; winds variable and ſmooth water. By the bearings of Trinidada at noon, allowing it to lye in 20° 27′ S according to Dr. HALLEY. Its longitude per T. K. 28°.13′ W 28°.13′ Mean diff. T.K. and ☽ 8 obſ. ⊙ and 1 * 1.58 8 obſ. ⊙ 2.12 Long. 8 obſ. ⊙ and ☽ 1 obſ. * At. 30.11 8 obſ. ⊙ & ☽ 30.25 By Dr. HALLEY, 29.50 29.50 Diff. 0.21 0.35 T.S. 74°.
18	75½	30.4	30.3½	75½	30.3½	f. f. c. c. c. f.	SE by E. SE. SE by E. SE by E. SE. SE. E by S. ESE. E by S.		8	4		26.29	29. 4	138 120	31.22 31. 4	3 3	⊙ ⊙	11 23	22. 9	21.29	31.15	E 0.20*	E 0.13*	13.	13.	13.	13.7	Very unſteady.	87	At Sun-ſet, Trinidada E by S ½ S 8 or 9 leagues. T.S 76°.
19	73½	30.4	30.3	73½	30.3	c. ſq. c. fr. ſq. r. ſq. r. f. ſq. r. ſq. r. c. f.	ENE. V. ESE. V. C. NE. V. C. N. V. N.					26.12	28.47	145	31.12	3	⊙	15	22.48	22. 8	30.59		0.44*	14.4	14.4	14.4	15.3	Pretty ſteady.	47	Moſtly light breezes and cloudy. T.S. 74°.
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1775	Th.	Marine Barometer R.	Marine Barometer S.	Marine Barometer Th.	Marine Barometer D.	Weather and Winds.		D.L. A.O. N	D.L. A.O. S	D.L. A.T.K. W	D.L. A.T.K. E	Longitude from Greenwich A.	Longitude from Greenwich TK.	D.☽ T.K.	☽	S.	O.	E D.	Latitude A.	Latitude O.	Cor. Long.	Magnetical Variation Az.	Magnetical Variation Amp.	South Dip Face E.	South Dip Face W.	MD.	C.D.		M L.	
	°	In.Dec.	In.Dec	°	In.Dec.			'	'	'	'	° '	° '	'	° '			'	° '	° '	° '	° '	' °	°	°	°	°		6084	
July 30	65¼	30.2¼	30.1¾	65¼	30.1½	c. ſq, r. c. ſq. r. ſq, r. c. c. c. ſq.	NW by W. NW by W. NW by N. NW by N. NW by N.		7		5	W 22.36	W 25. 6						S 32. 3	S 32. 8	W 27.10	E 2.26* *1.27	E *1.42	31.3	32.3	31.7	32.5	Not ſteady.	152	An increaſing gale from NW quarter, with frequent ſqualls. 9 P.M. bar. 30.1¾. 8 A.M. 30.2¼. T.S. 65°½.
31	62		29.8	62		l. c. ſq. r. ſq, r. ſq. r. hſq. hſq, r. ſq, hr. hſq.	NW by N. NW by N. NWbyN. NNW. NbyW. N.	5		26		19.14							33.14	[33.19]	[24. 3]	0.44*						No obſ. too much motion.	184	Strong gales, with hard ſqualls and rain. 9 P.M. bar. 30.1½. 12. 30.1. 8 A.M. 29.9½. 10. 29.9. 11½. 29.8½.
Aug. 1	61		29.8½	61¼		ſq, r, L. ſq, r, L. ſq, r. ſq. c. c.	NNW. NNW. NNW. NW. WNW. WNW. W by N.					16.28	19.24						33.35	33.35	21.22			34.	35.	34.4	35.2	Very unſteady.	141	Firſt part, blowing very freſh from N.W. Latter, more moderate, with very large ſea. Bar. 1½. 29.7½. 2½. 29.7. 4. 29.7½. 6. 29.8. 8. 29.8. 10. 29.8. A.M. 7. 29.8.
2	58½	29.9	29.8¾	58½	29.8½	h. h. c. ſq, r. L. ſq. c.	W. W. V. W by S. W by S. WSW. WSW. V. W by S.	2			16	14.53	17.33						34. 9	34. 7	19.28	W *2.55		33.4	34.4	34.	34.6	Unſteady.	86	Major part, light breezes, with great ſwell.
3	55½	29.3	29.2½	55½	29.2¼	c. ſq, r. L. to c.n.e. h. ſq. ſq. L, r. c. L, r. c. L, r. ſr.	SW. V. W. NE by N. ENE. V. NEbyE. N. WNW. V. W.		1		5	13.56	16.29						34.26	34.25	18.21		W 3.18*	34.4	35.4	35.	35.6	Not very unſteady.	55	Winds variable, with ſqually weather and confuſed ſea. bar. 3 P.M. 29.8 6. 29.7¾. 10. 29.7. 8 A.M. 29.3 9 A.M. 29.2¾. About noon, it appeared very threatning to the Northward, but the clouds diſſipated.
4	54¾		29.8	54¾		ſq. c. r. ſq, r. hard gales. ditto.	W by N. V. W by S. V. S by E. S by E. S by E. SSW.	2		12		11.45	14.30						33.37	33.34	16.19							No obſ. too much ſea.	129	Firſt part, ſqually with ſome rain. The middle, exceſſive hard gales, with a great ſea; and the ſpray ſo thick that the ſhip was covered with it. 2 P.M. bar. 29.7½. 4. 29.2. 6. 29.2½. 9. 29.5. 7 A.M. 29.7½. 9. 29.7½. 11. 29.8.
5	51¾		30.0½	50¾		h. ſq, r. ſq, r. ſq, hail. ſq, hail. ſq, r. hail. ſq. ſq. ſq, hail. hſq, hail.	SSW. SW by S. SW by S. V. SSW. S by W.	11			35	9.24	11.34						33.37	33.23	13.20					35.	35.6	Very unſteady.	117	Squally weather, with much hail and rain, and a large ſwell. Bar. 4 P.M. 29.8½. 6. 29.9. 8. 30.0. 7 A.M. 30.0¼. 9 A.M. 30.0¾.
6	53		30.2¾	52		ſq, r. ſq, hail. ſq, r. hail. ſq, r. c. c. ſq. ſq, r. ſq, r. ſq. ſr.	SSW. S by W. S by W. S. S. S. S by E.	14			2	7.19	9.27						33.37	33. 9	11.10								103	Firſt and middle, freſh gales and ſqually. Latter, moderate; great ſwell. A.M. ſome ſilver birds. Bar. 2 P.M. 30.0¼. 5. 30.1. 10. 30.2. 7 A.M. 30.2. 9. 30.3.
7	56¾		30.0½	56		c. c. f. h. h. c. ſq, r. ſq, r.	S by E. C. NNE. C. NNE. NE by N. V. NNE. NNE. NE by N.		25		6	6. 5							33 37	[33.21]	[9.53]								62	Firſt, light airs, with cloudy weather. Latter, freſh gales, with rain. P.M. Venus appeared very dim and fuzzy; a rim round the Moon. 9 A.M. bar. 30.1½. 11. 30.0½.
8	60¼	30.1	30.0	60	30.0	ſq, r. ſq, r. c. ſq, r. ſq, r. ſq, r. f.	NNE. NNE. NNE. V. N by E. N. V. NNW. V.					3.55	5.57						33.51	33.48	7.37								109	Firſt and middle, hard gales, ſqually with rain. Latter, moderate and hazy; great ſwell. A.M. ſome ſilver birds, a pintado bird. Bar. 2 P.M. 29.9½. 8 A.M. 29.9½. 9. 29.9¾. 10 {30.0. 30.0¼.
																													7222	

1775	Th.	Marine Barometer R.	Marine Barometer S.	Marine Barometer Th.	Marine Barometer D.	Weather and Winds.	D.L. A.O. N	D.L. A.O. S	D.L. A.T.K. W	D.L. A.T.K. E	Longitude from Greenwich A.	Longitude from Greenwich T.K.	D. of T.K.	☾	S.	O.	E.D.	Latitude A.	Latitude O.	Cor. Long.	Magnetical Variation Az.	Magnetical Variation Amp.	Magnetical South Dip Face E.	Magnetical South Dip Face W.	MD.	C.D.		ML.	
	°	In.Dec.	In.Dec.	°	In.Dec.		'	'	'	'	° ' E	° ' E	'	° ' E				° ' S	° ' S	° ' E	° ' W	° ' W	°	°	°	°		S365	
Aug. 19	60¼	30.1	30.0	60½	30.0	h. N by E. WNW. NW by N. h. dr. NW by N. WNW. dr. fr. WNW. V. fr. V.	7			8	19.49	19. 9						34.19	34.[illegible]	18. 6			45.2	46.6	46.	46.5	Very steady.	44	Most part, light breezes and hazy. At 2 P.M. saw the land from mast-head ESE. per compass. At 5½ h. { Peaked hum of Cape Good Hope, S 52.48 E / Hum. mistaken for Cape, S 57. E / L. ext. Table Land, E 4. N / L. ext. in sight, E 19. N At Sun-rise { Sugar Loaf, N 69°. E / Table Land, N 73. E / Cape Good Hope, S 60. E At noon { L. ext. N 55. E / Cape Good Hope, S 61. E Longitude per T.K. 16. 9 E Cape Good Hope per chart, [illegible] E Longitude of Cape per T.K. 19.[illegible] True longitude of Cape, [illegible] Error of T.K. [illegible]
20	61	30.0½	30.0½	60½	29.9½	h. NNW. h. NNW. N. V. NE by N. h. N by E. N. NNE. h. N by E. NE.													34.15			22.31*	45.6	45.6	45.6	46.3	Perfectly steady.	70	Major part, moderate and hazy. In False Bay. At 4 h. Cape Good Hope, E 16°. S. Sunset * L. ext. N 21°. E. E 2.18 Bluff Point in one with Hummock. 7?.50 Cape Good Hope * 2d. 86. 6 Hanglip. * 2d Cape Good Hope. L 24.30 Hummock, called Cape last night. 7. 0 Hummock. 1.5? Outer Hummock and highest part dist. Ridge. E 13.48 Hanglip. Bellows, E 15°. S. Sunrise, Hanglip, S 47°. E. Cape Good Hope, S 36°. W. White Sand Hills in Symmon's Bay, N 133°. W. At noon { Hanglip Point, S 35°. E / Cape Good Hope, S 50. W / Noah's Ark, NW about 4' dist. N. B. All these bearings are by compass, without allowing for variation.
21																							45.2	46.4	45.7	46.4	Very steady.	S479	At 4 P.M. being calm, hoisted out the pinnace, and towed in towards the harbour. At 7 anchored in 20 fath. Noah's Ark W. Roman Rock NW ½ W dist. about two miles.

1775	Th.	Marine Barometer R.	Marine Barometer S.	Marine Barometer Th.	Marine Barometer D.	Weather and Winds		D.L. A.O. N	D.L. A.O. S	D.L. A.T.K. W	D.L. A.T.K. E	Longitude from Greenwich A.	Longitude from Greenwich TK.	Longitude from Greenwich D.☽ T.K.	Longitude from Greenwich ☽	S.	O.	E.D.	Latitude A.	Latitude O.	Cor. Long.	Magnetical Variation Az.	Magnetical Variation Amp.	Magnetical South Dip Face E.	Magnetical South Dip Face W.	Magnetical South Dip MD.	Magnetical South Dip C.D.		M L.	*Cape* of Good Hope lat. 34°.22' S. long. 18.27 E.
	°	In.Dec.	In.Dec.	°	In.Dec.			'	'	'	'	° ' E	° ' E	'	° '			'	° ' S	° ' S	° ' E	° ' W	' ° W	°	°	°	°			
Sept. 19	59		30.1	59		f. c. f. f. c.	NW. NW by W. NW by W. WNW. C. C. E. C. WNW. NW.					18.43	18.52							34.48	18.52								17	At Sun-set, Cape Good Hope NW=WNW. Table Land N by W=NW by N. Hanglip E 7 N=E 29½ N. Eastern ext. in sight E 5° S=E 17½ N distance 6 leagues from nearest land. At Sun-rise, Cape Good Hope N by W½W=NW½N 9 or 10 leagues. Hanglip Point NE=NNE. Ext. to E[d], ESE=E. Saw a sail, supposed the Gatton, come round from Table Bay, to join us. At noon, Cape Good Hope N per comp.=N 22½ W. Hanglip N 35 E=N 12½ E 9 leagues. Eastern ext. N 80 E=N 57½ E. Major part, light breezes and fair weather, with great swell from SW.
20	59¼		30.0¾	59		c. c. sq. sq. c. c. sq. sq, r.	NW. NW by N. NW by N. N by W. N by W. NNW. NNW.	4				21.26							35.59	35.55	[21.15]								154	Fresh gale, with a very large sea. Bar. 9 P.M. 30.1. 8 A.M. 30.1.
21	57½		30.1½	57¾		c. c. sq. c. c. c. sq. sq, r.	NW by W. WNW. W by S. W by S. W. W. W by S. W by S.	8				24.22							36.21	36. 9	[24. 1]								144	Fresh gales and great swell. Bar. 8 P.M. 30.1. 8 A.M. 30.1.
22	62		30.2½	62		sq. c. f. f. c. c. f.	W by S. SW. S by W. S. S. C. V. NE. NE by E. ENE.	10		49		25.36							36.37	36.15	[25. 5]								66	First part, decreasing gale. Middle, calm. Latter part, a breeze from the NE. Great swell. A.M. bar. 30.3. N.B. it was not observed at noon. Past 1 P.M. it was 30.2½ Many alcatrasses, pettrils, pintado birds, &c.
23	66½		29.6¾	66		f. f. f. f.	NE by E. ENE. NE by E. NE by E. ENE. NE by E.	6				27. 8							37.34	37. 6	[26.27]								92	First part, fresh gales. Latter, hard gales, with great and confused sea, Mostly fair weather. 2 P.M. bar. 30.2½ 9. 30.1. 11. 30.0. 8½ A.M. 29.8. Many pettrils, pintado birds, &c.
24	65		29.5½	65		h. h. h. c. c.	NE by E. NE by E. NE by N. V. V. W. W. WNW. NW by W.		9			28.13	27.23						37.58	37.39	27.23								58	First part, hard gales from N[d]. Latter, from W[d]. with a very large NE swell, confused in heaps. Many alcatrasses, pettrils, pintado birds, &c. Bar. 29.6 to 29.5½.
25	58½		29.7	58½		c. c. h. c. r. c. c. r. r. sq.	W by N. V. W by N. W by N. WNW. V. NW. NW. V. WSW.	15				29.35							37.32	36.58	[29. 0]								69	First part, light breezes. Latter part, fresh breezes. A.M. swell much abated. Many alcatrasses, pintado birds, pettrils, &c. Bar. 2 P.M. 29.5¾. 3. 29.6. 12. 29.6. 8 A.M. 29.6½. 9. 29.6¼.
26	61	29.9½	29.9	61	29.8½	sq. sq. sq, sr. sq. c. r. r.	WSW. WSW. SW by W. SW by W. W by S. W by N. W by N. WNW.	21			31	32. 9	31.50						37. 1	36. 6	31.50			52.4	53.4	53.	53 4	Not very steady.	127	Strong gales, mostly W[d], with squalls and some rain. Many alcatrasses, pintado birds, &c. and great many pettrils. Bar. 2 P.M. 29.6½. 4. 29.7. 6. 29.8. 9. 29.8½. 8 A.M. 29.9. 9. 29.9¼. 11. 29.9¼.
27	62½	30.1½	30.1¼	62½	30.0¾	c. c. sq, r. sq, r. h. sq. c.	N by W. N by W. NNW. NNW. W by N. W by N. W. WNW.	1				35.18							36.40	35.24	[34.55]								158 885	First part, fresh gales and squalls. Latter, moderate. A great swell. Many alcatrasses, pintado birds, pettrils, &c. Bar. 4 P.M. 29.8¾. 6. 29.9. 9. 29.9. 12. 29.9. 8 A.M. 30.1. 9. 30.1¼. 11. 30.1¼.

1775	Th.	Marine Barometer. R.	S.	Th	D.	Weather and Winds.		D. L. A. O. N	S	D. L. A. T. K. W	E	Longitude from Greenwich A.	TK.	D. ☽ T.K.	☽	S.	O.	E. D.	Latitude A.	O.	Cor. Long.	Magnetical Variation. Az.	Amp.	South Dip Face E.	W.	MD.	C.D.		ML.	
	°	In.Dec.	In.Dec	°	In.Dec.			′	′	′	′	° ′	° ′	′	° ′			′	° ′	° ′	° ′	° ′	° ′	°	°	°	°		1961	
Oct. 8	61½	30.3½	30.3	61	30.2½	c. c. f. f.	SW. V. C. V. V. W. V. NW by W.		2		23	E 56.32	E 57.55	E	E				S 35.50	S 33.28	E 57.55	W 23.48*	W 24. 2*	61. 61.4	63. 62.	62. 61.6	62.4 62.2	Pretty steady. *Larboard-side of cabbin.* Ditto. *Starboard-side of cabbin.*	21	Mostly light airs and calms, with large swell from SW. Few birds. G var. 3 az. 23° 32′*, amp. 23°50′*. Bar. 6 P.M. 30.3½. 8 A.M. 30.3¼. 10. 30.3¼.
9	63½	30.4¼	30.3¾	63	30.3¾	f. f. f. f. h.	NW by W. WNW. W by N. W by N. WSW. WSW. SW by W. SW by W. V.	13			13	58.18	59.53	4	59.49	5	A *	34	36. 5	33.30	59.53	23.49*	23.57*	61.4	62.2	61.7	62.3	Very steady.	89	Major part, pleasant breezes and fair weather. Just before noon a *grampus* puffed close to the ship on the larboard side, at same time a flock of a vast number of *small white birds* at some distance on starboard quarter. G var. 2 az. 23° 25′*. Bar 2½ P.M. 30.3. 7. 30.2¾. 1 A.M. 30.3¼. 9. 30.3½.
10	61½	30.4½	30.4	61¾	30.3½	h. f. f. f. f.	S by W. S by E. SSE. V. SE by S. S by E. S by E. C. WNW. NW. WNW.	5			1	58.46	60.22	45	59.37	6	A *	28	36. 0	33.20	60.22	21.43*	21.52*	61.4	62.4	62.	62.4	Pretty steady.	24	Mostly light airs, with a large swell from Wd. P.M. a *grampus* very near the ship. Several alcatrasses, &c. G var. az. 22°19′*, amp. 22°23′*.
11	62½	30.3¾	30.2½	62¼	30.2½	f. f. c. c. c. f.	NW by W. NW by W. NW by W. WNW. WNW.	6			27	60.18	62.21						36. 6	33.20	62.21	23.10*	22.47*	61.4	62.4	62.	62.4	Not very steady.	77	Pleasant breezes. Some pintado birds, &c. smooth water, but long swell from SW. G var. az. 22° 30′*, amp. 22° 4′*.
12	64¼	30.3¾	30.3	64	30.2½	f. f. f. h. c. c.	WNW. WNW. WNW. WNW.	4			12	61.39	63.54						36.10	33.20	63.54	21. 5*	20.51*	62.2	63.2	62.6	63.2	Very steady.	68	First part, pleasant breezes. Latter, light airs, with great swell from SW. Some pintado birds, &c. Bar. 2 P.M. 30.2¾. 11½. 30.2¾. 8½ A.M. 30.3.
13	65¼	30.2¼	30.1¼	65	30.1¼	h. h. h. c. c. sq. c.	NW by N. NNE. NNE. N by E. N by E. N by E.		2		10	63.34	65.59						36.14	33.26	65.59	20.27*	20.34*	62.4	63.2	62.7	63.3	Not very steady.	96	First part, light breezes. Latter, moderate. Several pintado birds. A.M. a silver bird and pettril, several poor johns. G var. 3 az. 21° 28′*, amp. 20° 59′*. Bar. 2 P.M. 30.2¾. 10½. 30.2¾. 1 A.M. 30.2¼.
14	66	30.3	30.2½	65½	30.2	h. h. h. h.	N by E. N by E. NNE. NNE. NNE. NE by N.	2			21	65. 6	67.52	W 25	68.17	5	⊙	27	36. 4	33.14*	67.52	19.35*		61.6	62.4	62.1	62.5	Pretty steady.	78	Pleasant breeze. Great dews. G long. ⊙ and ☽ 67° 56′. Bar. 6 P.M. 30.2. 11. 30.2¼. 12. 30.2½. 8 A.M. 30.2¼.
15	64¼	30.0	29.9½	63½	29.8½	f. f. sq. sq. h. h. sq.	NNE. NE. NE. NE. NE by N. NNE. NNE.		24		24	67.26							35.55	[33.17]	[70.24]	18.43*							117	First part, moderate. Latter, fresh gales and squally. Very dirty weather. G var. 2 az. 19° 23′*, amp. 19° 43′. Bar. 6 P.M. 30.2. 12. 30.1¼. 9 A.M. 29.9½.
16	59	30.3	30.2½	59	30.2	sq. f. r. h. h. h. c. c.	NNE. V. N. N. NNW. NW. NW. W by S. SW by S. SW by S. S by W.					69.34	72.44	E 35	72. 9	2	⊙	19	35. 4	32.38	72.44			61.4	62.2	61.7	62.3	Not steady.	120	Fresh gales, with squally weather. A.M. many pintado birds, some alcatrasses, &c. Bar. 2 P.M. 29.8½. 4. 29.8¼. 6. 29.7¾. 10. 29.8½. 12. 29.9. 7½ A.M. 30.1½. 9. 30.2. 10. 30.2.
17	62¾	30.4½	30.4¼	62¼	30.2½	c. c. c. c.	SSE. SSE. SE by S. SE by S. ESE. V. V. C.	11		5		70.30	73.35						34.57	31.41	73.35								66 2717	First part, moderate. Latter, light airs inclinable to calm, with a great swell. Several pintado birds, some alcatrasses, &c. At night, water luminous like blubbers. Bar. 2 P.M. 30.2½. 4½. 30.3. 6. 30.3. 10. 30.3½. 12. 30.4. 8 A.M. 30.4. 9. 30.4½. 10. 30.4¼.

1775	Th.	Marine Barometer R.	Marine Barometer S.	Marine Barometer Th.	Marine Barometer D.	Weather and Winds.		D.L. A.O. N	D.L. A.O. S	D L. A. T.K. W	D L. A. T.K. E	Longitude from Greenwich A.	Longitude from Greenwich T.K.	Longitude from Greenwich D. ☾ T.K.	Longitude from Greenwich ☾	S.	O.	E. D.	Latitude A.	Latitude O.	Cor. Long.	Magnetical Variation Az.	Magnetical Variation Amp.	Magnetical South Dip Face E.	Magnetical South Dip Face W.	Magnetical South Dip MD.	Magnetical South Dip C.D.		ML.	
	°	In.Dec.	In.Dec.	°	In.Dec.			′	′	′	′	° ′ E	° ′ E	′ E	° ′ E				° ′ S	° ′ S	° ′ E	° ′ W	° ′ W	°	°	°	°		3443	
Oct. 28	72	30.2½	30.2	72	30.1½	sq. sq, r. sq. sq. c. sq. sq. h.	E. E by S. E by S. ESE. E by S. E by S. ESE.	16			8	76. 9	79.10						24.17	19.54	79. 4								96	Mostly fresh gales from Eastward, with squalls and rain and a large swell. Several birds as before. A.M. many flying fish. Bar. very unsteady, 1 P.M. 30.2½. 4. 30.1½. 5¼. 30.2. 6. 30.2. 8. 30.2¼. 10. 30.2½. 12. 30.2½. 9 A.M. 30.2½.
29	73	30.2½	30.2	73¾	30.1½	c. c. c. sq. h. h. f.	ESE. E by S. ESE. E by S. ESE. E by S. E by S. ESE. E by S.		2	8		76.52	79.45	35 56	79.10 78.49	4 4	☉ ☉	27 10	22.54	18.33	79.33								93	Fresh gales, with some squalls. Several birds as before. P.M. a large white bird which flapped its wings and flew high, unlike the birds which are found in the main sea. G long. ☉ and ☾ 77° 39′ E. Bar. 2 P.M. 30.1¾. 4. 30.1½. 6. 30.1½. 10. 30.2. 11½. 30.2. 8 A.M. 30.2¼. 9. 30.2¼. 10. 30.2½.
30	75	30.2	30.1½	74½	30.1	f. sq. sq. c. c. c.	ESE. E by S. ESE. E by S. E by S. E by S.	5		15		77.39	80.17						21.11	16.45	79.59		4. 4*	46.2	47.2	46.6	47.3	Unsteady.	113	Strong gales, with frequent squalls. Some birds as before. Vast flocks of flying fish. Swell abated. Bar. 2 P.M. 30.2. 3. 30.1¾. 4. 30.1½. 10½. 30.2. 12½. 30.2. 8 A.M. 30.1½. 9. 30.1½.
31	77	30.1	30.0½	77	30.0	h. h. sq. sq, r. sq. sq.	ESE. E by S. E by S. E by S. E. E. E by N.		3		3	78.10	80.51						19. 8	14.45	80.27	*3.17		44.	45.	44.4	45.1	Ditto.	127	Squally. Few birds as before. Swell increased and weather unsettled. G long. ☉ and ☽ 79° 8′ E. Bar. 2 P.M. 30.1. 4. 30.1. 6. 30.0¾. 10. 30.1. 12½. 30.1. 6 A.M. 30.0½. 8. 30.0¾. 9. 30.0¾. 10½. 30.0½.
Nov. 1	78	30.1	30.0¼	78½	30.0	h. sq, r. sq, r. h. h. sq, hr. sq. c.	E. VE. E. E. VEy. VEy.	1			1	78.17	80.59						17.15	12.51	80.29			41.	42.	41.4	42.1	Very steady.	114	Squally. P.M. a tropic bird, which resembles a paroquet in shape, but is of a whitish grey. Swell much abated. G long. ☉ and ☾ 79° 10′ E. Bar. 2 P.M. 29.9¾. 4. 30.0. 6. 30.0. 9½. 30.0¼. 12½. 30.0. 9 A.M. 30.0¼.
2	81	30.0¾	30.0¼	81	30.0	h. h. h. h. f.	E by N. E by S. E by N. V. E. E by N. E by N. E.			10		78.26	80.58	114 108	79. 4 79.10	8 6	☉ ☉	42 30	15.44	11.20	80.22	1.20* *1.12	*1.42	38.6	39.4	39.1	39.7	Unsteady.	92	Moderate breezes, with some squalls. A.M. few birds; the grey white under wings; frequently overcast. A.M. a little more swell from Northward. G var. 2° 3′*, amp. 2° 4′*. Bar. 2½ P.M. 29.9¾. 6. 30.0. 12½. 30.0¼. 8 A.M. 30.0¾. 9½. 30.0¼. 11½. 30.0¼.
3	81	30.0½	30.0	80¾	29.9½	f. c. c. c. f. f.	E. E. E. E.	5		9		79. 5	81.28						13.47	9.18	80.46	0.59*		35 4	36.4	36.	36.6	Not steady.	123	Pleasant gale, with fair weather. A.M. a bird, larger than a tropic bird, with black wings and white body at a distance, it flapped the wings very much. A head swell. G var. az. 1° 0′*, 0° 52′*. Bar. 2 P.M. 30.0¼. 6. 30.0¼. 7½ A.M. 30.0½.
4	82	30.0½	29.9¾	82	29.9¼	f. c. c. f. f. f.	E. E by S. E. E. E. E.	5		13		79.36	81.46	82 41	80.24 81. 5	6 6	*At.* *A*	17 28	11.57	7.23	80.58	0. 3*		32.6	33.6	33.2	34.		114	Major part moderate. P.M. some *Southern lights*, very rare and motionless. Bar. 2¼ P.M. 29.9½. 6. 29.9¾. 8. 30.0.
5	82	30.0	29.9½	81½	29.9	h. h. h. f. f.	E. E. E. V. E. E.	7		18		80.19	82.11						10. 5	5.26	81.17	*0. 8		29.2	29.6	29.4	30 3	Extremely steady.	119 4434	Fresh gales, with fair weather. P.M. a large bird of the booby kind above the ship; it had a long neck, and was something in shape and size like a wild goose. At night, *Southern lights*, some diverging in rays downwards, some like a rare cloud, very little motion; a faint ring round ☾. A.M. four tropic birds. G var. 4 az. 0° 21′ *E. az. *0° 4′ E. Bar. 2½ P.M. 29.9½. 6. 29.9¼. 1 A.M. 29.9. 8. 29.9½.

1775	Th.	Marine Barometer. R.	Marine Barometer. S.	Marine Barometer. Th.	Marine Barometer. D.	Weather and Winds.	D.L. A O. N	D.L. A O. S	D.L. A. T.K. W	D.L. A. T.K. E	Longitude from Greenwich A.	Longitude from Greenwich TK.	Longitude from Greenwich D.☽ T.K.	Longitude from Greenwich ☽	S.	O.	E. D.	Latitude A.	Latitude O.	Cor. Long.	Magnetical Variation Az.	Magnetical Variation Amp.	Magnetical South Dip Face E.	Magnetical South Dip Face W	Magnetical South Dip MD.	Magnetical South Dip C.D.		ML.	
	°	In.Dec.	In.Dec.	°	In.Dec		′	′	′	′	° ′ E	° ′ E	′ E	° ′ E				° ′ S	° ′ N	° ′ E	° ′ W	° ′ W	°	°	°	°		5014	
Nov. 15	81¾	29.9½	29.9	81¾	29.8½	c. sq, r. sq, hr. V. c. sq. r. V. W by N. W by S. r. sq, r. c. W by S. SSE. SSW. c. SW.		19	2		86. 9	83.46						0.57	2.50	81.52			14.4	14.6	14.5	15.4		71	Mostly variable wind and rain. P.M. a sail; shewed French colours on firing a gun to speak with her, but did not shorten sail. Several birds and many fish. Bar. 2 P.M. 29.8½. 6. 29.9. 12. 29.9¼. 8 A.M. 29.9¼.
16	81	30.0	29.9¼	80½	29.9	c. sq, hr. SW. S. hr. V. S by W. hr. c. S by W. SW. c. r. c. SW.		9		6	87. 5	84.48	76	83.32	4	⊙	18	0.46	2.52	82.48			13.4	14.	13.6	14.5	Very steady.	57	First part, mostly squally with very heavy rain. Middle and latter, light airs and cloudy with some rain. ⊙'s limb not very distinct when *long.* observed. Bar. 2 P.M. 29.8¾. 4. 29.8¾. 10. 29.9½. 12. 29.9½. 8 A.M. 29.9½. 9. 30.0.
17	80	29.9¾	29.9	80	29.8¾	sq, hr. c. sq. S. V. c. hr. c. sq, hr. S by W. SSE. SE. sq, hr. V. S by E. r. c. S by E. C.	21			18	88. 2	86. 3						0.32	3.27	83.57			14.4	15.2	14.7	15.6		59	Very unsettled weather with rain. Bar. 1 P.M. 29.9½. 2. 29.9. 4. 29.9. 9½. 30.0. 1 A.M. 29.9¼. 9. 29.9¾. *At first the dipping-needle had no motion,* but with the face East, was at 12°4⁄8; when moved a little, *it remained not right*: when moved with the face to West, *the needle went round, and stood any where.* Moved several times, it pointed with face E 14⅝ W 15⅜ Very steady.
18	83½	30.0	29.9¼	83½	29.9	hr. c. C. E. E by S. c. r. E by S. r. c. E. E by N. NE by E. c. sq. NE by E. NE by N. N by E.	6			9	88 16	86.26						0. 3	4. 2	84.13			11.1	11.5	11.3	12.2	Pretty steady.	38	Mostly light breezes. P.M. saw two dolphins, heard a tropic bird in night. A.M. caught two dolphins. *Dipping-needle pretty steady, and no appearance of defect any where.* Bar. 2 P.M. 29.8¾. 4. 29.8¾. 12½ A.M. 29.9. 8. 29.9½. 11. 29.9½.
19	83	29.9¾	29.9	83	29.8¾	c. sq, r. sq, th, L, hr. NNW. NW. V. sq. th, L, hr. V. C. V. C. r. c. V. N. c. r. c. N. N by E. NE by N.		18		19	88.57	87.26						0. 3	3.44	85. 7			10.6	11.2	11.	11.7		44	Variable winds, with rain. P.M. a swallow with short wings and white breast. A.M. a small land bird shaped like a water wag-tail, but in colour like a linnet, with a few yellowish feathers in the flank. A.M. a bird at distance like a gannet. Bar. 2 P.M. 29.8½. 4. 29.8½. 6. 29.8½. 1 A.M. 29.9½. 8. 29.9¼.
20	82½	30.0	29.9½	82¼	29.9	sq, fr. sq. NE by N. NE. sq. sq, L, hr. hr. NE by E. E by N. NE by E. ENE. NE. hr. sq. V. SE by E. SE. SSE. c. sq, flying sh. SSE. S by W.		30		13	90.27	89. 9						0.17	3. 0	86.40			11.4	12.2	11.7	12.6	Not very steady.	102	Fresh breezes, with some squalls and a great deal of rain. The land bird aboard still. P.M. heard a bird, supposed to be a booby. A.M. passed a drift; a grey sea bird shaped like a plover. A black sea bird at a distance. Bar. 1 P.M. 29.8¾. 6. 29.8½. 1 A.M. 29.9. 7½. 29.9½. 9. 29.9½. 10. 29.9¾. 11. 29.9¾. 11¼. 29.9¾.
21	80¾	30.0¼	29.9¾	80½	29.9¼	r. SW. V. r, L. to nw. V. C. V. h. V. C. h. r. h. C. V. W. V. N.	15			27	90.54	92. 3						0. 3	3.29	87.28			10.2	10.6	10.4	11.3	Very steady.	30	Variable, with rain and calms. The land bird aboard still. A.M. saw a tropic bird. Passed a drift. Light airs. Bar. ½ P.M. 29.9½. 2. 29.9. 4. 29.9¼. 6. 29.9½. 7. 29.9½. 12½ A.M. 29.9¾. 8. 29.9¾. 10. 30.0.
22	81¾	30.0	29.9½	81¾	29.9	c. sq. N. N by W. sq. hr. c. r. N by W. C. SE by S. V. r. V. r. c. NE by E. C.	14			55	91.17	91.21						N 0. 9	3.55	88.39			10.6	11.2	11.	11.7	Very steady, but ship not having steer-age way.	30	Major part, light breezes, with some rain. The land bird aboard still. P.M. saw a booby, some fish of the porpoise kind. A.M. a horse fly, several riplings, and many fish. Water luminous at night. Bar. 4 P.M. 29.9½. 5. 29.9½. 12½ A.M. 29.9½. 7. 29.9¾. 8½. 30.0. 9. 30.0. 10½. 30.0
23	82		29.9½	82		c. C. E. V. c. V. E by N. c. h. NE by E. NE. h. NE by E. NE. NNE.	6			8	91. 8	91.20						0.45	4.37	88.31			7.6	8.4	8.1	9.	Very steady.	39	First part, calm. Latter, light breeze from NE. with hazy weather. The land bird aboard still. A.M. several tropic birds. Passed a bamboo adrift. Water luminous at night. Bar. 4 P.M. 29.9¼. 6. 29.9. 12½ A.M. 29.9½. 8. 29.9¾. 9. 30.0.
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1775	Th.	Marine Barometer R.	Marine Barometer S.	Marine Barometer Th.	Marine Barometer D.	Weather and Winds.		D L A.O. N	D L A.O. S	D.L. A.T.K. W	D.L. A.T.K. E	Longitude from Greenwich A.	T.K.	D. ☾ T.K.	☾	S.	O.	E.D.	Latitude A.	Latitude O.	Cor. Long.	Magnetical Variation Az.	Magnetical Variation Amp.	South Dip Face E.	South Dip Face W.	M.D.	C.D.		ML.	
	°	In.Dec.	In.Dec.	°	In.Dec.			′	′	′	′	° ′ E	° ′ E	′ E	° ′ E				° ′ S	° ′ S	° ′ E	° ′ E	° ′ E	°	°	°	°		5939	
Nov. 30	84½	30.2½	30.0	85	29.9½	c. c. c. h. h.	NE by E. ENE. ENE. NE by E. NE by E.	11			5	90.15	87.19						7.13	10.55	84 55			4.	4.4	4.2	3.3	Very steady.	79	Moderate breezes, and mostly hazy. Bar. 3 P.M. 29.9½. 5. 29.9¼. 8 A.M. 30.0.
Dec. 1	78		30.0	77½		h. h. h. c. sq, r. sq, r. sq, hr.	NE by E. N. NE by N. NE by N. NNE. NNE. NE. NE. V.		9	6		90.36	87.34						7.24	[11. 1]	85.15			3.6	4.2	4.	3.1	Pretty steady.	61	Variable winds and weather, with much rain. A.M. a small land bird, probably same we left some days ago. Bar. 1 P.M. 30.0. 3. 29.9¾. 4. 29.9¼. 6. 29.9½. 9¾. 30.0¼. 12½ A.M. 29.9¾. 7½. 30.0. 8. 30.0¼. 8½. 30.0½.
2	82½	30.0	29.9½	82	29.9¼	sq, r. r. r. c. c. c.	V. V. NE. N by E. N by E. NE by N. NNE. NE by N.			5		91.20	88.13						7.17	10.50	85.58			2.2	2.6	2.4	1.5	Not steady.	52	First part, squally with rain. Past 6 P.M. weather looked very black and threatening, but the barometer did not fall and it came to nothing. Middle and latter, moderate. A.M. the same land bird. Bar. 2 P.M. 29.9½. 2½. 29.9¾. 4. 29.9. 6. 29.9. 9¾. 30.0. 12½ A.M. 29.9¼. 6. 29.9. 7½. 29.9½. 8¾. 29.9¾. 11½. 29.9¾.
3	83½	30.0	29.9	83¾	29.9	c. c. c. h. h. f.	NE by N. NNE. NE by N. NE. NNE. N by E. N. NNW. N by W.		8		9	92. 4	89. 6	97	87.29	4	*A *F	43	7. 8	10.33	86.49			3.	3.6	3.3	2.4	Not very steady.	47	Decreasing breeze, with pleasant weather. A.M. the same land bird. A small mew about ship. Bar. 2½ P.M. 29.9. 4. 29.9. 7. 29.9¼. 12. 29.9¼. 7 A.M. 29.9¼. 7½. 29.9½.
4	83	30.0	29.9½	83¾	29.9	h. h. c. L. to nw. c. c.	NNW. NW. NW by W. V. V. WNW. WNW. V. SW. V. SSW.		4		3	92.16	89.21	116	87.25	6	*A	12	7.52	11.13	87. 2	1.32*	1.41*	5.4	5 6	5.5	4.6	Very steady.	52	Mostly light airs. A.M. the same land bird. Bar. 2 P.M. 29.8¾. 6. 29.8½. 10. 29.9¼. 12. 29.9¼. 7 A.M. 29.9.
5	84	30.0¼	29.9¾	83¼	29 9¼	h. h. h. f. f.	SSW. SSW. S. S. V. SE by S. SE by S.	2			5	91.58	89. 8						8.39	12. 2	86.47	*1.12	*1.22	6.6	7.2	7.	6.1	Very steady.	50	Light breezes. A.M. the same land bird. P.M. a land bird. It flew like an owl. Bar. 3 P.M. 29.9. 4. 29.9¼. 7. 29.9½. 7 A.M. 29.9¾. 11½. 29.9¾.
6	83	30.0	30.0	83¼	29.9	f. f. sq, r. sq, fr. sq, hr. c. c.	SE. SE by E. SE by E. E. ENE. V. V. C. NE by E. E by N.		9	9		91.16	88.17	137	86. 0	6	*P	28	9.42	12.56	85.44			7.6	8.2	8.	7.1	Pretty steady.	75	First part, moderate breezes. Middle and latter, squally. The same land bird on board. A sand lark on board. Saw a snow standing to N°: it hoisted English colours. Bar. 4 P.M. 29.9½. 7½. 30.0. 7¾, in squall, 29.9¾. 10. 30.0. 12½ A.M. 29.9¾. 12¾, before a squall, 29.9¾. 29.9¾. 8½. 30.0. 9¼. 30.0.
7	80½	30.0¾	30.0¼	80½	29.9¾	c. c. c. c. f.	ENE. NE by E. NE by E. NE. NE by E. NE by E. NE by E.		12	17		89.26	86.10						11. 0	14. 2	83.45			8.6	9.2	9.	8.1	Pretty steady.	134	Fresh gales, with pleasant weather. The same land bird on board. A mottled bird came on board. Bar. 2½ P.M. 29.9¼. 4. 29.9¼. 6. 29.9¼. 10. 30.0. 8 A.M. 30.0¼. 11. 30.0¾.
8	81¼	31.0½	31.0	81½	30.0½	h. c. f. f. f. h.	ENE. ENE. NE. NE. ENE. ENE.		12	3		87. 9	83.50	188 205	80.42 80.15	5 6	*A *A	36 43	11.27	14 17	81.23			9.2	9.6	9.4	8.5	Very steady.	135	Fresh breeze throughout. A small land bird aboard. Bar. 2¾ P.M. 29.9½. 4. 30.0. 6. 30.0. 1 A.M. 30.0. 7. 30.0½. 9. 31.0¼.
																													6624	

In the explanation of *weather* dr. denotes *drizling rain*, and a comma (,) after any *weather* marked, implies that *all* the *weathers*, ſo ſeparated by *comma*, are connected: thus, ſq, r, th, L. imply *ſqually* with *rain*, *thunder* and *lightning*: ſq. r. th, L. imply *ſqually*, *rain*, *thunder* and *lightning*, to have all been in the period of ſix hours, but not at the ſame time.

In the explanation of the 5th column, *for* N denoting that the *obſervation* read N denoting that the *latitude* by *obſervation*.

For	D.L. A.O. N	D.L. A.O. S	D.L. A. T.K. W	D.L. A. T.K. E	Long. from Gr. A.	Long. from Gr. TK.	Latitude A.	Latitude O.	*Read*	D.L. A.O. N	D.L. A.O. S	D.L. A. T.K. W	D.L. A. T.K. E	Long. from Gr. A.	Long. from Gr. T.K.	Latitude A.	Latitude O.
	′	′	′	′	° E ′	° ′	° ′	° ′		′	′	′	′	° ′	° ′	° ′	° ′
May 2d,	—	9	—	—	—	—	—	—	—	9							
4th,	—	31	—	—	—	—	—	—	—	—	23						
8th,	—	39	—	—	—	—	—	—	—	—	19						
9th,	—	11	—	—	—	—	—	—	—	—	10						
10th, add 4′ to Lat. A till 15th incluſive, and	—	—	—	—	—	—	39.26	—	—	—	—	—	—	—	—	39.30	
14th,	—	20	—	—	—	—	—	—	—	—	26						
15th,	—	25	—	—	—	—	—	—	—	—	20						
18th, in column D. D T.K. *for* W *read* E																	
22d,	—	18	—	—	—	—	—	—	—	—	16						
27th, ſubtract 10′ from Lat. A till 20th of June incluſive, and	—	—	—	—	—	—	15.32	—	—	—	—	—	—	—	—	15.22	
June 1ſt, ſubtract 10′ from Long. A to 25th of June incluſive, and	—	—	—	—	16.51	—	—	—	—	—	—	—	—	16.41			
5th,	—	—	—	20	—	—	—	—	—	—	—	—	19				
9th,	—	—	—	—	—	14. 1	—	—	—	—	—	—	—	—	14.15		
21ſt, ſubtract 11′ from Lat. A till 1ſt July incluſive, and	—	—	—	—	—	—	3.45	—	—	—	—	—	—	—	—	3.34	
26th,	21	—	—	—	10. 2	—	—	—	—	18	—	—	—	10.16			
27th,	—	—	—	—	10. 6	—	—	—	—	—	—	—	—	10.20			
28th,	—	—	—	—	11.10	—	—	—	—	—	—	—	—	11.24			
29th, add 13′ to Long. A till 2d Auguſt incluſive, and	—	—	—	—	12.18	—	—	—	—	—	—	—	—	12.31			
July 2d,	—	—	—	—	—	—	0.22 S	—	—	—	—	—	—	—	—	0.33 S	
3d,	—	—	—	—	—	—	1. 6	—	—	—	—	—	—	—	—	1.23	
4th, add 21′ to Lat. A. till 19 Auguſt incluſive, and	—	—	—	—	—	—	2.13	—	—	—	—	—	—	—	—	2.34	
24th, add 2′ to Long. T.K. till 28th July incluſive, and	—	—	—	—	—	28.12	—	—	—	—	—	—	—	—	28.14		
28th,	—	—	9	—	—	—	—	—	—	—	—	—	9				

N. B. The ſhip having an iron tiller, and much iron about the after part, it was found, the obſervations taken in the cabbin were not conſiſtent or meriting confidence. From *Anjengo* they were all taken on the binnacle, which ſeemed to be the only fit place in the ſhip.

1776		Lat.	Long.	Magnetical Variation Az.	Magnetical Variation Amp.	South Dip Face E.	South Dip Face W.	MD.	C.D.	
		° ′ N.	° ′ E.	° ′ W	° ′ W	°	°	°	°	
Oct. 14	Aſhore at *Trinquemalé* on Ceyloan.	8.32	81.30			3.6	3.6	3.6	4.5	
28	At anchor 10 fath. ſand and ſhells, *Anjengo* Road.	8.41	76.54	1.12*	1.13*	3.4	3.4	3.4	4 3	Very ſteady.
30		9.58	75. 4			1.6	1.6	1.6	2.5	Pretty ſteady.
31		9.54	74 24	1. 3*	0.53*	2.1	2.1	2.1	3.	Ditto.
Nov. 1	Off *Kalpeny.* } Lacadivé Iſl.	9.57	73.35	1.25*		1.7	1.7	1.7	2.6	Not very ſteady.
2	Off *Scheulpar.* } Lacadivé Iſl.	9 59	72.30			1.5	1.5	1.5	2.4	Pretty ſteady.
3		9.38	71.18			2.7	2.7	2.7	3.6	Ditto.
4		9 49	70.26	1.30*		3.2	3.2	3.2	4.1	Ditto.
5		9.53	69.11	2 12*	1.54*	3.	3.	3.	3.7	Ditto.
6		9.55	67.35			3.	3.	3.	3 7	Not very ſteady.
7		10.43	65.30	4. 0* *4. 4	4. 3* *4.18	1.4	1.4	1.4	2.3	Ditto.
8		11.57	63.22	4.23*		ND. 0.4	ND. 0.4	ND 0 4	0.3 ND.	Not ſteady, but taken when moſt at reſt.
9		12.45	60.34	5.15* *5.37		3.	3.	3.	2.1	Very unſteady.
10		12.59	57.21	6. 0* *7. 4	6. 6*	4.	4.	4.	3.1	Pretty ſteady.
11		13.29	55.12	7.30* *8.24	7.54*	5	5.	5.	4.1	Ditto.
12	Off *Socotra.*	13.16	52.55	7.57*	8.15*	5.4	5 4	5.4	4.5	Ditto.
13		13.28	50.58	9.20* *8.59	8.58* *9. 0	5.6	6.6	6.2	5.3	Very ſteady.
14	In ſight of *Coaſt* of *Arabia.*	13.51	49.37	9.35*	9.32*	7.2	8.6	8.	7.1	Quite ſteady.
15	Ditto.	13.37	48.33	9. 1* *8.46	9.30* *9.30	8.	9.	8.4	7.5	Ditto.

1776		Lat.	Long.	Magnetical Variation Az.	Magnetical Variation Amp.	North Dip Face E.	North Dip Face W.	MD.	C.D.	
		° ′	° ′	° ′	° ′	°	°	°	°	
Nov. 16	In ſight of *Coaſt* of *Arabia.*	12.54	47. 4	9.31* *10.38	9.39* *10.35	6.2	7.2	6.6	5.7	Pretty ſteady.
17	*Cape Aden* in ſight.	12.41	45.25	*11.25	*11.30	6.2	7.2	6.6	5 7	Ditto.
18		12.38	[44. 5]	11. 3* *11.14	11. 5* *11.18	7.2	8.2	7.6	6.7	Ditto.
20	At anchor at *Mocha.*	13.22	44.10	11.35*	11.22*	8.6	9.6	9.2	8.3	Ditto.
21	Ditto.	13.22	44.10		11. 2*	8.4	9.4	9.	8.1	Not ſteady, much wind.
	Aſhore at the *Company's factory.*	13.20	44.11			9.2	9.6	9.4	8.5	
22	In ſight of *Gebel Zeker.*	14.24	43.35		11.11*	12.4	12.4	12.4	11.5	Pretty ſteady.
23	In ſight of *Gebel Tar.*	15.29	43.12	13.10* *13.14	12.50*	14.2	14.2	14.2	13 3	Very ſteady.
24	Ditto.	16. 2	42.49	14. 3* *12.57	13.53*	15.6	15.6	15.6	14.7	Ditto.
25		16.24	42.22	14. 2* *13.31	14.19*	16.6	16.6	16.6	15.7	Ditto.
26		17. 5	41.54	13.44*	13.50*	19.1	19.1	19.1	18.2	Ditto.
27		18.14	41. 0	13 54*	13.58*	20.6	21.	20.7	20.	Pretty ſteady.
28		19.36	40 30	13.12* *12.56	13.19* *12.42	24.2	24.6	24.4	23.5	Ditto.
29	Off *Judda.*	20.59	39.56	12.52*	12.59*	26.7	26.7	26.7	26.	Ditto.
30		21.43	39. 3		12.49*	29.6	29.6	29.6	28.7	Very unſteady, much wind and pitching.
Dec. 1	In ſight of *Coaſt* of *Abyſſinia.*	21.56	37.57				about	30.		Very unſteady.
2		22.44	38.24			32.6	32.6	32.6	32.	Not very ſteady.
3		22.46	38.11			32.	32.	32.	31.2	Pretty ſteady.
4		22.54	37.44			32.4	32.4	32.4	31.6	Not very ſteady.
6	At anchor on *Coaſt* of *Arabia.*	24.16	38.29			34.6	34.6	34.6	34.	Pretty ſteady.
7	Off *Coaſt* of *Arabia.*	24.17	38.21	14.52*	15. 0*					

XX. *An Essay on Pyrometry and Areometry, and on Physical Measures in general.* By John Andrew De Luc, *F. R. S.*

PART THE FIRST.

Concerning the measure of the expansion of solids by heat.

Read March 19, 26, and April 9, 1778.

MY investigations of this measure have been owing to accident. A new Hygrometer led me to them: I have already mentioned this instrument in the paper which the Royal Society has done me the honour to insert in the last volume of the Philosophical Transactions.

I had

Essai sur la Pyrométrie et l'Aréométrie, et sur les Mesures Physiques en général. Par J. A. De Luc, *Membre de la Societé Royale,* &c.

PREMIERE PARTIE.

De la mesure des expansions des solides par la chaleur.

MES recherches sur cette mesure ont été accidentelles: c'est un nouvel Hygrométre qui en est l'occasion. Je faisois déja mention de cet instrument dans le mémoire que la Societé Royale m'a fait l'honneur d'insérer dans les Transact. Phil. de l'année derniére.

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I had carried it with me to the top of the Hartz, with an intention to repeat there the obſervation upon the dryneſs of mountainous air, which I had made in the Alps; but it fell out, as it often does on mountains, that what I did obſerve was the extreme humidity.

I will not enter upon the conſtruction of this inſtrument, which I have not yet been able to take up again, to bring it to the exactneſs of which it is capable: all that is neceſſary to mention here is, that it is made of ivory, as the firſt was, but in a glaſs frame; and that the effects of the *humor* upon ivory being inconſiderable, I wanted, in order to meaſure them correctly, to deſtroy the effect of heat upon the frame, which I have done (as in the compound pendulum) by the expanſion of a rod of braſs in a contrary direction. But to do this it became neceſſary to determine the proportion between the

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Je l'avois porté ſur la plus haute ſommité du Hartz, dans l'intention d'y répéter l'obſervation de la ſécherеſſe de l'air des montagnes, que j'avois faite dans les *Alpes*: mais il arriva, ce qui arrive auſſi très ſouvent ſur les montagnes, que ce fut l'humidité extrême que j'y obſervai.

Je n'entrerai pas dans le détail de la conſtruction de cet inſtrument, que je n'ai pu reprendre encore pour l'amener au point d'exactitude dont il eſt ſuſceptible: il ſuffira de dire ici, qu'il eſt d'yvoire comme le prémier que j'avois imaginé, que ſa monture eſt de verre, et que les effets de l'*humor* ſur l'yvoire étant peu conſidérables, j'ai voulu, pour les meſurer correctement, détruire l'effet de la chaleur ſur la monture, ce qui s'exécute, comme dans les pendules compoſés, par l'expanſion en ſens contraire d'une lame de léton. Il failloit donc déterminer les rapports

the dilatations of braſs and glaſs by heat, and that was the occaſion which led me to Pyrometry. One cannot advance a ſtep towards the improvement of any of the ſciences, without contributing at the ſame time to bring the others to the ſame level.

Being thus obliged to know with ſome degree of accuracy the relations of dilatations between braſs and glaſs, I began by conſidering the methods which had been made uſe of to eſtimate them, and found in them nothing but uncertainty. The mountings of the inſtruments were to be ſuſpected, and their influence not ſufficiently guarded againſt: Micrometers appeared to me uncertain; for wheels and levers are liable to almoſt unavoidable irregularities; ſimilar degrees of preſſure in the contact are difficult to eſtimate; and ſuch methods of increaſing ſmall phyſical

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rapports des dilatations du léton et du verre par la chaleur; et c'eſt cet objet qui m'a jetté dans la Pyrométrie. On ne ſauroit faire avancer d'un dégré vers la perfection quelque branche des ſciences, ſans qu'elle tende à porter les autres au même niveau.

Me trouvant donc dans la néceſſité de connoître avec quelque préciſion ces rapports des dilatations du léton et du verre, je réflechis ſur les moyens qu'on avoit employés pour les déterminer, et je n'y trouvai qu'incertitude. Les montures des machines me parurent ſuſpectes; je ne trouvai pas qu'on fût aſſez à l'abri des effets de leur propre dilatation: les Micrométres ſurtout me parurent peu ſûrs: car des rouages et des leviers ſont ſujets à des irrégularités preſque inévitables; des dégrés ſemblables de preſſion dans les contacts ſont difficiles à ſaiſir; et agrandir ainſi

physical effects render them indeed more apparent, but do not at all contribute to their exact mensuration.

I had heard the ingenious Mr. RAMSDEN say, that he had a notion of a Pyrometer different from all that had been invented; and knowing his great skill in philosophical and mechanical matters, I applied to him, and pressed him to execute his idea. The multitude of his other engagements prevented his complying with my request; and he advised me to look no farther for the proportions of the expansions of brass and glass than to Mr. SMEATON's experiments, which he looked upon, with reason, as the best that had been made[a]. Still, however, upon my desiring him to explain by what means he thought of being able to correct the faults of the ancient instruments, he was kind

(a) Phil. Trans. 1754.

enough

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ainsi les petits effets physiques, c'est bien les rendre plus apparents, mais nullement les mesurer avec exactitude.

J'avois ouï dire à l'ingénieux Mr. RAMSDEN, qu'il avoit l'idée d'un Pyromètre différent de tous les autres; et connoissant sa grande intelligence dans les matières de physique et de méchanique, j'eus recours à lui, et je le pressai d'exécuter son plan. Mais la multitude des objets qui l'occupent l'en empêcha, et il me conseilla de m'en tenir pour le rapport des expansions du léton et du verre, aux expériences de Mr. SMEATON, qu'il regardoit avec raison comme les plus sures*. Je priai cependant Mr. RAMSDEN de m'expliquer par quel moyen il comptoit de pouvoir éviter les défauts des machines anciennes; et il eut la complaisance

* Phil. Trans. 1754.

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enough to do it, and told me, that he propoſed meaſuring the expanſions of bodies, by the Micrometer of a Microſcope; by which means he ſhould obviate the greateſt mechanical difficulties. He added, moreover, that he had made a firſt trial of his method a long while ago, and was aſſured of the ſucceſs.

This idea ſtruck me, and being very deſirous of following it in my preſent need, I determined, if I could hit upon any method within my compaſs of ability, to undertake to execute it myſelf.

I found many difficulties ſo long as I only thought of abſolute meaſures of the expanſions of bodies. I was determined not to ſet to work without the hope of making an inſtrument that ſhould be really an exact one, and the Micrometer always puzzled me. But coming happily to reflect that I did not want abſolute meaſures,

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de le faire. Il me dit donc, qu'il ſe propoſoit de meſurer les éxpanſions des corps, au moyen du Micrométre d'un Microſcope; ce qui éviteroit les plus grandes difficultés méchaniques: il ajouta même, qu'il avoit fait depuis long tems un premier eſſai de cette méthode, et qu'il étoit perſuadé du ſuccès.

Cette idée me frappa; et deſirant beaucoup d'en faire uſage dans mon beſoin préſent, je me déterminai à entreprendre de l'exécuter moi-même, ſi je pouvois imaginer quelque moyen qui fût à ma portée.

Je trouvai beaucoup de difficultés à cette entrepriſe, tant que je ne ſongeai qu'à des meſures abſolues des expanſions des corps. Je ne voulois pas mettre la main à l'oeuvre, ſans avoir l'eſpérance de faire une machine vraiment exacte; et le Micromètre m'embaraſſoit toûjours. Mais venant heureuſement à conſidérer, que

meaſures, and that it was enough for me to find the proportions of dilatibility between two different bodies, I was led by that idea to a very ſimple method, which made all the difficulties vaniſh, and gave me the confidence I wanted to ſet me to work. Afterwards, indeed, I went much farther than I expected in the abſolute meaſures themſelves, as I ſhall ſhew, after having firſt explained how I propoſed to aſcertain the relative expanſions, and the great advantage of that method in practice.

Principle on which is founded the comparative meaſure of the expanſions of bodies by heat.

I ſuppoſe[b] a perſon to take two rods of the ſame ſort of ſubſtance, or of two different ſubſtances equally dilatable

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que je n'avois pas beſoin de meſures abſolues, et qu'il me ſuffiſoit de trouver avec certitude les rapports des dilatabilités de deux corps différens, je fus conduit par là à une idée fort ſimple, où toutes les difficultés s'évanouirent: ce qui me donna la confiance dont j'avois beſoin pour mettre la main à l'oeuvre. Mais enſuite j'ai été plus loin que je n'aurois oſé eſpérer, dans les meſures abſolues mêmes: c'eſt ce que j'expoſerai, après avoir expliqué d'abord, comment je me propoſai de trouver les expanſions rélatives, et la ſureté qu'il y a dans la pratique en les enviſageant ſous ce point de vuë.

Principe de meſure comparative des expanſions des corps par la chaleur.

Je ſuppoſe (b) qu'on prenne deux branches de même matière, ou de matières également dilatables par la chaleur, et que, les pôſant l'une ſur l'autre, on les lie

(b) See plate. VII. fig. 1. and its explanation.

dilatable by heat, and laying them one over the other, to rivet them together at one of their ends. If then they are solidly suspended by the opposite end of one of the rods only, and on the free rod there be marked a point, at the level of the point of suspension of the other; these two points will remain equally immoveable, whatever be the heat which affects the two rods, so long as it affects them equally: for the expansion downwards in the fixed rod will be compensated by the expansion upwards in that which is free; and consequently, the point marked upon this will always remain equally high, that is, corresponding in the same manner with the point of suspension of the other; so the proportion of expansions of the two rods will be that of equality, since the distances from the point of union of the rods to the two immoveable points will be equal; and that, consequently,

lie ensemble par un de leurs bouts: si on les suspend solidement par le bout opposé de l'une des branches seulement, et qu'on marque un point sur la branche libre vis à vis du point de suspension de l'autre, ces deux points resteront également immobiles, quelle que soit la chaleur qui affecte les deux branches, dès que ce sera également: car l'allongement vers le bas dans la branche fixée, sera compensé par l'allongement vers le haut dans la branche libre; et par conséquent le point marqué sur celle ci, restera toûjours exactement à la même hauteur, c'est à dire vis à vis du point de suspension de l'autre. Ainsi le rapport des expansibilités des deux branches, sera celui d'égalité; puisque la distance du point de réunion des branches, aux deux points immobiles, sera la même, et que par

ſequently, the ſame length of the two ſubſtances will be requiſite to produce the ſame lengthening of them by heat.

But if the free rod ſhould have more or leſs expanſibility than the fixed rod, the immoveable point of the former will not be any longer at the ſame height as the point of ſuſpenſion of the latter; it will be lower, if its expanſibility be greater, becauſe a leſs length of this rod will be required to make up for the whole lengthening of the other. It will be higher, on the contrary, if its expanſibility be leſs; and the diſtances, from the point immoveable by ſuſpenſion, and the point immoveable by compenſation, to the point of union, will be always in the inverſe ratio of the expanſibility of the two ſubſtances.

If then we can find this immoveable point by compenſation, and its diſtance from the point of union, that of the point of ſuſpenſion being given, we ſhall have the relation

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par conſéquent il aura fallu une même longueur des deux matières, pour produire un même allongement par la chaleur.

Mais ſi la branche libre a plus ou moins d'expanſibilité que la branche fixée, le point immobile de la première, ne ſera plus vis à vis du point de ſuſpenſion de l'autre: il ſera plus bas ſi ſon expanſibilité eſt plus grande; parce qu'il faudra moins de longueur de cette branche, pour compenſer tout l'allongement de l'autre: il ſera plus haut au contraire, ſi ſon expanſibilité eſt moindre: et les diſtances du point de réunion, au point immobile par la ſuſpenſion et au point immobile par compenſation, ſeront toûjours en raiſon inverſe de l'expanſibilité des deux matiéres.

Trouver donc ce point immobile par compenſation, et ſa diſtance au point de réunion, celle du point de ſuſpenſion étant connue, ce ſera trouver le rapport des expanſibilités

relation of the expanſibilities of the two ſubſtances. Now nothing is eaſier than to do this by means of a Microſcope, furniſhed with a ſingle immoveable wire; for the wire being fitted to a point of the free rod, and the two rods being equally warmed, if this point moves, it is a ſign that it is not that which is ſought for, which will then ſoon be found by pointing the Microſcope higher or lower on the free rod, according to what ſhall have been indicated by the firſt trials.

Thus then will the relation of the expanſibilities of two ſubſtances be procured without the neceſſity of having recourſe to any Micrometer; conſequently, without the riſks of the errors thoſe inſtruments are ſubject to, when they are uſed in very nice meaſures. All that will be neceſſary for the exactneſs of the obſervations will only be,

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expanſibilités des deux matiéres. Or il eſt très aiſé de le trouver par le moyen d'un Microſcope muni d'un ſeul fil immobile. Car en ajuſtant ce fil ſur un point de la branche libre, et échauffant également les deux branches, ſi ce point ſe meut, ce ne ſera pas celui qu'on cherche; mais on le trouvera bientôt par tatonnement, en pointant le Microſcope plus haut ou plus bas ſur la branche libre, ſuivant que les prémières tentatives l'auront indiqué.

On aura donc ainſi les rapports des expanſibilités de deux matières, ſans beſoin de Micromètre, et par conſéquent ſans être expoſé aux erreurs qu'il pourroit introduire dans des meſures auſſi délicates: et tout ce qui eſt néceſſaire pour l'exactitude ne conſiſte qu'à s'aſſurer, qu'un Microſcope

be, to be affured, that, whilft the fubftances compared are warming, a Microfcope and a point of fufpenfion will be fecured from being difturbed by any motion; which is not very difficult.

Defcription of an inftrument, intended to find out the comparative expanfibilities of bodies by heat.

I flatter myfelf, that a defcription will make this inftrument fufficiently underftood to render it unneceffary for me to give a figure of it: fhould the fociety, however, be defirous of having a drawing of it, I fhall with pleafure obey their commands[c].

The bafis of the inftrument is a rectangular piece of deal-board, very ftrait-grained, two feet and a half long, fifteen inches broad, and one inch and a half thick: it is to this that all the other parts are fixed. The firft thing I did

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et un point de fufpenfion feront mis à l'abri de tout mouvement, tandis qu'on échauffera les matières comparées; ce qui n'eft pas bien difficile.

Defcription d'un inftrument deftiné à trouver les expanfibilités comparatives des corps par la chaleur.

Je crois pouvoir me difpenfer de donner une figure de ma machine, parce que j'efpéré qu'une fimple defcription la fera comprendre. Si cependant la Société Royale fouhaitoit que je la fiffe deffiner, je me conformerois à fon intention (c).

Une planche de fapin à fibres bien droites, de $2\frac{1}{2}$ pieds de long, 15 pouces de large, et $1\frac{1}{2}$ pouce d'épaiffeur, fait la bafe de la machine. C'eft à cette planche

(c) See plate VII. fig. 2. and its explanation.

que

I did was, to mount it in the manner of a table, with four deal legs, each a foot long, and an inch and a half ſquare, very ſolidly fitted near its four angles, and kept together at the other ends by four croſs pieces, likewiſe very ſolid. I ſhall always conſider this ſmall table as hung to a ſtand, capable of being tranſported to whatever part there is moſt light in; the board being in a vertical ſituation in the direction of its grain, and bearing its legs forward in ſuch a manner as that the croſs pieces which join them may form a frame, likewiſe placed vertically facing the obſerver.

This frame ſuſtains the Microſcope, which is firmly fixed in another frame that moves in the former by means of grooves; but with ſuch a degree of tightneſs as to render it neceſſary to uſe ſmall ſtrokes of a hammer in order to make it ſlide. The preſſure of four ſcrews will

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que ſont attachées toutes les autres parties. Je l'ai montée d'abord comme une petite table, ayant quatre jambes de ſapin d'un pied de long et d'1½ pouce en quarré, poſées bien ſolidement près de ſes quatre angles, et réunies à l'autre extrémité par quatre traverſes auſſi très ſolides. Je ſuppoſerai toûjours cette petite table ſuſpendue à un ſupport propre à être tranſporté là où l'on aura le plus de lumière, ſa planche étant dans une ſituation verticale dans la direction de ſes fibres, et portant ſes quatre jambes en avant, de manière que les traverſes qui les réuniſſent forment un cadre, ſitué auſſi verticalement en face de l'obſervateur.

C'eſt à ce cadre qu'eſt ſolidement adapté le Microſcope, dans un chaſſis qui monte et deſcend à couliſſe, et avec aſſez de juſteſſe, pour qu'il faille employer de petits coups de marteau pour le faire mouvoir: la preſſion de quatre

will give it the degree of friction one thinks proper. I preferred this mode to that of moving the Microscope by a screw, because this last would have required metal, which is more susceptible of the impressions of heat than deal is; and because the execution of it would have been longer, and more expensive, from the degree of perfection it would have required, whilst this simple method has succeeded perfectly well.

The inner sliding frame, which is likewise of deal, keeps the tube of the Microscope in an horizontal position, and in great part without the frame, insomuch that the end which carries the lens is but little within the space between the frame and the board. This Microscope is constructed in such a manner as that the object observed may be an inch distant from the lens, and it

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quatre vis lui donne le dégré de frottement que l'on veut. J'ai préféré ce moyen, à celui de conduire le Microscope par une vis; parce que ce dernier auroit exigé du métal, plus susceptible des impressions de la chaleur que le sapin; et que l'exécution auroit été plus longue et plus dispendieuse, par la perfection qu'elle eût exigé: tandis que cette voye simple a parfaitement réussi.

Le chassis, qui est aussi de sapin, tient le tube du Microscop. situé horizontalement, et en grande partie au dehors du cadre; tellement que le bout qui porte la lentille n'est que fort peu en dedans de l'espace compris entre le cadre et la planche. Ce Microscope est construit de manière, que l'objet observé peut être

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it has a wire which is situated in the focus of the glasses in which the objects appear reversed.

At the top of the apparatus there is a piece of deal, an inch and a half thick, and two inches broad, lain in an horizontal direction from the board to the top of the frame. It is this piece to which the rods of the different substances, whose expansion by heat one wants to measure, are suspended: one end of it slides into a socket, which is cut in the thickness of the board; and the other end, which rests upon the frame, meets there with a screw which makes the piece move backwards and forwards, to bring the objects to the focus of the Microscope. There is a cork very strongly driven through a hole bored vertically through this piece; and it is in another hole, likewise vertical, made through the cork, that

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à un pouce de distance de la lentille; et il porte un fil au foyer des verres, où les objets se peignent renversés.

Au haut de l'appareil est une pièce de sapin, d'un pouce et demi d'épaisseur et de deux pouces de largeur, qui part horizontalement de la planche et vient se reposer sur le haut du cadre. C'est à cette pièce que sont suspendues verticalement les branches des diverses matières dont on veut mesurer l'expansion par la chaleur: elle glisse par un bout, en tenon, dans une mortaise percée dans l'épaisseur de la planche; et l'autre bout, qui repose sur la cadre, y est reçu par une vis qui fait mouvoir la pièce en arrière ou en avant, pour amener les objets au foyer du Microscope. Un bouchon de liège passe avec force par un trou percé verticalement dans cette pièce; et c'est par un trou, aussi vertical, qui traverse ce

that the rods are fixed at the top: so that they hang only, and their dilatation is not counteracted by any pressure.

In order to convince myself of the solidity of this instrument, I adjusted the wire of the Microscope upon a point of a rod thus suspended, and left it in that state for several hours, during which I not only moved but struck the machine, without perceiving any change in the relative position between the point and the wire, which perfectly answered my end.

The next thing to be done was to heat my rods: for this purpose I procured a cylindrical bottle of thin glass, about twenty-one inches high, and four inches in diameter, which I placed in the inside of my machine, upon a stand independent of the rest of the apparatus. The rods are suspended in this bottle at a little less than an inch

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ce liège, que les branches sont retenues par leur bout supérieur: elles sont ainsi simplement suspendues, et par conséquent leur dilatation ne se consume à aucun effort.

Pour m'assurer de la solidité de cette machine, j'ajustai le fil du Microscope sur un point d'une branche ainsi suspendue, et je le laissai dans cet état pendant plusieurs heures, en transportant et heurtant même la machine, sans appercevoir aucun changement dans le rapport du point avec le fil: ce qui repondoit parfaitement à mon but.

Il s'agissoit alors d'échauffer mes branches. Je fis faire pour cela une bouteille cylindrique de verre mince, de 21 pouces de haut et de 4 pouces de diamétre, que je plaçai dans l'intérieur de ma machine sur un support indépendant de tout le reste de l'appareil. Les branches pendent dans cette bouteille à un peu moins

inch diſtance from one of the ſides, in order to have them near the Microſcope. I poured into this bottle water of different degrees of heat, which I ſtirred about with a little piece of wood, faſtened horizontally at the end of a ſtick, which I moved upwards and downwards at one of the ſides of the bottle; in this water I hung a thermometer, the ball of which reached to the middle of the height of the rods.

The water during theſe operations riſes to the cork, which thus determines the length of the heated part: the bottle is covered, to prevent the water from cooling too rapidly at the ſurface; and a thin caſe of braſs prevents the depoſit of the vapor upon the piece of deal to which the rods are fixed.

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moins d'une pouce de diſtance de l'un des côtés, parce qu'il faut qu'elles ſoyent à portée du Microſcope. Je verſe de l'eau à diverſes températures dans cette bouteille, et pendant les expériences, j'agite cette eau par le moyen d'une petite planchette tenue horizontalement au bout d'un bâton, que je fais mouvoir de haut en bas et de bas en haut à l'un des côtés de la bouteille. Je ſuſpends dans cette eau un Thermométre dont la boule atteint le milieu de la hauteur des branches.

L'eau, pendant les opérations, s'élève juſqu'au liège, qui détermine ainſi la longueur de la partie échauffée. La bouteille a un couvercle, pour empêcher le réfroidiſſement trop rapide de l'eau à la ſurface; et un étui de léton mince empêche ſa vapeur de ſe dépoſer ſur la pièce de ſapin où les branches ſont fixées.

 Voila

This is a ſketch of my machine, the whole of which conſiſts in the keeping the Microſcope and the point of ſuſpenſion of the rods free from motion during the obſervation, and in heating the rods with water. I will now give an account of the experiments I have made with it.

Application of the method of finding the proportions between the expanſibilities of different matters by heat.---Determination of the relative expanſibilities of braſs and glaſs.

The firſt experiment I made with this machine was that which I wanted for my Hygrometer. I took a glaſs tube, ſimilar to thoſe which are made uſe of for common

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Voila l'eſquiſſe de cette machine, où tout conſiſte à rendre le Microſcope et le point de ſuſpenſion des branches à l'abri de mouvement pendant l'obſervation; et à échauffer les branches par le moyen de l'eau. Je vais maintenant rendre compte des expériences que j'ai faites par ſon moyen.

Application de la méthode de trouver les rapports des expanſibilités de matieres differentes par la chaleur.—Détermination des expanſibilités relatives du léton et du verre.

La premiere expérience que je fis avec cette machine, fut celle dont j'avois beſoin pour mon Hygrométre. Je pris une branche de verre percée, ſemblable à celles qu'on employe aux Barométres communs dont le tube eſt fort épais: ces tubes

common Barometers; that which I uſed for the frame of my Hygrometer, and on which I made the experiment, had an external diameter of about three-eighths, and an internal of about one-eighth of an inch: the rod was from twenty-one to twenty-two inches long, but it paſſed under the cork only eighteen Engliſh inches, reckoning from the point to which was fixed at its bottom the lamella of braſs the dilatation of which I wanted to compare with that of the glaſs. The lamella was applied from this point length-ways and upwards along the tube: it had been made thin by rollers, to render it the more elaſtic; and as it was too thin to ſupport itſelf upright, I kept it ſtretched in that direction by means of a thread, which, going over a pulley, bore at its other end a weight fit to give it the ſame degree of tenſion which it has in my Hygrometer.

Upon

tubes dont je fais uſage pour la monture de mes Hygromètres, et que je ſoumis à l'expérience, ont environ $\frac{3}{8}$ de pouce de diamètre extérieur, et d'$\frac{1}{8}$ à l'intérieur. Le tube que j'emploiai avoit 21 à 22 pouces de long, mais il n'excédoit le liège que de 18 pouces Anglois, à compter du point où étoit fixée dans le bas la lame de léton dont je voulois comparer la dilatation avec celle du verre. De ce point, la lame de léton s'élevoit le long du tube. Elle étoit faite au laminoir pour la rendre plus élaſtique; et comme elle ſe trouvoit trop mince pour ſe ſoutenir de bout par elle-même, je la tenois tendue dans cette direction par un fil, qui, paſſant ſur une poulie, portoit à ſon autre bout un poids propre à lui donner le même degré de tension qu'elle éprouve dans mon Hygromètre.

 J'avois

Upon this lamella I had marked a scale which began at its point of union with the glass, and was divided into small equal parts at the space along which I thought I might be obliged to move my Microscope, in order to look for the point which would neither rise nor fall by the variations of the heat.

Hitherto the difficulties had been inconsiderable, or rather I had experienced none at all: but it was not so in the physical inquiry which was my first object. The nearer we survey nature, the more we see of the difficulty there is in unfolding her mysteries; as in the affairs of ordinary life, those ever find them the most difficult, who understand them best. The moral and physical Microscope are equally fit to render men cautious in their theories.

The

J'avois tracé sur cette lame une échelle qui partoit de son point de réunion avec le verre, divisée en petites parties égales dans l'étendue où je pensois pouvoir être obligé de promener mon Microscope pour chercher ce point fixe, c'est à dire le point qui ne monteroit ni ne descendroit par les variations de la chaleur.

Les difficultés avoient été fort peu considérables jusques là, ou plutôt je n'en avois éprouvé aucune. Mais il n'en fut pas de même dans la recherche physique qui étoit mon premier objet. Plus nous voyons de près la Nature, plus nous appercevons les difficultés qu'elle oppose à se laisser dévoiler. C'est ainsi que dans les affaires mêmes de la vie, il n'y a personne qui les trouve plus difficiles que ceux qui les voyent le mieux. Le Microscope, physique ou moral, est bien fait pour rendre l'homme circonspect dans ses théories.

The firſt phyſical difficulty I met with was not new to me: I had already met with it in two different machines in which I had made uſe of metals; in the one, to mark the variations of the heat; in the other, to compenſate the effects of it: it was the irregular dilatations of metals.

The laſt of theſe machines, which is that on which I have beſtowed the moſt care, and which I have ſtudied with the greateſt attention, corrects the effects of the heat upon a Barometer, and upon an Hygrometer of my firſt conſtruction, which is joined to it. A ſtrong rod of well hardened braſs ſupports upon an edge, at a convenient diſtance from the center of motion, a lever, which holds the ſcale of the Barometer ſuſpended, and makes it riſe or fall by the dilatation or condenſation of the braſs rod, as the quickſilver riſes or falls in the

La première des difficultés phyſiques que j'ai rencontrées ne m'etoit pas inconnue: je l'avois déja éprouvée en deux machines différentes où j'avois employé des métaux, dans l'une pour marquer les variations de la chaleur, et dans l'autre pour en compenſer les effets; c'eſt l'irregularité des dilatations des métaux.

La dernière de ces machines, qui eſt celle à laquelle j'avois donné le plus de ſoin, et que j'ai étudiée avec le plus d'attention, corrige les effets de la chaleur ſur un Baromètre, et ſur un Hygromètre de ma première conſtruction qui lui eſt joint. Une forte branche de léton, bien durcie à la filière, ſoutient ſur un tranchant, à une diſtance convenable du centre de mouvement, un levier qui tient ſuſpendue l'echelle du Baromètre, et qui la fait monter ou deſcendre, par la dilatation ou condenſation de la branche de léton, comme le mercure monte ou deſcend

the Barometer, by the correſponding variations of heat. This ſcale of the Barometer, when it moves, draws or looſens a thread of ſilk-graſs, which goes over a ſmall pulley placed upon the ſame axis with a much larger one, to which the ſcale of the Hygrometer is hung likewiſe by a ſimilar thread, which thus varies, by the proportion of the diameters of the pullies, as the heat makes the quickſilver in the Hygrometer vary.

This inſtrument is extremely convenient for meteorological obſervations, becauſe it ſaves one obſervation, and two corrections for the heat, and thus makes a ſaving of time, which is ſpecially precious to the Aſtronomer, who has ſo many objects of attention. But it is neceſſary from time to time to correct an irregularity in it, which one eaſily perceives by means of an index carried by the moveable ſcales

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deſcend dans le Baromètre par les variations correſpondantes de la chaleur. Cette échelle du Baromètre, dans ſes mouvemens, tire ou relâche un fil de pite, qui paſſe ſur une petite poulie, miſe ſur un même axe avec une autre beaucoup plus grande à laquelle pend, auſſi par un fil de pite, l'échelle de l'Hygromètre, qui varie ainſi, par le rapport des diamètres des poulies, comme la chaleur fait varier le mercure de l'Hygromètre.

Cet inſtrument eſt très commode pour les obſervations météorologiques, parce qu'il diſpenſe d'une obſervation et de deux corrections pour la chaleur, et qu'il épargne ainſi du tems; œconomie principalement utile à l'Aſtronome, qui a tant d'objets d'attention à la fois. Mais il faut y corriger de tems en tems une irrégularité, qu'on apperçoit aiſément par le moyen d'un index que portent les échelles

ſcales of the two inſtruments, which, going over immoveable ſcales of the ſame ſort, ſhews their difference of height. When this difference is no longer conformable to the indication of the Thermometer, it is eaſily rectified by turning ſmall pegs, on which are twiſted the thread of ſilk-graſs which ſerves for the ſuſpenſion of the ſcales.

The irregularity of which I have been ſpeaking conſiſts in this, that when the heat, after having varied, returns to the ſame point of the quickſilver Thermometer, the metallic Thermometer does not return to it exactly, but varies nearly in the following manner. During the ſummer the metallic Thermometer gains conſtantly on the other; I mean, that amidſt its variations, it always preſerves a ſmall part of the lengthening, which

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échelles mobiles des deux inſtrumens, et qui, paſſant ſur des échelles immobiles ſemblables, fait voir ainſi leur différence de hauteur. Si cette différence ceſſe d'être conforme à l'indication du Thermomètre, on la rectifie aiſément, en tournant de petites chevilles ſur leſquelles s'enveloppent les fils de pite qui ſervent à la ſuſpenſion des échelles.

L'irrégularité dont il s'agit conſiſte en ce que, lorſque la chaleur, après avoir varié, revient au même point ſur le Thermomètre de mercure, le Thermomètre métallique n'y revient pas toûjours exactement; et la marche des irrégularités eſt celle-ci: en Eté, le thermomètre métallique gagne de plus en plus ſur l'autre; c'eſt à dire que, dans ſes variations, il conſerve toûjours quelque petite partie de l'allonge-

which is at that time its ordinary ſtate. In winter, on the contrary, it becomes inſenſibly a little too ſhort.

The other metallic Thermometer I have mentioned is made of lead. I made it ſeven or eight and twenty years ago, for an inſtrument which is more agreeable than uſeful, on account of its irregularity. A rod of lead, communicating by a thread of ſilk-graſs, with a ſmall pulley fixed to the ſame axis with a greater one, conducts, by means of another pulley, a needle through whoſe axis, which is bored, paſſes another axis which carries the needle of a pulley Barometer. Thus this inſtrument marks the heat and weight of the air upon two concentric circles, by means of two needles turning upon the ſame center, as in clocks; beſides which, the needle of the Thermometer points out upon a third circle the correction for the heat, to be made on

l'allongement qui fait alors ſon état ordinaire: en hiver au contraire, il devient inſenſiblement un peu trop court.

L'autre Thermomètre métallique dont j'ai parlé, eſt de plomb. Je le fis il y a 27 ou 28 ans pour une machine plus agreable qu'utile à cauſe de ſon irrégularité. Une branche de plomb, communiquant auſſi par un fil de pite avec une petite poulie, miſe ſur un même axe avec une plus grande, conduit, par une autre poulie et au moyen d'un contrepoids, une aiguille, dont l'axe percé, laiſſe paſſer celui qui porte l'aiguille d'un Baromètre à poulie. Ainſi cet inſtrument marque ſur deux cercles concentriques la chaleur et le poids de l'air, par deux aiguilles tournant ſur un même centre, comme dans les pendules; et celle du Thermomètre indique de plus ſur un troiſième cercle, la

on the Barometer, which at that time I had already determined.

The irregularities of this last metallic Thermometer are of the same nature, but much more considerable than those of the other: this had already made me suspect, that they proceeded from the metal itself, though the wood, of which the frame was made, appeared a little suspicious on account of the humidity.

I met with the same effects in my last experiments, when the frame no longer interfered; and the irregularity shewed itself in this manner. Whenever I had observed the point at which my Microscope pointed upon the lamella of brass, which was suspended, with the rod of glass, in water at the temperature of the air, and then put warm water in the room of this; if I cooled the water

la correction à faire pour la chaleur sur le Baromètre, que j'avois déja déterminée alors.

Les irrégularités de ce dernier Thermomètre métallique, qui sont de même nature que celles de l'autre, sont encore beaucoup plus grandes; ce qui m'avoit déja fait soupçonner qu'elles procédoient du métal même; quoique la monture, qui est de bois dans les deux machines, me fût un peu suspecte à cause de l'humidité.

Dans mes dernières expériences j'ai éprouvé les mêmes effets; et ici la monture n'étoit plus une raison de doute. Voici comment cette irrégularité se manifestoit. Quand j'avois observé le point où mon Microscope visoit sur la lame de léton suspendue avec la branche de verre dans l'eau à la température de l'air, et que je substituois à cette eau de l'eau chaude; si je refroidissois l'eau peu à peu, je

water by degrees, I found that the lamella of brafs preferved a little of its lengthening. I did not fufpect the rod of glafs, becaufe the elafticity of this fubftance is phyfically perfect, and that this quality feems to me to be proper to bring back bodies to the fame ftate, whenever the caufes, of what nature foever they be, which have drawn them from it, ceafe: and I had foon after direct evidence of the irregularity not proceeding from the glafs.

It was eafy for me to find the proportion of the lengths of the brafs and the glafs, which, changing fuddenly the water, which had the fame temperature as the air, for warm water, produced no difference in the heighth of the point upon which the Microfcope pointed: which fhewed that the two fubftances had been equally expanded in contrary directions; but when I afterwards gradually

je trouvois que la lame de léton confervoit un peu de fon allongement. Je ne fufpectois pas la branche de verre; vu que l'élafticité de cette matière eft phyfiquement parfaite, et que cette qualité me paroit propre à ramener les corps au même état, quand les caufes, quelles qu'elles foyent, qui les en ont tirés, viennent à ceffer: et j'eus lieu enfuite de reconnoitre immédiatement que l'irrégularité ne venoit pas du verre.

Il me fut aifé de trouver la proportion des longueurs du léton et du verre par laquelle, changeant tout à coup l'eau qui étoit à la température de l'air, en de l'eau chaude, il ne fe trouvoit aucune différence dans la hauteur du point fur lequel le Microfcope vifoit: ce qui montroit que les expanfions des deux matières en fens contraires avoient été égales. Mais en diminuant enfuite par degrès la chaleur

gradually diminiſhed the heat of the water, this point roſe little by little; that is to ſay, the lamella of braſs preſerved a part of its lengthening, whilſt the rod of glaſs contracted itſelf to the ſame point at which it was at the beginning of the experiments. After a number of ſimilar trials, which always ended in the ſame manner, I went to work another way: when no change had been produced in the heighth of the point, by the ſudden change of water of the ſame temperature as the air into warm water, I immediately put, in the place of that warm water, water of the ſame temperature as the air, and in that caſe the point did not change neither.

It will not be uſeleſs to mention here, that I had in my bottle a ſyphon with a cock, by which I drew out the water without motion; and that I poured it in

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chaleur de l'eau, ce point s'élevoit peu à peu; c'eſt à dire que la lame de léton conſervoit une partie de ſon allongement, tandis que la branche de verre ſe contractoit au même point où elle l'étoit au commencement de l'expérience.

Après nombre de tentatives de même eſpéce, dont les réſultats furent toûjours ſemblables, je procédai d'une autre manière. Lorſque, par le changement ſubit de l'eau qui étoit à la temperature de l'air, en eau chaude, il ne s'étoit fait aucun changement dans la hauteur du point, je ſubſtituois, ſubitement auſſi, à cette eau chaude, de l'eau à la température de l'air; et alors le point ne changeoit pas non plus.

Il ne ſera pas inutile de dire ici, que j'avois un ſyphon à robinet dans ma bouteille, par lequel je retirois l'eau ſans mouvement; et que je l'y verſois au travers d'un

in through a long tunnel, by which means every thing remained quiet during the time of the experiment.

By comparing theſe two obſervations together, it ſhould ſeem, that when the igneous fluid is agitated for ſome time, in ſubſtances, the particles of which have not that ſtrong tendency towards each other which conſtitutes elaſticity, it produces ſome change in the arrangement of theſe particles, which hinders the compound from reſuming the ſame bulk upon the return of the ſame degree of heat; a change which there is not time to bring about, when the variations are ſudden, if there be at leaſt ſome elaſticity in the matter.

In ſupport of this we already know, that glaſs, the moſt elaſtic of known ſubſtances, is not ſubject to this irregularity, as I ſhall ſoon ſhew; that braſs, which is much leſs elaſtic, is ſenſibly affected by it; and that lead, the

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d'un entonnoir à long tuyau. Ainſi tout reſtoit tranquille pendant l'expérience.

Ces deux obſervations comparées ſemblent donc indiquer, que lorſque le fluide igné s'agite pendant quelque tems dans les matières dont les particules n'ont pas entr'elles cette forte tendance qui fait l'élaſticité, il produit dans l'arrangement de ces particules quelque changement, qui empêche les compoſés de reprendre exactement le même volume quand le même degré de chaleur revient: changement qui n'a pas le tems de s'effectuer, lorſque les variations ſont ſubites, s'il y a du moins quelque élaſticité dans la matière.

Nous avons déja pour fondement de cette idée, que le verre, la plus élaſtique des matières connues, n'eſt pas ſujet à cette irrégularité, comme je le montrerai bientôt; que le leton, bien moins élaſtique, l'a ſenſiblement, et que le plomb, le

the leaſt elaſtic of all metals, is ſtill more ſubject to it than braſs, as I have experienced in my Thermometer made of it. Beſides, this ſeems to have an abſolute dependance upon that general law, that the leſs elaſticity ſubſtances have, the leſs time is neceſſary to make them take the bent one means to give them: lead takes it inſtantaneouſly; glaſs never takes it at all. A ball of the crumb of bread, which one ſo eaſily faſhions in one's fingers, preſerves its form when it is thrown with violence againſt a plain ſurface; becauſe the fluid which conſtitutes its elaſticity has not time to eſcape. Wood, braſs, and ſteel, reſiſt, but in the end give way. This effect is probably the ſame with that which the *humor* produces in the bodies it penetrates, which by degrees likewiſe take the habit of their ſtate; and this ſo much the more as

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le moins élaſtique de tous les métaux, y eſt encore beaucoup plus ſujet que le léton, comme je l'ai vu dans mon Thermomètre fait de cette matière. D'ailleurs cela me paroit rentrer abſolument dans ce phénomène ſi général; que moins les matières ont d'élaſticité, moins il faut de tems pour leur faire contracter le plis qu'on leur donne: le plomb le prend dans l'inſtant; le verre ne le prend jamais; une balle de mie de pain, qu'on façonne ſi aiſément entre ſes doigts, conſerve ſa forme en bondiſſant, quand on la jette avec violence contre une ſurface unie, parce que le fluide qui fait ſon élaſticité n'a pas le tems de s'échapper; le bois, le léton, l'acier, réſiſtent, mais enfin ſe rendent. Cet effet eſt probablement le même que celui que produit l'*humor* dans les corps qu'elle pénètre, auxquels nous voyons auſſi prendre peu à peu l'habitude de leur état, et d'autant plus qu'ils

as they are leſs elaſtic: this is what made me chuſe to make my Hygrometer of ivory, as I have already ſaid in my paper on that ſubject.

If this conjecture upon the effects of heat in bodies which have little elaſticity be grounded, as I think it is, clocks would gain ſome more regularity, if in the compoſition of their pendulum, glaſs were uſed inſtead of ſteel, and bell-metal inſtead of braſs; for theſe two ſubſtances being the moſt elaſtic we are acquainted with, by uniting them we ſhould be much ſurer of preſerving the ſame length of the pendulum in the variations of the heat.

There would be another ſmall advantage in making uſe of theſe ſubſtances, which is, that bell or mirrour metal, having the ſame expanſibility as braſs, as Mr. SMEATON has demonſtrated by his experiments, and glaſs having

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qu'ils ſont moins élaſtiques. C'eſt ce qui m'a fait choiſir l'yvoire pour l'Hygromètre, comme je l'ai dit dans mon mémoire ſur cet inſtrument.

Si cette conjecture ſur les effets de la chaleur dans les corps peu élaſtiques, eſt fondée, comme elle me paroit l'être, il y auroit quelque choſe à gagner pour la régularité des pendules, à employer le verre au lieu de l'acier, et le métal de cloche en place du léton, dans la compoſition de la verge de leur pendule. Car ces deux matières étant les plus élaſtiques que nous connoiſſions, nous ſerions bien plus ſûrs de conſerver par leur combinaiſon, une même longueur au pendule dans les variations de la chaleur.

On auroit encore ce petit avantage en employant ces matières, que le métal de cloche ou celui des miroirs ayant la même expanſibilité que le léton, comme Mr. SMEATON l'a trouvé par ſes expériences, et le verre en ayant moins que l'acier,

having leſs than ſteel, the grid-iron would be ſhorter or more ſimple.

The proportion of the dilatations of braſs and ſteel, being only as five to three, it is impoſſible to compenſate the dilatation of the ſteel by a ſingle rod of braſs; for as there muſt be a ſecond rod of ſteel to go downwards again, there will be two lengths of ſteel for one of braſs, for which a proportion in the dilatation of ſix to three, or two to one, would not even ſuffice. One is therefore obliged to have a ſecond rod of braſs, which goes upwards, by which means the grid-iron comes to be compoſed of nine rods, the two aſcending pairs of which are braſs. Thus then the lens, which is always very heavy, is born by the top of one of the pairs of the rods, which, through the medium of a pair of rods of ſteel, are themſelves ſupported on the top of the other pair of the rods of

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l'acier, on auroit une grille moins longue ou plus ſimple.

Le rapport des dilatations du léton et de l'acier étant ſeulement de 5 à 3, il eſt impoſſible de compenſer la dilatation de l'acier par un ſeul retour du léton de bas en haut. Car comme il faut enſuite une autre branche d'acier qui redeſcende pour porter la lentille, on a deux longueurs d'acier pour une de léton; à quoi un rapport des dilatations de 6 à 3 ou 2 à 1 ne pourroit pas même ſuffire: je ne m'arrêterai pas à le montrer. On eſt donc obligé de faire remonter une ſeconde fois le léton; et c'eſt par cette raiſon que la grille eſt compoſée de 9 branches, dont 2 paires montantes ſont de léton. Ainſi la lentille, qui eſt toûjours d'un aſſez grand poids, eſt portée par le haut de l'une des paires, qui, elle même, par l'entremiſe d'une paire de branches d'acier, pèſe ſur le haut de l'autre paire de branches de léton.

of brafs. Now there muft be fome flexion in thefe rods of brafs during the ofcillations of the pendulum, in which the lens acquires a fmall centrifugal force from the end of one vibration to the other, and thus weighs differently upon the rods. Befides, this neceffity of turning back twice, increafes the total length of the rods, and confequently all the caufes of irregularity. On the contrary, by making ufe of bell-metal and glafs, the dilatations of which are as feven to three, a fingle pair of rods of the former, will be fufficient to compenfate the dilatations of the glafs rods; confequently the gridiron may either be fhorter, or made up only of five rods, a pair of glafs ones defcending on the outfide, a pair of bell-metal ones afcending on the infide, and a glafs rod defcending at the center, and carrying the lens. This

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léton. Or il doit y avoir quelque flexion dans ces branches de léton par les ofcillations du pendule; la lentille acquerant un peu de force centrifuge d'une extrémité à l'autre de chaque excurfion, et pefant ainfi différemment fur les branches. D'ailleurs cette néceffité de retourner deux fois en arrière, augmente la longueur totale des branches, et par conféquent elle augmente toutes les caufes d'irrégularité. Aulieu qu'en employant le métal de cloche et le verre, dont les dilatations font comme 7 à 3, une feule paire de branches du premier, fuffira pour compenfer la dilatation des branches de verre. La grille pourra donc être ou plus courte, ou compofée feulement de 5 branches: une paire de verre defcendantes à l'extérieur, une paire de métal de cloche montantes en dedans, et une branche de verre defcendante au centre pour porter la lentille.

This diminution of the total length of the rods, and that of the flexion, muſt contribute to increaſe the regularity of the pendulum, independently of the greater regularity of the expanſion of the ſubſtances themſelves.

As I have begun this digreſſion upon an object ſo eſſential to clock-making, I ſhall obſerve farther, that the experiments I am ſpeaking of are applicable to it in another reſpect, which ſeems to me of ſome importance. Subſtances of the ſame denomination are not homogeneous enough for us to conclude, that what has been found in the one, with reſpect to their very delicate properties, will always be exactly the ſame in the other: and that, for inſtance, the lengthening of a certain rod of braſs by heat, is an indication that every rod of braſs will have preciſely the ſame lengthening. My opinion therefore

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tille. Cette diminution de longueur totale des branches, et celle de la flexion, ne peuvent qu' augmenter la régularité du pendule, indépendamment de la plus grande régularité de l'expanſion des matières mêmes.

Puiſque j'ai commencé une digreſſion ſur cet objet eſſentiel à l'horlogerie, je ferai encore remarquer ici, que les expériences dont je parle lui ſont applicables à un autre égard qui me paroit de quelque importance. Les matières qui portent le même nom, ne ſont pas aſſez homogènes, pour que dans leurs propriétés délicates nous puiſſions conclure, de ce que nous avons trouvé dans une, qu'il en ſera exactement de même de toutes celles qui ont la même dénomination; et qu'ainſi par exemple, l'allongement d'une certaine branche de léton par la chaleur, nous indique l'allongement de tout léton par cette cauſe. Je crois donc

therefore is, that it would be of ſome advantage, if, upon every pendulum intended for very delicate obſervations, there were made the experiment of the comparative lengthening of the ſubſtances which compoſe it; marking the center of oſcillation upon the lens by the known methods, and pointing a Microſcope upon this center whilſt the heat is made to vary.

By uſing for theſe pendulums glaſs and bell-metal, the firſt of which is not at all affected by water, and the other very little (which little might even be prevented by varniſhing it) it would be eaſy to obſerve the effects of the heat upon them in water, by means of a tin veſſel, having a glaſs facing the center of oſcillation. By pouring water of different temperatures into this veſſel, whilſt the Microſcope remains pointed to that center, one would diſcover, in the ſureſt and ſhorteſt manner, that combination

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donc qu'on obtiendroit plus d'exactitude ſi l'on faiſoit immédiatement ſur chaque pendule deſtiné à des obſervations délicates, l'expérience de l'allongement comparatif des matières qui le compoſent, en marquant ſur la lentille, par les méthodes connues, le centre d'oſcillation, et pointant un Microſcope ſur ce centre, tandis qu'on feroit varier la chaleur.

En employant à ces pendules le verre et le métal de cloche, dont le prémier n'eſt point affecté par l'eau, et l'autre l'eſt très peu, et pourroit même être verniſſé, il feroit aiſé encore d'y obſerver les effets de la chaleur dans l'eau, au moyen d'une caiſſe de fer blanc, qui auroit une glace vis à vis du centre d'oſcillation. En y verſant de l'eau à diverſes températures tandis que le Microſcope feroit pointé ſur ce centre, on trouveroit de la manière la plus ſure et la plus courte

nation of the two ſubſtances, which would preſerve the ſame length to the pendulum between two determined temperatures.

To make this ſtill eaſier, it would be poſſible to conſtruct the grid-iron in ſuch a manner as that its corrective rods being fixed by ſcrews, the proportions of their lengths might be changed, till the bell-metal perfectly compenſate for the lengthening of the glaſs between the two fixed temperatures, of which I ſhall ſpeak hereafter. For there would be nothing to correct by this method, but the difference of expanſibilities of the ſubſtances employed, comparatively with the mean expanſibility of ſubſtances of the ſame denomination, which would have been firſt diſcovered by experiments made for that purpoſe. I ſhall reſume hereafter this correction of the pendulum,

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courte, la combinaiſon des deux matières qui conſerveroit ſenſiblement au pendule, une même longueur dans des variations déterminées de température.

Pour rendre ce moyen plus commode, on pourroit aiſément conſtruire la grille de manière, que ſes branches correctives étant retenues dans leurs traverſes par des vis, on pût changer les rapports de leurs longueurs, juſqu'à ce que le métal de cloche compenſât parfaitement l'allongement du verre entre les deux températures fixes, dont je parlerai ci-après. Car on n'auroit à corriger par cette méthode, que les différences d'expanſibilité des matières employées, comparativement à l'expanſibilité moyenne des matières de même nom, qu'on auroit trouvée prémièrement par des expériences faites à ce deſſein. Je reviendrai dans la ſuite à cette

dulum, in order to confider it in a point of view ftill more important.

I now return to the comparative dilatations of glafs and brafs; I mean to the experiments which I made in order to determine at what heighth of the lamella of brafs the point was, which fhould remain unmoved by the variations of heat.

Thefe experiments, notwithftanding all my pains, turned our furprizingly irregular; and it was neceffary to make a great number of them, to arrive at any degree of probability. In the firft place, the duration of the operations made the brafs take, what I have named, the habit of its ftate, which prevented it from returning exactly to that in which it was at the fame degree of heat:

à cette correction du pendule, pour la confidérer fous une face plus importante encore.

Je reviens aux dilatations comparatives du verre et du léton; c'eft à dire à ces expériences par lefquelles je cherchois à déterminer à quelle hauteur fur la lame de léton fe trouveroit le point qui refteroit immobile par les variations de la chaleur.

Malgré tous les foins que je pris, ces expériences fe trouvèrent d'une irrégularité furprenante; et il fallut en faire un bien grand nombre pour arriver à des réfultats un peu probables. D'abord, la longueur des opérations faifoit toujours prendre au léton ce que j'ai appellé l'habitude de fon état; ce qui l'empêchoit de revenir exactement à celui où il étoit auparavant par le même degré de chaleur.

heat[d]: the quicker, however, the returns, the more regular they were; it will be therefore only from ſuch as were quick that I ſhall hereafter deduce my concluſions.

Another difficulty I met with in my experiments aroſe from the difference of the effect of the different changes of the temperature. When I ſuddenly changed the temperature of the water from 10° to 70° of the Thermometer to which I have given the name of *common* in my work upon the Modifications of the Atmoſphere (which anſwer to $54°\frac{1}{2}$ and $189\frac{1}{2}$ of FAHRENHEIT) a ſomewhat leſs length of braſs was wanted to compenſate the dilatation of the glaſs, than when I increaſed the heat leſs; as,

(*d*) Since the writing of this I have been told, that the ſame phenomenon is obſerved in the correcting Thermometer of watches; and that it produces irregularities which are in an increaſing progreſſion.

for

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leur (*d*). Quand ces retours étoient prompts ils étoient plus réguliers. Ce ne ſera donc que de celles de mes obſervations qui furent promptes, que je tirerai ci-après les réſultats probables.

Un autre embarras que j'éprouvai dans ces expériences, provint de la différence d'effet des changemens différens de température. Quand je portois tout à coup la température de mon eau de 10° à 70° du Thermomètre que j'ai appellé *commun* dans mon ouvrage ſur les Modifications de l'Atmoſphére, et qui correſpondent à $54°\frac{1}{2}$ et $189°\frac{1}{2}$ de FAHRENHEIT, il me failloit une longueur un peu moindre de léton pour compenſer la dilatation du verre, que lorſque j'augmentois moins la

(*d*) J'ai appris depuis que j'ai écrit ceci, qu'on remarque ce phénomène dans les Thermomètres correcteurs des montres, et qu'il y cauſe des irrégularités croiſſantes.

chaleur,

for instance, from 10° to 40°, to which, however, I limited myself for a reason I will soon mention.

The more I approached the heat of boiling water in warming the water in my experiment, the nearer I came to the proportion found by Mr. SMEATON between the lengthenings of these two bodies by heat; which he has fixed between glass and brass wires (which answers to my milled brass) as 100 to 232. I had a point upon my lamella of brass which answered to this proportion; that is, the lengths of the brass and glass were at that point in an inverse ratio of these numbers; and this point did not change sensibly when, instead of water of 10°, I substituted water which I poured boiling into my bottle, and which brought the Thermometer to 74° or 76°. But when I only encreased the heat to 40°, this length of brass

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chaleur, comme par exemple de 10° à 40°, à quoi je me bornai par la raison que j'indiquerai.

Plus j'approchois de la chaleur de l'eau bouillante en échauffant mon eau, plus je me rapprochois du rapport qu'a trouvé Mr. SMEATON entre les allongemens de ces deux corps par la chaleur, qu'il a fixé, entre le verre et du léton tiré à la filière (qui répond à mon léton laminé) comme 100 à 232. J'avois un point sur ma lame de léton, qui correspondoit à ce rapport; c'est à dire que les longueurs du léton et du verre y étoient en raison inverse de ces nombres; et ce point ne changeoit pas sensiblement de hauteur, quand je substituois à l'eau de 10°, de l'eau que je versois bouillante dans ma bouteille, et qui portoit le Thermomètre à 74° ou 76°. Mais lorsque je n'augmentois la chaleur que jusqu'à 40°, cette

for inſtance, from 10° to 40°, to which, however, I limited myſelf for a reaſon I will ſoon mention.

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brass was not sufficient; the point insensibly sunk: I speak of the greatest number of experiments, for there were some contrary ones.

These uncertainties recalled my attention to the particular object on account of which I had undertaken my experiments; I mean my new Hygrometer, on which, in order to compensate the effect of heat upon glass, it was necessary to produce this compensation in the natural variations of the temperature of the air. I therefore confined myself within these limits; not indeed precisely within the same temperatures, which seemed too difficult, though it would have been better; but between temperatures nearly equally different.

I will not give a minute account of these experiments; be it sufficient to say, that from the difference of temperature

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cette longueur de léton ne suffisoit pas; le point baissoit sensiblement. Je parle du plus grand nombre des expériences; car il y en eut quelquefois de contraires.

Voyant ces incertitudes, je tournai mon attention sur l'objet particulier pour lequel j'avois entrepris ces expériences; c'est-à-dire mon nouvel Hygromètre, où, pour compenser l'effet de la chaleur sur le verre, je n'avois besoin de produire cette compensation que dans les variations naturelles de la température de l'air. Je me renfermai donc dans cet espace; non précisément entre les mêmes températures, ce qui me parut trop difficile, quoique c'eût été le mieux; mais entre des températures à peu près également différentes.

Je ne rendrai pas compte ici du détail de ces expériences; il suffira de dire, que par la différence de température entre 10° et 40° de mon Thermomètre, le rapport

rature between 10° and 40° of my Thermometer, the mean proportions of the dilatations of the braſs and glaſs were as 21 to 10; conſequently, I had need of a length of braſs which ſhould be to that of the glaſs in my Hygrometer as 10 to 21, in order to make up for the dilatability of its frame; whereas it needed to have been from 10 to 23 in the tranſition from water in ice to boiling water.

I do not give this latter proportion as being equally to be depended upon as the other; but it is ſufficient that there be really a difference between the two, to ground the general reflexions I ſhall make, after having previouſly noticed another difference of the ſame kind which I have found by means of this Pyrometer.

But before I mention theſe new experiments, I will beſtow another moment upon thoſe which relate to the compa-

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rapport moyen des dilatations du léton et du verre fut comme 21 à 10; et que par conſéquent j'avois beſoin d'une longueur de léton qui fût à celle du verre dans mon Hygromètre, comme 10 à 21, pour compenſer la dilatabilité de ſa monture; au lieu qu'il l'auroit fallu de 10 à 23 dans le paſſage de l'eau dans la glace à l'eau bouillante.

Je ne donne pas ce dernier rapport comme auſſi ſûr que l'autre; mais il ſuffit qu'il y aît certainement une différence entre les deux, pour fonder les réflexions générales aux quelles je viendrai, après avoir expliqué une autre différence du même genre que j'ai trouvée au moyen de ce Pyromètre.

Mais avant que de rapporter ces nouvelles expériences, je m'arrêterai encore un moment ſur celles qui regardent les expanſibilités comparatives ou les rapport entre

comparative expansibilities, or the proportion there is between the expansibilities of bodies, which seem to me by this method to be reduced to an operation as simple as it is conclusive.

First, with respect to the frame of my Pyrometer, however rude it be, I have hitherto discovered no defect in it. A point of suspension of the rods which preserves throughout the experiment its relative position to the Microscope, is the easiest thing to obtain by means of this deal frame, which is not sensibly affected by the small changes in the air, and is exposed to no other variation: and this is a great point gained when it is considered, that hitherto frames had a sensible effect upon the indications of the Pyrometers; and that all that could be done, was to endeavour to obviate this influence.

Again,

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entre les expansibilités des corps, qui me paroissent reduites par cette route à une opération aussi simple que sure.

D'abord quant à la monture de mon Pyromètre, quelque grossière qu'elle soit, je n'y ai trouvé jusqu'à present aucun défaut. Un point de suspension des branches, qui conserve pendant l'expérience sa position relative à celle du Microscope, est la chose la plus aisée à obtenir par cette monture de sapin; les petits changemens de l'air ne l'affectant point sensiblement, et n'étant exposée à aucune autre cause de variation. Or c'est là un grand point d'obtenu, quand on considère que jusqu' ici les montures influoient toûjours essentiellement sur les indications des Pyromètres, et que tout ce qu'on pouvoit faire, étoit de chercher à en corriger les effets.

 Ensuite,

Again, meaſurement will itſelf always be an occaſion of error more or leſs conſiderable in phyſics, becauſe our Micrometers are all imperfect. It is true, indeed, that we are daily improving them, and with great reaſon, ſince we are obliged to meaſure almoſt every where; but it is not leſs true, that, not to be obliged to meaſure, is a great additional ſecurity. Now by this method there is no neceſſity for meaſuring: all one wants is, to find a point upon the rods of different ſubſtances thus ſuſpended, which neither riſes nor falls when the temperature of the water is changed; and it is ſufficient for this purpoſe, that the Microſcope do not vary during the obſervation.

As to the meaſure of the lengths of the rods, whoſe expanſions compenſate each other at this point, all poſſible errors in this reſpect are hitherto of no import. We

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Enſuite, meſurer, ſera toûjours en phyſique une occaſion d'erreurs plus ou moins grande; parcequc nos Micromètres ſont tous imparfaits. On les perfectionne tous les jours d'avantage; et il le faut bien, puiſque nous ſommes obligés de meſurer preſque partout; mais il n'en eſt pas moins vrai, qu'être diſpenſé de meſurer eſt une grande ſureté de plus. Or on l'eſt totalement par cette méthode. Trouver ſur l'une des branches de matières différentes ainſi ſuſpendues, un point qui ne s'élève ni ne s'abaiſſe en changeant la température de l'eau, eſt tout ce dont on a beſoin; et il ſuffit pour cela que le Microſcope ne varie pas tandis qu'on obſerve.

Quant à la meſure de la longueur des branches dont les expanſions ſe compenſent à ce point, c'eſt un objet ſur lequel les erreurs poſſibles ne ſont encore d'aucune

We are very far from being able to perceive, in the quantities of expansions, those differences which might arise from the imperfection of this measure.

One may take therefore a rod of some substance, glass for instance, and put at the end of it a convenient clasp for holding other rods of different substances: thus, by separately comparing their expansions with that of the rod of glass, by the position of the immoveable point, one will obtain the proportion of their expansibility with that of glass, and consequently with each other.

Nor may solids only be subjected to these experiments, but fluids also: for by enclosing them in a cylindrical tube of glass, the expansibility of which is known, they will be as rods, which may be thus submitted to the same experi-

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cune conséquence. Nous sommes bien loin de pouvoir reconnoitre dans les quantités des expansions, les différences qui pourroient résulter de l'imperfection de cette mesure.

On pourra donc avoir une branche d'une certaine matière, de verre par exemple, à l'un des bouts de laquelle on aura ajusté une pince commode, pour y fixer d'autres branches de diverses matières: et comparant ainsi séparément leur expansion avec celle de la branche de verre, par la position du point immobile, on aura les rapports de leur expansibilité avec celle du verre, et par conséquent entre elles.

Ce ne sera pas seulement les solides qu'on pourra soumettre à ces expériences; mais les fluides. Car en les renfermant dans un tuyau de verre cylindrique, dont l'expansibilité soit connue, on en fera comme des branches, qu'on pourra ainsi soumettre.

experiments, by means of a ſmall opaque body floating at the top of them, and to which the Microſcope may be pointed. I do not, however, inſiſt upon this application of the machine; ſince the expanſion of liquids may be obſerved in vaſes, the dimenſions of which are known, by joining cylindrical tubes to them, which render their expanſions much more ſenſible. I ſhall only obſerve, that from the knowledge of the dilatability of the tube, one ſhould not reduce its capacity to what it would be if the glaſs did not dilate itſelf, as is done with reſpect to vaſes in order to know the true change in the volume of liquids contained in them; but that, on the contrary, we ſhould here ſuppoſe the dilatation of glaſs greater than it is, and as it would be if it had the ſame expanſibility as the liquid; and then diminiſh according to that proportion

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ſoumettre aux mêmes expériences, en faiſant flotter un petit corps opaque à leur ſurface, pour y pointer le Microſcope. Cependant je n'inſiſte pas ſur cette application de la machine; parceque les expanſions des liquides peuvent être obſervées dans des vaſes dont on connoit la capacité, en y joignant des tubes cylindriques; ce qui rend leur expanſion beaucoup plus ſenſible. Je remarquerai donc ſeulement; qu'il ne faudroit pas, d'après la connoiſſance de la dilatabilité de la matière du tuyau, réduire par le calcul ſa capacité, à ce qu'elle feroit ſi le verre ne ſe dilatoit point, comme on le fait à l'égard des vaſes pour connoitre le changement réel du volume des liquides qu'ils contiennent; mais qu'au contraire il faudroit ici ſuppoſer cette capacité plus grande, en la portant à ce qu'elle feroit, ſi le verre avoit la même expanſibilité que le liquide; et diminuer dans cette proportion l'allongement

portion the obſerved lengthening of the latter: for in comparing the expanſion of a fluid with that of a ſolid, we muſt take notice, that we meaſure the change of bulk of the latter, according to one of its three dimenſions only, for which reaſon the fluid muſt be brought to the ſame predicament.

Air might alſo be ſubjected to theſe experiments by encloſing it in a glaſs tube by means of a ſmall column of quickſilver. But I cannot help being of opinion, that all experiments made on air encloſed, will be found inaccurate, when applied to the air in general. The expanſibility of air by heat varies exceedingly, according to its greater or leſs degree of humidity; and I know from experience, how difficult it is to encloſe in a tube, air of a determinate dryneſs: but if it is more humid than its mean

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l'allongement obſervé de celui-ci. Car pour comparer l'expanſion d'un fluide à celle d'un ſolide, il faut avoir égard à ce que nous ne meſurons le changement de volume de ces derniers que ſuivant une ſeule de leurs trois dimenſions, et que par conſéquent il faut reduire le fluide au même cas.

On pourroit auſſi ſoumettre l'air à ces mêmes expériences, en le renfermant dans le tube de verre par une petite colonne de mercure. Mais je ne puis m'empécher de croire que les expériences ſur l'air renfermé ſeront toûjours inexactes quand on les appliquera à l'air en général. L'expanſibilité de l'air par la chaleur varie beaucoup, ſuivant qu'il eſt plus ou moins humide; et je ſais par expérience, qu'il eſt bien difficile de renfermer dans un tube, de l'air d'une ſécherеſſe déterminée.

mean ſtate in the atmoſphere, its expanſibility by heat will be greater.

It is not impoſſible that this may be the reaſon why Colonel ROY and Sir GEORGE SHUCKBURGH found a greater expanſibility in the air encloſed in their Manometers than what I deduced from my obſervations in the open air. The bare difference there is between the air of London and that of the mountains of Switzerland may be ſufficient to account for this effect. I believe, indeed, that the differences of humidity will be cauſes of error in the Barometrical meaſures of heights, ſo long as theſe differences ſhall not enter into the formulæ; and it was this conſideration which firſt led me to think of an Hygrometer(e).

(e) I ſhall return to this object, and treat it more particularly, in a paper upon Refractions, of which I ſhall ſpeak hereafter.

As

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déterminée. Or s'il eſt plus humide que ſon état moyen dans l'atmoſphère, ſon expanſibilité par la chaleur ſera plus grande.

Il n'eſt pas impoſſible que ce ne ſoit là la raiſon pour laquelle Mr le Col. ROY et Mr le Chev. SHUCKBURGH ont trouvé à l'air renfermé dans leurs Manométres, une expanſibilité plus grande que celle que j'ai déduite de mes obſervations dans l'air libre; la difference peut-être de l'air de Londres et de celui qui environne les montagnes de la Suiſſe, peut produire cet effet: je crois même que les différences d'humidité feront des cauſes d'écarts dans les meſures Barométriques des hauteurs, tant qu'on ne pourra pas faire entrer ces différences dans les formules: c'eſt le prémier motif qui m'a fait chercher un Hygromètre (e).

(e) Je reviendrai à cet objet pour le traiter plus particuliérement, dans un mémoire ſur les Réfractions dont je parlerai ci-après.

Quant

As to the differences of the conclusions drawn from these gentlemen's experiments and mine made in the open air, though they be conformable to the above-mentioned difference, they may yet arise from another cause. I always observed the temperature of the air with my Thermometer in open air, and in the Sun when it shone; whereas they observed in the shade. As often then as I found the air warmer in the Sun, than I should have found it in the shade, which was almost always the case, especially in the plain, I did not stand in need of as great a correction as those gentlemen for each degree of the Thermometer; since in the same circumstances the degrees were more numerous in my observations than in theirs, and consequently, with a less correction for each degree, my whole correction was equal to theirs. I will add, that I did

Quant aux différences des résultats des expériences de ces Messieurs et des miennes dans l'air libre, quoique conformes à la différence precédente, elles pourroient bien venir d'une autre cause. J'observois toûjours la température de l'air avec mon Thermomètre à boule isolée, en plein air, et au soleil quand il luisoit; au lieu qu'ils l'observoient à l'ombre. Si donc je trouvois l'air plus chaud au soleil, que je ne l'aurois trouvé à l'ombre, ce qui étoit presque toûjours le cas, surtout à la plaine, je n'avois pas besoin d'une si grande correction que ces Messieurs pour chaque degré du Thermomètre, puisqu'ils étoient plus nombreux dans mes observations que dans les leurs par les mêmes circonstances; et qu'ainsi, avec une moindre correction pour chaque dégré, j'avois une correction totale aussi grande. J'ajouterai, que je n'ai pas trouvé que les rayons directs du soleil

did not find that the direct rays of the Sun heated irregularly the glass of the ball of a Thermometer when clean, which may be easily seen by looking at the experiment mentioned in p. 56, 57. of the second volume of my work: consequently, when these direct rays act upon the air, it is a cause of heat which should not be neglected.

I am still therefore of opinion, that it is better to observe the Thermometer in the Sun than in the shade; and that the correction for the heat of the air may stand such as this method of observing requires it. There are always acting causes enough in the column of air weighing upon the inferior Barometer, which cannot be known in the superior station, for us not to neglect any of the ordinary causes which may be perceived.

It

soleil échauffassent irrégulièrement le verre bien net de la boule d'un Thermomètre; ce qu'on pourra voir aisément par l'expérience rapportée aux p. 56 et 57 du second volume de mon ouvrage: et ainsi quand ces rayons directs agissent sur l'air, c'est une cause de chaleur qui ne me semble pas devoir être négligée.

Je crois donc toûjours qu'il convient mieux d'observer le Thermomètre au soleil qu'à l'ombre, et de laisser la correction pour la chaleur de l'air proportionnée à cette méthode. Il reste toûjours assez de causes agissantes dans la colonne d'air qui pèse immédiatement sur le Baromètre inférieur, qu'on ne peut pas connoitre à la station supérieure, pour qu'on ne doive négliger aucune des causes communes qui sont saisissables.

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It is therefore probable, that had I obſerved in the ſame places as theſe gentlemen with my Barometer, expoſing at the ſame time my Thermometer to the Sun, I ſhould have found the real height as well as they, without changing my rule; which already appears, I think, from my having derived it from experiments made in the ſame place where Sir GEORGE SHUCKBURGH has made his principal obſervations.

I will only add, that if, in the different opportunities I have had of trying my rule ſince it is fixed, it had conſtantly given me the heights too ſmall, as thoſe gentlemen have found it, even conſidering what is above ſtated, I ſhould have ſuſpected with Colonel ROY, that I ought not to have taken out from the obſervations from which I have concluded my rule, thoſe which I made

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Il eſt donc aſſez probable, que ſi j'avois obſervé dans les mêmes lieux que ces Meſſieurs avec mon Baromètre, et en expoſant mon Thermomètre au ſoleil, j'aurois trouvé comme eux les hauteurs réelles, ſans changer ma règle: et c'eſt ce qui paroit déja, ce me ſemble, de ce que je l'ai conclue d'expériences faites dans le même lieu où Mr le Chev. SHUCKBURGH a fait ſes principales obſervations.

J'ajouterai ſeulement, que ſi dans les diverſes occaſions que j'ai eues d'éprouver cette règle depuis qu'elle eſt fixée, elle m'avoit donné conſtamment les hauteurs un peu trop petites, comme il reſulteroit des expériences de ces Meſſieurs, même en ayant égard aux conſidérations ci-deſſus, j'aurois ſoupçonné alors avec Mr le Col. ROY, que je n'aurois pas dû retrancher du nombre des obſervations dont j'ai tiré ma régle, celles que j'avois faites au lever du ſoleil, qui toutes donnent les

made at Sun-riſe; all which, according to this rule, give the heights too ſmall. For it would then appear, that it is owing to accident alone that the exceptions of this kind happen to be at that preciſe time of day; that they are deviations which are ſtill to be expected, till more circumſtances have been taken notice of in the obſervations, and new equations are introduced in the formula; and that having admitted the exceptions on the contrary ſide, I ought to have left thoſe in the bulk of my obſervations, before I deduced the mean laws from them, which would have brought me nigher to the concluſions drawn from Sir GEORGE SHUCKBURGH'S and Colonel ROY'S obſervations.

The late Mr. DE LA CONDAMINE, one of thoſe rare men who take an intereſt in the labours of their friends, was already of this opinion; and I ſhould have made uſe of it, had not my tables been already calculated. However,

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les hauteurs trop petites ſuivant cette règle. Car il paroitroit en ce cas, que ce n'eſt qu' accidentellement que les exceptions dans ce ſens là ſe rencontrent à ce moment du jour; que ce ſont des écarts auxquels on doit encore s'attendre, juſqu'à ce qu'on ait embraſſé plus de circonſtances dans les obſervations, et de nouvelles équations dans la formule; et qu'ayant admis les exceptions contraires, je devois laiſſer celles là dans l'enſemble de mes obſervations, avant d'en déduire les loix moyennes: ce qui m'auroit rapproché d'avantage des réſultats des obſervations de M^r le Col. ROY et de M^r le Chev. SHUCKBURGH.

Feu M^r DE LA CONDAMINE, l'un de ces hommes rares qui ſavent s'intéreſſer aux travaux de leurs amis, m'avoit déja fait faire cette réflexion; et j'y aurois eu

ever, I did not find afterwards any neceſſity for it by my own obſervations.

Here then is a new ſubject of inveſtigation, and conſequently thoſe gentlemen's obſervations are exceedingly intereſting, ſince they will engage natural philoſophers not to give up this object till it is entirely cleared up.

I return to my idea of encloſing bodies in tubes of glaſs, only to obſerve that it will be abſolutely neceſſary to make uſe of this method, in the experiments upon the expanſion of bodies affected by humidity, as well as by heat, for thoſe cannot be expoſed naked to the heat of the water. Woods, therefore, may be compared either with one another, or with metals, by encloſing them in glaſs tubes. Some difficulties I met with from the vapours which form themſelves in heated tubes; one of the

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eu egard ſi mes tables n'avoient été toutes calculées. Cependant dès lors je n'en ai pas apperçu le beſoin par mes propres obſervations.

Voila donc un nouvel objet d'examen; et par conſéquent les obſervations de ces Meſſieurs ſont très intéreſſantes; puis qu'elles engageront les phyſiciens à ne pas abandonner cet objet, juſqu'à ce qu'il ſoit éclairci.

Je reviens à l'idée de renfermer les corps dans des tubes de verre, pour ajouter ſeulement, qu'il ſera indiſpenſable d'employer ce moyen, lors qu'on voudra ſoumettre à ces expériences des corps que l'humidité affecte auſſi bien que la chaleur: car ceux là ne peuvent pas être expoſés nuds à la chaleur de l'eau. On pourra donc par exemple comparer les bois entr'eux ou avec les métaux, en les renfermant dans des tubes de verre. Quelques difficultés que j'ai éprouvées, à cauſe des vapeurs qui ſe forment dans les tubes échauffés, et qui ſont une de mes raiſons

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the reafons why I fufpect the experiments made upon air in Manometers, has obliged me to fufpend the experiments I had undertaken upon the expanfion of woods.

This method of finding the relative expanfibilities of bodies may eafily be turned into a method of finding their abfolute expanfibilities: for if one knows with certainty the expanfibility of the rod of glafs to which all the other bodies are compared; by means of that, one will come to the knowledge of the abfolute expanfibility of all thefe bodies.

The point then would be to give all poffible attention, and ufe all the refources of art, to the determining the expanfibility of this rod of glafs; and this one may hope to arrive at by this fame machine, as I fhall fhew by the following account of my firft trial.

Effay

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de fufpecter les expériences faites fur l'air dans les Manomètres, m'ont obligé de fufpendre celles que j'avois entreprifes fur l'expanfion des bois.

Cette méthode de trouver les expanfibilités rélatives des corps, peut encore être changée aifément en une méthode de trouver leurs expanfibilités abfolues. Car fi l'on connoit par exemple celle de la branche de verre à laquelle on comparera tous les autres corps, on connoitra par elle l'expanfibilité abfolue de tous ces corps.

Il ne s'agiroit donc que de concentrer fon attention, et toutes les reffources de l'art, fur la détermination de l'expanfibilité de cette branche de verre, à quoi l'on peut efpérer de réuffir avec cette même machine, comme je vais le montrer d'après un prémier effai.

Effai

Essay upon the measure of the absolute expansion of bodies by heat.

Though I did not at first intend to make use of my instrument in the measure of absolute expansions, I could not help making some experiments on this subject.

Besides the immoveable wire placed in the focus of the Microscope, I had desired Mr. RAMSDEN to put in another, moveable by a screw: I then began, first, by seeking the value of the parts of the Micrometer, in doing which the little scale I had traced on the lamella of brass was again of service to me. I had made it as exact as I possibly could; each of its divisions was the 400th part of a French foot. The divided part of it was three

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Essai sur la mesure des expansions absolues des corps par la chaleur.

Quoique je n'eusse pas intention d'abord d'employer ma machine à mesurer des expansions absolues, je ne laissai pas de tenter quelques expériences sur cet objet.

Outre le fil immobile placé au foyer du Microscope, j'avois demandé à Mr RAMSDEN d'en mettre un qui fût mobile, et conduit par une vis. Je cherchai donc d'abord la valeur des parties du Micromètre; à quoi me servit encore la petite échelle tracée sur ma lame de léton. Je l'avois faite aussi exactement qu'il m'avoit été possible, et ses parties étoient des 400mes du pied de France. Elle avoit

three French inches in length, confequently confifted of 100 of thefe parts, that were, or were fuppofed to be, equal.

Pointing then my Microfcope at firft upon one of the extremities of the fcale, the two wires coinciding, I brought the moveable wire to the next point, counting the turns of the fcrew: then conducting the immoveable wire from one part to another of the fcale, and bringing at every change the moveable wire to the point immediately following, I noted all thefe lengths of the parts meafured by the Micrometer, the fmall differences of which marked the imperfections of the fcale. The mean of thefe 100 meafures gave me 21,333 turns of the fcrew, for a part of my fcale, that is, for $\frac{1}{400}$th of a French foot. This, according to the proportion of 16 to 15 between this foot and the Englifh, a proportion exact enough

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avoit 3 pouces de France dans la portion divifée, et par conféquent 100 de ces parties égales, ou cenfées l'être.

Pointant donc d'abord mon Microfcope fur l'une des extrémités de l'échelle, tandis que les deux fils coïncidoient, j'amenai le fil mobile fur le point fuivant, en comptant les tours de la vis; puis conduifant le fil immobile de partie en partie de l'échelle, et amenant à chaque fois le fil mobile au point immédiatement fuivant, je notai toutes ces grandeurs des parties, mefurées par le Micromètre, dont les petites différences marquoient les imperfections de l'échelle. Le milieu entre ces 100 mefures me donna 21,333 tours de la vis, pour une partie de mon échelle, c'eft à dire pour $\frac{1}{400}$ du pied de France. Ce qui, dans le rapport de 16 à 15 de ce pied

enough for this meaſure (and perhaps very exact) makes twenty turns for $\frac{1}{400}$th of the Engliſh foot, or one turn for $\frac{1}{8000}$th of a foot. One could eaſily diſtinguiſh the effect of $\frac{1}{10}$th of a turn; conſequently, the inſtrument was ſenſible at $\frac{1}{80000}$th of a foot, or about $\frac{1}{7000}$th of an inch.

Knowing thus the value of the parts of my Micrometer, I undertook to meaſure the abſolute lengthening of my rod of glaſs, which was of 18 Engliſh inches; and from a mean of four experiments, the reſult of which differed very little, I found that my rod of glaſs had lengthened 7,5 turns of the ſcrew of my Micrometer from the heat of 10° of my Thermometer to that of 70°.

I ſhall not make any ſenſible error if I augment this number of turns in the proportion of 60 to 80, in order to obtain the total expanſion which would be made by the

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pied à celui d'Angleterre, aſſez exact pour un objet de cette nature (et peut-être très exact) fait 20 tour pour $\frac{1}{400}$ du pied Anglois, ou 1 tour pour $\frac{1}{8000}$ de pied. On pouvoit diſtinguer aiſément l'effet d'$\frac{1}{10}$ de tour, et par conſequent l'inſtrument étoit ſenſible à $\frac{1}{80000}$ de pied, ou environ $\frac{1}{7000}$ de pouce.

Connoiſſant la valeur des parties de mon Micromètre, j'entrepris de meſurer l'allongement abſolu de ma branche de verre, qui avoit 18 pouces anglois: et par un milieu entre quatre expériences, dont les réſultats différèrent très peu, je trouvai que, de la chaleur de 10° ſur mon Thermomètre à celle de 70°, ma branche de verre s'étoit allongée de 7,5 tours de la vis du Micromètre.

Je ne ferai pas une erreur ſenſible en augmentant ce nombre de tours dans le rapport de 60 à 80, pour avoir l'expanſion totale qui ſe feroit faite par le paſſage de l'eau dans la glace à l'eau bouillante, ſoit de 0 à 80°, malgré la conſidération de

the paſſage of water in ice to boiling water, that is, from 0 to 80° upon my Thermometer, notwithſtanding the conſideration of the different progreſs of quickſilver and glaſs in their expanſions by heat, which I ſhall ſpeak of hereafter; becauſe the two terms of the obſervation, 10° and 70°, are equi-diſtant from the two fixed points of the Thermometer. I ſhall have then a third part to add to the number of turns for the expanſion of 18 inches of glaſs paſſing from the heat of water in ice to that of boiling water, which will make 10 turns, or $6\frac{2}{3}$ for the expanſion of one foot.

One turn of the ſcrew being equal to $\frac{1}{8000}$th of a foot, $6\frac{2}{3}$ make $\frac{6\frac{2}{3}}{8000} = \frac{1}{1200}$th of a foot $= \frac{1}{100}$th of an inch in one foot. Now this is preciſely what had been found by Mr. SMEATON. However, this ſingular conformity may be

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de la différence des marches du mercure et du verre dans leurs expanſions par la chaleur, dont je parlerai ci-après: parce que les deux termes de l'obſervation, qui ſont 10° et 70°, ſe trouvent à égale diſtance de ces deux points fixes du Thermomètre. J'aurai donc un tiers à ajouter au nombre des tours, pour l'expanſion de 18 pouces de verre, paſſans de la chaleur de l'eau dans la glace à celle de l'eau bouillante; ce qui fera 10 tours, ou $6\frac{2}{3}$ pour l'expanſion d'1 pied.

Un tour du Micromètre étant égal à $\frac{1}{8000}$ de pied, $6\frac{2}{3}$ font $\frac{6\frac{2}{3}}{8000} = \frac{1}{1200}$ de pied $= \frac{1}{100}$ de pouce dans 1 pied. Et voila préciſément ce que Mr SMEATON avoit trouvé par ſes expériences. Cependant cette conformité ſingulière pourroit bien

be only accidental; for I do not believe that all glaſſes have an equal dilatability by heat. Their dilatability often appears different when they are ſoldered; for it is no doubt owing to that, that the parts which are united when they are melted, often ſeparate when they grow cold, which does not happen when the glaſs is exactly the ſame. It is poſſible, therefore, that this apparently exact conformity was occaſioned by ſome compenſation, rather than by real exactneſs.

I ſaid before, that the irregularities I obſerved, when the glaſs and the braſs were combined, were not to be attributed to the glaſs, and here is a proof of it. When I had adjuſted the immoveable wire of my Microſcope to a ſharp point which terminated my rod of glaſs, the water being at the temperature of 10° of my Thermo-

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bien n'être qu'accidentelle; car je ne crois pas que les différens verres ayent tous une égale dilatabilité par la chaleur: on ne voit que trop ſouvent quand on les ſoude, que leurs dilatabilités peuvent être différentes; car c'eſt ſans doute par là, que les parties réunies quand elles ſont fondues, ſe ſéparent quelquefois en ſe refroidiſſant; ce qui n'arrive pas quand c'eſt exactement le même verre. Il ſe pourroit donc que cette exacte conformité apparente vînt de quelque compenſation plutôt que d'une exactitude réelle.

J'ai dit ci-deſſus que les irrégularités que je remarquois lorſque le verre et le léton étoient combinés, ne devoient pas être attribuées au verre; et en voici la preuve. Lorſque j'avois ajuſté le fil immobile de mon Microſcope ſur une pointe aigue qui terminoit ma branche de verre, tandis que l'eau étoit à la température

Thermometer, and that, after having heated it to 70°, I brought it back gradually to 10°, the point either returned exactly to the wire, or so near to it that I could draw no conclusion, from the small difference, against the regularity of the return of the glass to its same length in the same temperature. In one of the four experiments it returned exactly; in a second, I wanted light indeed to observe this last point, but I could judge from the preceding steps that it would be exact; in the others, the differences would have seemed to indicate, that the glass had retained some part of its lengthening; but the quantity was so small, that even when it was real, it might be considered as null in practice.

Glass consequently is the fittest substance to be made use of as the standard of comparison in experiments upon the comparative dilatabilities of bodies; since whatever

pérature de 10° de mon Thermomètre, et qu'après l'avoir échauffée à 70° je la ramenois peu à peu à 10°, la pointe revenoit exactement au fil, ou si près, que je n'ai pu en tirer aucune conséquence contre la régularité du retour du verre à sa même longueur dans la même température. Dans une des quatre expériences il y revint exactement; dans un autre le jour me manqua pour observer ce dernier point, mais je pus juger par les pas précédens, qu'il seroit juste; et dans les deux autres les différences auroient indiqué que le verre aussi avoit conservé un peu de son allongement: mais la quantité étoit si petite, que lors même qu'elle seroit réelle, on pourroit la regarder comme nulle dans la pratique.

Le verre est donc la matière la plus propre à servir de terme de comparaison dans les expériences sur les dilatabilités comparatives des corps; puisque les irrégularités

ever irregularities there might be in the obſervations, they would certainly ariſe only from the bodies that may be compared to it, and might for that reaſon be more eaſily aſcertained and determined. It has even another uſeful property for ſuch a purpoſe, and that is, its being one of the leaſt dilatable of all bodies, from which it would almoſt always happen that it ſhould be the rod of glaſs which would be fixed, the other being ſhorter; which would prevent making any changes in the apparatus.

Glaſs, as I have ſaid before, would likewiſe be an uſeful ſubſtance for the pendulum; ſince one might depend upon the conſtancy of the progreſs of its variations by heat. It is true, indeed, that its fragility would be an objection to uſing it in common clocks; but the aſtronomer, accuſtomed

larités qu'on appercevroit dans les obſervations, ne viendroient ſurement que des
corps qui lui ſeroient comparés, et pourroient être par là plus aiſément con-
ſtatées et déterminées. Il a même encore pour cet uſage une autre propriété
utile; c'eſt d'être un des moins dilatables des corps: par là il arriveroit preſque
toûjours que ce ſeroit la branche de verre qui ſeroit fixée, l'autre étant plus
courte; ce qui épargneroit des changemens dans l'appareil.

Le verre ſeroit encore, comme je l'ai déja dit, une matière précieuſe pour le
pendule; puis qu'on pourroit compter ſur la conſtance de ſa marche dans les va-
riations de la chaleur. Sa fragilité ſeroit ſans doute une objection pour les pen-
dules

accustomed to respect his clock as well as all his other instruments, would not be prevented by this consideration.

This regularity of the returns of glass to the same length by the same temperature in my four experiments, is likewise a proof of the exactness of the instrument; and if the value of the parts of the Micrometer was well ascertained, one might be sure of the absolute expansion of the glass I made use of.

I dare not yet be positive that this is so, because the part of the screw which measured these expansions is not the same as that which measured the parts of my scale. But for an experiment which it should be necessary to make only once, it would be easy to measure the expansions of the glass by many parts of the screw, in the intervals of those turns which had served to measure the parts of the scale,

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dules ordinaires; mais l'Astronome, accoutumé à respecter sa pendule comme tous ses autres instrumens, ne sera pas arrêté par cette considération.

Cette régularité des retours du verre à la même longueur par la même température dans mes quatre expériences, est aussi une preuve de l'exactitude de l'instrument. Et si la valeur des parties du Micromètre étoit bien déterminée, on pourroit être sûr de l'expansion absolue du verre que j'employai.

Je ne puis pas l'assurer encore, parce que la partie de la vis qui mesura ces expansions, n'est pas la même que celle qui mesuroit les parties de mon échelle. Mais il seroit aisé, pour une expérience qu'on ne seroit obligé de faire qu'une fois, de mesurer les expansions du verre par plusieurs parties de la vis, dans l'intervalle de ses tours qui auroit servi à mesurer les parties de l'echelle, pour prendre

ſcale, in order, if the reſults ſhould happen to be different, to take a mean of them. In a word, for this one meaſure one might uſe all the precautions that are not grudged in a fundamental experiment, though one is apt to neglect them in common uſe.

The expanſion of a certain rod of glaſs might therefore be thus determined; and by fixing to it afterwards any other ſubſtance, in the manner which I have explained, one would have, by means of the immoveable wire alone, their abſolute expanſibility, free from any ſenſible error occaſioned from the inſtrument.

Notwithſtanding that the expanſions of the glaſs were regular in my experiments, they did not obſerve the ſame progreſs as my Thermometer in their degrees. Thoſe of the glaſs were always increaſing, or its condenſations decreaſing,

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prendre enſuite le milieu entre les réſultats, s'ils étoient différens. En un mot on pourroit prendre dans cette meſure unique, toutes les précautions qu'on ne regrette pas dans une expérience fondamentale, mais qu'on néglige ſi aiſément dans l'uſage ordinaire.

On détermineroit donc ainſi l'expanſion d'une certaine branche de verre, d'après laquelle, en attachant enſuite à cette branche toute autre matière comme je l'ai expliqué, on auroit, par le fil immobile ſeul, leur expanſibilité abſolue exempte de toute erreur ſenſible provenant de l'inſtrument.

Quoique les expanſions du verre ſe trouvaſſent régulières dans mes expériences, elles ne ſuivirent pas la marche du Thermomètre dans leurs degrés: celles du verre furent toûjours croiſſantes, ou ſes condenſations décroiſſantes, compa-

decreasing, comparatively, with those of the quicksilver in the Thermometer.

Having observed this progress of the glass very clearly in my three first experiments, I directed the last to the purpose of ascertaining it, and for this reason I made it with the greatest care. I first of all adjusted the Microscope to the point at the extremity of the glass, the two wires coinciding, and the water being at 10°: I afterwards changed this first water into warm water of 70°, and was obliged to move the moveable wire 7,6 turns of the screw to reach the point. I then cooled the water progressively by 10° at a time, and these are the proportions of the condensations of the glass as they were marked by the moveable wire regressively, 31, 29, 26, 24, 22, 19. These are twentieth parts of the turns of the screw, the sum of them makes 7,6 turns, by which

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comparativement à celles du mercure dans le Thermomètre.

Ayant remarqué cette marche du verre d'une manière très sensible dans mes trois premières expériences, je dirigeai la quatrième vers le but de la déterminer, et je l'exécutai pour cela avec le plus grand soin. J'ajustai d'abord le Microscope sur la pointe qui étoit à l'extrémité du verre, les deux fils coincidant, et l'eau étant à 10°: je changeai ensuite cette première eau en de l'eau chaude à 70°: et il me fallut mouvoir le fil mobile de 7,6 tours de la vis, pour attendre la pointe. Puis je réfroidis l'eau de 10° en 10°, et voici les rapports des condensations du verre, tels que le fil mobile les indiqua en retrogradant: 31, 29, 26, 24, 22, 19. Ce sont des 20mes de tours de la vis; leur somme fait les 7,6 tours dont la vis s'étoit

which the ſcrew had got forward; and it is this one, of my four experiments, in which I ſaid that the return of the glaſs to the point from whence it had ſet out was perfectly exact. Theſe numbers are ſenſibly in arithmetical progreſſion; but I do not pretend to infer from thence, that this is the true law obſerved by the condenſations of glaſs, compared with the condenſations of quickſilver equal between themſelves; to affirm that, one ſhould have examined the Micrometer better. It is evident, however, that they are conſiderably decreaſing.

I muſt mention here, why I choſe to obſerve the condenſations of glaſs in water ſucceſſively leſs heated, rather than its dilatations in water ſucceſſively more heated. It is becauſe by this means I brought the water with much more certainty to an uniform temperature.

If

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s'étoit avancée; et c'eſt celle de mes quatre expériences où j'ai dit, que le retour du verre à ſon point de départ fut parfaitement exact. Ces nombres ſont ſenſiblement en progreſſion arithmétique; mais je ne prétends pas en conclure que ce ſoit là la vraie loi que ſuivent les condenſations du verre, comparativement à des condenſations du mercure égales entr'elles; il faudroit pour cela avoir mieux examiné le Micromètre: cependant on voit au moins avec certitude qu'elles ſont conſidérablement décroiſſantes.

Je dois faire mention ici de la raiſon pour laquelle j'ai préféré d'obſerver des condenſations du verre dans l'eau ſucceſſivement moins chaude, plutôt que ſes dilatations dans l'eau ſucceſſivement plus chaude. C'eſt que par cette voye j'amenois beaucoup plus ſurement mon eau à une température uniforme.

Si

If you pour warm water upon water that is leſs warm, whether it be that the firſt, being lighter, remains at the top, or that heat deſcends difficultly in water, or from both cauſes united, certain it is, that there may be ſurprizing differences between the top and the bottom. My Thermometer, as I ſaid before, was hung in ſuch a manner that the ball of it was near the middle of the rod of glaſs. When I poured warm water ſlowly upon water that was leſs warm, it ſometimes happened that the Thermometer did not vary till I had mixed them. I tried to convey warm water to the bottom through a pipe; but it immediately roſe: and if the Thermometer did not happen to be upon its aſcending ſtream, it ſtill roſe very little, and it was always requiſite to ſtir the water before the Thermometer was fixed. On the contrary, when I poured water

Si l'on verſe de l'eau chaude ſur de l'eau moins chaude; ſoit que la premiére étant plus légère reſte à la ſurface; ſoit que la chaleur deſcende difficilement dans l'eau; ſoit par l'une et l'autre de ces cauſes; il peut y avoir des différences de chaleur ſurprenantes entre le haut et le bas. Mon Thermométre, comme je l'ai dit, étoit ſuſpendu de maniére que ſa boule étoit au milieu de la hauteur de la branche de verre. Quand je verſois lentement de l'eau chaude ſur de l'eau moins chaude, il arrivoit quelquefois que le Thermométre ne varioit pas juſqu'à ce que je les euſſe mêlées. Je voulus eſſayer de porter l'eau chaude dans le fond par un tuyau; mais elle s'élevoit auſſitôt: et ſi le Thermométre ne ſe trouvoit pas ſur ſon paſſage, il montoit peu encore; et toûjours il falloit beaucoup agiter l'eau avant qu'il fût immobile. Au contraire lorſque je verſois l'eau moins chaude ſur

water leſs warm upon warmer water, it was ſcarce neceſſary to ſtir the mixture: before I had done it, the Thermometer had almoſt quite fallen to the point of the mean temperature. This therefore is much the ſafeſt method for all operations of this kind. I had already experienced it in the compariſon of Thermometers made of different liquors, which I have mentioned in my work above quoted.

By operating in this manner I was ſure to have given the rod of glaſs the degree of heat indicated by my Thermometer; and I repeat it, without aſſuring that the numbers above written give us the true law of the condenſations of glaſs comparatively with degrees that are equal among themſelves upon the Thermometer, their difference is too great, and too regular not to point out a progreſſion ſenſibly decreaſing. Here

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ſur l'eau plus chaude, je n'avois preſque pas beſoin d'agiter le mélange; avant que je l'euſſe fait, le Thermomètre avoit baiſſé preſque entièrement au point de la température moyenne.

C'eſt donc la méthode la plus ſure de beaucoup pour toutes les opèrations de ce genre. Je l'avois déja éprouvé dans la comparaiſon des Thermomètres de différentes liqueurs dont j'ai parlé dans mon ouvrage cité ci-devant.

En opérant donc de cette manière j'étois ſûr d'avoir communiqué à ma branche de verre la chaleur qu'indiquoit mon Thermomètre. Et je le répète, ſans aſſurer que les nombres rapportés ci-deſſus nous donnent la vraie loi des condenſations du verre, comparativement à des dégrés égaux entr'eux ſur le Thermomètre, leur différence eſt trop grande et trop régulière, pour ne pas indiquer une progreſſion ſenſiblement décroiſſante.

Here then is a ſecond inſtance in theſe experiments alone, of the difference there may be between the laws that follow in their progreſs different effects of the ſame cauſes; an object very important in natural philoſophy, and to the elucidating of which I dedicate the ſecond part of this paper.

PART

Ainſi voilà le ſecond exemple, dans ces expériences ſeules, de la différence qu'il peut y avoir entre les loix que ſuivent les différens effets des mêmes cauſes; objet important en phyſique, et auquel je deſtine la ſecond partie de ce Mémoire.

SECONDE

PART THE SECOND.

Observations upon physical measures.

MOST of our physical instruments are measures of effects. The progress made in natural philosophy, encreases every day the number of these measures; or rather it is by the encrease of them that natural philosophy has been so much improved within a century, and that it still continues daily to improve. In proportion as its different branches encrease or extend themselves, we see the catalogue of our *meters* encrease. Instead of continuing to be satisfied with perceiving, with conjecturing, with forming systems upon what is improperly called the possible, and is in fact the land of visions, we endeavour to

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SECONDE PARTIE.

Remarques sur les Mesures physiques.

LA plûpart de nos machines de physique, ne sont que des mesures d'effets. Le perfectionnement de la Physique augmente tous les jours le nombre de ces mesures; ou plutôt, c'est par leur augmentation que la physique a tant gagné depuis un siècle, et qu'elle gagne encore chaque jour: nous voyons s'accroître le catalogue de nos *mètres*, à mesure que ses diverses branches se développent et s'étendent. Peu contens aujourd'hui d'appercevoir, de conjecturer, de faire des systèmes dans ce qu'on appelle improprement le possible, et qui n'est que la région des chimères, nous entreprenons de découvrir les causes par leurs effets, en

to investigate causes through their effects, by measuring these wherever nature gives us a sufficient hold, in order not to be deceived by semblances of truth.

The first rays of this light, the dawn of all true knowledge in philosophy, were extremely weak. At first philosophers were satisfied with having instruments which indicated the existence of certain causes that our organs could either not discover at all, or discovered very imperfectly. Hence the modest names given by the first inventors to their instruments. They called only Baroscopes, Thermoscopes, Microscopes, those instruments which were intended to show the weight of the air, the dilatation of bodies by heat, the objects which escaped the naked eye.

These

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en mesurant ceux-ci, partout où la nature nous donne quelque prise, pour n'être pas trompés par des *apperçus*.

Les prémiers rayons de cette lumière, qui étoient l'aurore des vrayes connoissances en physique, furent d'abord très foibles. On se trouva bien content d'avoir des machines qui fissent appercevoir surement l'existence de certaines causes, que nos organes seuls ne pouvoient découvrir, ou ne decouvroient que très imparfaitement. De là les dénominations modestes que les prémiers inventeurs donnèrent à leurs machines. Ils n'appellèrent que Baroscopes, Thermoscopes, Microscopes, leurs instrumens destinés à montrer, le poids de l'air, la dilatation des corps par la chaleur, les objets qui échappent à la vue.

On

These names were too soon changed, in calling *measures* what was not yet such; but we every day become more delicate with respect to the conditions they require; and the progress made towards perfecting them, are the most effectual steps which have been made towards the knowledge of Nature; for it is they that have given us a disgust to the jargon of systems, founded upon mere hypotheses or deceitful appearances, the consequences of which were spreading fast into metaphysics, in which it occasioned general confusion.

The improvement of physical measures does not only lead us to a better knowledge of the immediate causes of the effects thus measured, but it assists us in decomposing complex effects, and especially in discovering and determining the simultaneous effects which I shall hereafter name the co-effects of the same causes.

When

On a changé trop tôt la terminaison de ces noms et de bien d'autres semblables, en qualifiant de *mesure* ce qui ne l'étoit point encore. Mais chaque jour on devient plus délicat sur les conditions qu'elles exigent; et les progrès vers leur perfection, sont les pas les plus réels qu'on ait faits vers la connoissance de la Nature: car ce sont ceux qui ont le plus contribué à nous dégouter du jargon des systèmes fondés sur des hypotheses ou des apparences trompeuses, dont les conséquences passoient en foule dans la Métaphysique, et y boulversoient tout.

Le perfectionnement des Mesures physique ne nous conduit pas uniquement à mieux connoitre les causes immèdiates des effets mésurés; mais il nous aide à décomposer les effets complexes, et surtout à découvrir et déterminer les effets simultanés, que je nommerai dans la suite les co-effets des mêmes causes.

Quand

When from experiments, sometimes very nice, we have assured ourselves that two or more effects constantly go together in certain relations, we may content ourselves with observing the most evident of them, and depend upon the existence of the others, as if they were immediately observed. This leads us from relation to relation to the discovery of operations of Nature which before were entirely hidden: and nothing can be more useful to man, than sometimes to examine, how he reasoned upon those objects before he was guided by experience.

These instances of the connections of effects, discovered, and afterwards measured one by the other, are now become so frequent in natural philosophy that it would be useless to insist upon them: and indeed when one considers our physical instruments, one may see that the greatest part

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Quand par des expériences, souvent très délicates, nous nous sommes assurés que deux ou plusieurs effets marchent toûjours ensemble dans certains rapports, nous pouvons nous contenter d'observer le plus évident, et compter sur l'existence des autres comme s'ils étoient immédiatement manifestes; ce qui nous conduit, de rapport en rapport, à découvrir des opérations de la Nature, qui avant cela nous étoient entièrement voilées: et rien n'est plus nécessaire à l'Homme, que d'examiner quelquefois comment il en raisonnoit avant qu'il eût ces secours.

Les exemples de ces liaisons d'effets, découverts, et mesurés ensuite les uns par les autres, sont aujourd'hui si multipliés dans la Physique, qu'il seroit inutile d'insister sur ce point: et quand on considère même l'ensemble de nos machines de

part of them are intended only for the discovery of co-effects, by the knowledge of those which are more evident. Our search after new measures has likewise the same end in view. If we wish for an Hygrometer, an Electrometer, a Photometer, it is less with a design of arriving by means of them to a knowledge of the absolute or relative quantities, of moisture, of electric fluid, of light, than to endeavour afterwards to connect the perceptible effects of these causes upon our measures, with other less evident effects, but which depend upon them, either as separate co-effects, or as modifications of other effects.

With all this the general problem of physical measures is a complicated one from the first outset. The first object of all these measures is to know the existence of a simple cause and of its degrees of intensity; and we have

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de physique, on voit que le plus grand nombre n'est destiné qu'à déterminer des co-effets, par la connoissance de ceux qui sont le plus évidens. La plûpart aussi de nos recherches de nouvelles mesures tendent à ce même but. Si nous desirons un Hygromètre, un Electromètre, un Photomètre, c'est moins pour connoitre, en les observant, les quantités absolues ou rélatives de l'humidité, du fluide électrique, de la lumiére; que pour travailler ensuite à lier les effets évidens de ces causes sur nos mesures, à d'autres effets moins évidens qui en dépendent, ou comme co-effets séparés, ou comme modifications d'autres effets.

Cependant le problème général des mesures physiques est compliqué dès son premier pas. Connoitre l'existence d'une cause simple et ses degrès d'intensité, est

have nothing to come at it, but the effects which this cause produces upon other bodies, which, for the most part, do themselves comprehend a great number of other causes. We can never therefore, observe effects absolutely simple; and consequently, sensible effects which are equal amongst themselves, do not point out degrees likewise equal amongst themselves in the cause to which they are attributed. What, for instance, are our measures of heat? The dilatations of bodies. What our measures of the weight of the air? The height of the quicksilver in the Barometer. But the dilatation of bodies by heat depends upon the nature of bodies, as well for its quantity, as for the law of its progression by equal augmentations of heat; and the effects of the weight of the air upon the quicksilver of the Barometer, are modified by the different degrees of the heat of this liquid, by

est le prémier objet de toutes ces mesures; et nous n'avons pour y arriver, que les effets que produit cette cause sur certains corps, qui déja eux-mémes renferment le plus souvent une multitude d'autres causes. Jamais donc nous ne pouvons observer des effets absolument simples; et par conséquent, des effets sensibles qui sont égaux entr'eux, ne marquent point des degrés, aussi égaux entr'eux, dans la cause à laquelle nous les attribuons. Qu'est ce par exemple que nos mesures de la chaleur? Ce sont les dilatations des corps. Qu'est.ce que la mesure du poids de l'air? C'est la hauteur du mercure dans le Barométre. Et déja la dilatation des corps par la chaleur dépend de leur nature, tant pour sa quantité, que pour la loi de ses progrés par des augmentations égales de la chaleur: et l'effet du poids de l'air sur le mercure du Barométre est modifié, par les divers degrés

by the nature of the *vacuum* in which it is suspended, by the attraction of the glass, by the friction, perhaps by the permeability of glass to some particles of that mixed fluid to which we give the general name of air, or by various other causes that are equally unknown to us. The same holds true with regard to all other physical measures; and this first step of the ladder, by which we strive to raise ourselves to the knowledge of causes, is already very difficult to ascertain.

The second, however, is much more so, since it depends upon determining the co-effects of the same causes, either in the same or in different bodies. Thus when we shall be possessed of an Hygrometer, we shall endeavour to find out what effects this *humor*, whose presence and degrees will be indicated by the instrument, produces

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degrés de chaleur de ce liquide, par la nature du vuide dans lequel il est suspendu, par l'attraction du verre, par le frottement; et peut-être encore par la perméabilité du verre à quelques particules de ce que nous appellons en général l'air, ou par d'autres causes qui nous sont également inconnues. Il en est de même de toutes les autres mesures physiques; et ce premier échellon, par lequel nous cherchons à nous élever à la connoissance des causes, est déja très difficile à bien assurer.

Mais le second l'est bien advantage. Il consiste à déterminer les co-effets des mêmes causes, ou dans les mêmes corps, ou dans des corps différens. Ainsi, quand nous aurons un Hygromètre, nous chercherons à savoir quel effet produit, sur l'yvoire qui s'allonge, sur les sels dont le poids augmente, sur la densité de l'air

produces upon ivory which lengthens, upon salts whose weight increases, upon the density of the air which varies, upon its salubrity, its refringent power, &c.

But even when this is done, we shall be embarassed by the degrees of these co-effects; probably they will not all increase in the same proportions as the dilatations of the ivory, or the augmentations of the weight of an absorbant body; and a great many experiments will be necessary to discover the laws they observe by the different intensities of this common cause.

It is notwithstanding upon relations of this sort, that every thing depends in experimental philosophy and indeed in all true philosophy. Consequently, the perfecting the methods of determining these relations must be a principal object with all good philosophers. I will not

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l'air qui varie, sur sa salubrité, sur sa vertu refringente, cette *humor* dont la présence et les degrés seront indiqués par l'instrument.

Mais alors encore les degrés des co-effets nous embarrasseront: il ne croitront pas vraisemblablement dans les mêmes rapports que les dilatations de l'yvoire, ou les augmentations de poids d'un corps absorbant; et il faudra bien des expériences pour découvrir les loix qu'ils suivent par les différentes intensités de cette cause commune.

C'est cependant à des rapports de ce genre que tout se réduit dans la physique expérimentale, et par cela même dans la bonne physique spéculative qui ne se paye pas de mots. Par consequent le perfectionnement des méthodes pour déterminer ces rapports, doit être un des objets de la plus grande attention des Physiciens.

not enter into the methods which lead us ſtep by ſtep to the diſcovery of the correſponding progreſſions of co-effects of the ſame cauſes: it would be too difficult to generalize them whenever one went beyond that fundamental principle of all ſcience, ſound logic; and, what is the beſt preſervative againſt precipitation, the knowledge of the weakneſs of our organs and of our underſtanding.

But it is not always practicable to trace in all its points, the curve that is deſcribed by a ſeries of correſponding phenomena; and we are often forced to content ourſelves with conſidering as proportional in all degrees of intenſity of the cauſe, ſome relations that have been either obſerved or found by experiment. This expedient one is likewiſe often reduced to in practice, in order not to complicate proceſſes without reaſon. Thus, for

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ſiciens. Je ne m'arrêterai pas à celles qui conduiſent à découvrir pas à pas les marches correſpondantes des co-effets des mêmes cauſes: il ſeroit trop difficile de les généraliſer dès qu'on voudroit aller au de là de ce principe fondamental de toute ſcience, une bonne logique; et de ce préſervatif contre la précipitation, la connoiſſance de la foibleſſe de nos organes et de notre intelligence.

Mais il n'eſt pas toûjours poſſible de tracer par tous leurs points, les courbes que décrivent les ſuites des phénomènes correſpondants; et l'on eſt ſouvent réduit à ſe contenter de regarder comme proportionnels dans tous les degrés d'intenſité de la cauſe, quelques rapports obſervés ou trouvés par l'expérience. C'eſt même un expédient auquel on eſt le plus ſouvent réduit dans la pratique, pour n'y pas compliquer les procédés ſans avantage. Ainſi dans le Pendule, comme dans mon

Hygromètre,

for inſtance, in the Pendulum and in my Hygrometer, if one would take notice of the different laws which follow, in their dilatations by heat, the ſubſtances whoſe effects one means to compenſate the one by the other, beſides the difficulty of finding theſe laws, the application might perhaps throw us into mechanical complications, that would deſtroy all the exactneſs we want to produce by theſe means.

In general, the fixing of laws is ſcarce ever the firſt ſtep taken in new diſcoveries. One begins by eſtabliſhing by experiment ſome fundamental relations, and one conſider afterwards the correſponding points of the phenomena as being proportional, till be degrees, overcoming the difficulties, one becomes to grow familiar with what one uſed to look upon before as great ſtrides; and one perceives

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Hygromètre, ſi l'on vouloit avoir égard aux differentes loix que ſuivent dans leurs dilatations par la chaleur, les matières dont on cherche à compenſer les effets les uns par les autres; outre la difficulté de découvrir ces loix, on ſe jetteroit peut-être dans des complications mécaniques, qui détruiroient toute l'exactitude qu'on vouloit chercher par ce moyen.

En général, dans toutes les nouvelles découvertes, les premiers pas ſont rarement des fixations de Loix. On établit quelque rapports fondamentaux par l'expérience, et l'on regarde les autres points correſpondans des phénomènes, comme proportionnels à ceux là; juſqu'à ce que, ſurmontant par degré les obſtacles, on ſoit parvenu à ſe familliariſer avec ce qu'on regardoit d'abord comme de grands pas

perceives that there are greater ſteps that may and ought to be made.

It is uſeful, therefore, to conſider in what manner one might with ſome ſafety mark out theſe firſt ſketches of the laws of Nature, by finding the particular relations of the co-effects which might be applied with leaſt error to proportional ſcales. It will be contributing to bring forward the moment in which, ſeeing clearer into the nature of things, and having learnt to diſtinguiſh real knowledge from what has only the appearance of it, we ſhall be led to ſeek for exactneſs in every thing.

A prac-

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pas, et à ſentir qu'il faut et qu'on peut aller plus loin.

Il eſt donc avantageux de conſidérer, comment on pourroit tracer avec quelque ſureté ces premières eſquiſſes des Loix de la Nature, en trouvant les rapports particuliers des co-effets qui s'appliqueroient avec le moins d'erreur à des échelles proportionnelles. Ce ſera accélèrer le moment, où, voyant plus clair dans la nature des choſes, et diſtinguant bien les connoiſſances réelles, d'avec ce qui n'en a que l'apparence, nous nous ſentirons conduits à chercher l'exactitude partout.

Moyen

A practical method of approximation in the determination of the co-effects of the same causes.

It has been thought hitherto, that in order to leſſen the effects of the errors which are unavoidable in obſervations and experiments, one ought to look for the relations of the co-effects at the greateſt poſſible diſtances; becauſe in that caſe the errors being divided upon a greater ſpace, each ſeparate part is leſs affected by them. Thus, in order to find the relation of the dilatations of braſs and ſteel employed in the Pendulum, one would willingly expoſe theſe metals to an artificial congelation and to the heat of boiling oil, that, by meaſuring greater lengthenings,

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Moyen pratique d'approximation, dans la fixation des rapports des co-effets des mêmes cauſes.

On a imaginé juſqu'ici, que pour diminuer les effets des erreurs inévitables dans les obſervations et les expériences, il falloit chercher les rapports des co-effets aux plus grandes diſtances poſſibles; parce que ces erreurs ſe diviſant ſur un plus grand intervalle, deviennent plus inſenſibles ſur chacune de ſes parties. C'eſt ainſi que pour trouver le rapport des dilatations du léton et de l'acier qu'on employe au Pendule, on expoſeroit volontiers ces métaux à une congélation artificielle et à la chaleur de l'huile bouillante; afin que pouvant meſurer de plus grands

lengthenings, the imperfection of the meaſure might become inſenſible in the determination of their relation.

This method is indeed very good to compare with each other effects, the progreſſions of which are proportioned; and one is right to make uſe of it whenever extent or weight is concerned. But it is often very deceitful in phyſics: for as the co-effects ſeldom go by proportional degrees, the more the obſerved points of their relations are diſtant, the more the deviations become conſiderable in the intermediate points, when they are conſidered as proportional to the whole. It is thus that two different curves, which cut each other in two points, deviate the more from each other in the ſpace comprized between the two interſections, the more diſtant the points of interſection are from each other. Now the correſpondent points, taken by obſervation in two ſeries of phenomena which

grands allongemens, l'imperfection de la meſure devînt inſenſible dans la fixation de leur rapport.

Cette méthode ſeroit très bonne pour comparer entr'eux des effets qui auroient des marches proportionnelles; et on l'employe avec raiſon quand il s'agit de l'étendue ou des poids: mais elle eſt le plus ſouvent fort trompeuſe en Phyſique. Car dès que les co-effets marchent rarement par degrés proportionnels, plus les points obſervés des rapports ſont diſtans, plus les écarts deviennent grands dans les points intermédiaires, en les regardant comme proportionnels au rapport total. C'eſt ainſi que deux courbes différentes qui ſe croiſent en deux points, s'écartent d'autant plus l'une de l'autre dans l'intervalle des deux interſections, que ces points de rencontre ſont plus éloignés. Or les points correſpondans par obſervation,

which follow different laws, are the interſections of the curves; and the errors we make in the intermediate relations when we conſider them as proportional, are like the deviations of the two curves in the interval of the interſections.

The effect then intended to be produced by taking very diſtant points of compariſon being, in general, to accumulate within the intervals of theſe points the deviations of the laws which happen to be different; much will be gained, in thoſe caſes in which the laws themſelves cannot be diſcovered, by ſeeking for points of compariſon within the leaſt diſtances that the particular obſervations, for which the phyſical meaſures are intended, will allow.

It is thus that we have happened to have uſed for a long time Thermometers of quickſilver and ſpirits of wine, in

tion, de deux ſuites de phénomènes qui ſuivent des loix différentes, ſont les interſections de ces courbes; et les erreurs qu'on fait dans les rapports intermédiaires en les conſidérant comme proportionnels, ſont comme les écarts des deux courbes dans l'intervalle des interſections.

L'effet de prendre des points de comparaiſon fort diſtans, étant donc en général, d'accumuler dans l'intervalle de ces points les écarts des Loix qui ſe trouvent différentes, on gagnera beaucoup, dans les cas où l'on ne pourra pas découvrir les Loix elles-mêmes, à chercher des points de comparaiſon dans les moindres diſtances que puiſſent comporter les obſervations particulières auxquelles on deſtinera les Meſures phyſiques.

C'eſt ainſi que par hazard on a eu longtems des Thermomètres de mercure

in which the difference of the progreffions between thefe two liquids in their dilatations by heat was not obferved. Mr. DE REAUMUR's Thermometer, one of the firft to which one endeavoured to affign fixed points, was too difficult in its conftruction for each Thermometer to be immediately graduated; and indeed the author himfelf only ufed his method in making ftandards to which the Thermometers intended for common ufes were afterwards compared. Abbé NOLLET, his difciple, who for a long time gave the *ton* for Thermometers in France and in the Southern countries, followed his mafter's method. The only immediate point he marked upon his Thermometers was that of *freezing*; and he compared them afterwards in water of 30° of that fcale in which the real interval between the *freezing* and *boiling points* ought to be divided into about 100°. By this method,

mercure et d'efprit de vin où l'on ne remarquoit pas les différences de marche de ces deux liquides. Le Thermomètre de Mr DE REAUMUR, l'un des premiers auxquels on aît tenté de donner des points fixes, étoit trop difficile à conftruire, pour que chaque Thermomètre pût être gradué immédiatement. Auffi l'auteur lui-même n'employoit-il fa méthode qu'à faire des étalons, auxquels il comparoit enfuite les Thermomètres deftinés aux ufages ordinaires. Mr l'Abbé NOLLET, fon difciple, qui pendant long tems a donné le ton pour les Thermomètres, tant en France que dans les pays méridionaux, fuivit la méthode de fon maitre: il ne marquoit immédiatement fur fes Thermomètres, que le point de la congélation; et il les comparoit enfuite dans de l'eau à 30° de cette échelle là, où 100° environ, divifoient l'intervalle réel de la congélation à l'eau bouillante.

 Par

thod, and at that period in which natural philoſophy was ſtill very inaccurate, one did not obſerve the difference between the progreſſions of ſpirits of wine and quickſilver; and in fact it was eaſy to miſtake them. I have ſhewn it in ſpeaking of theſe Thermometers, and I ſhall have occaſion to ſhew it more fully very ſoon.

This doubtleſs was a defect, and a very conſiderable one, whether we conſider that the Thermometer is intended to indicate degrees of heat, both much larger and much ſmaller than the interval between theſe 30°; or that it is a moſt capital inſtrument in natural philoſophy, and as ſuch may be uſed in experiments where the leaſt defects may have ſenſible conſequences. For theſe reaſons I only mention this caſe, as an help for me to explain what I propoſe to ſay upon other inſtruments in which

Par cette méthode, et dans ce tems là où la Phyſique étoit encore fort peu exacte, on ne remarquoit pas la différence des marches du mercure et de l'eſprit de vin, & l'on pouvoit en effet s'y méprendre. Je l'ai montré en traitant de ces Thermomètres, et je le montrerai plus particulièrement bientôt.

C'étoit là ſans doute un défaut, et un défaut très grand; ſoit parce que le Thermomètre eſt deſtiné à indiquer des degrés de chaleur bien plus grands et bien moindres que l'intervalle de ces 30°; ſoit parce que c'eſt un inſtrument fondamental en Phyſique, qui peut être employé à des expériences où le moindre défaut auroit des effets ſenſibles. Auſſi ne rapporté-je ce cas, que pour m'expliquer plus

which a greater degree of exactneſs would be either uſeleſs or impoſſible.

Simple and evident as this principle is, yet, as it often happens that obvious ideas do not ſtrike, even becauſe their being obvious gives them a trivial air, I will ſtrengthen this by uſeful examples, and one offers itſelf to which I have been led by the foregoing.

Suppoſe one wants to know the dilatations of ſpirit of wine by heat, in order to have regard to it in eſſaying the ſpirituoſity of vinous liquors, which is wont to be eſtimated by their ſpecific gravities; an article as every one knows of much conſequence in the brandy trade, and which has likewiſe ſome connection with chemiſtry: I ſay, that one would make a conſiderable error if, in order to determine the relations of the dilatations of the brandy with the indications of the quickſilver Thermometer, which ſhould

plus aiſément dans ce que je me propoſe de dire ſur ceux où une plus grande exactitude ſeroit ou inutile ou impoſſible.

Quelque ſimple et évidente que ſoit cette règle, comme il arrive ſouvent que les idées ſimples ne frappent point, préciſément à cauſe de leur ſimplicité qui leur donne un air trivial, je fortifierai celle-ci par des exemples utiles. Et en voici un d'abord, auquel l'exemple précédent m'a conduit.

Je ſuppoſe qu'on veuille connoitre les dilatations de l'eſprit de vin par la chaleur, afin d'y avoir égard dans la meſure de la ſpirituoſité des liqueurs vineuſes, eſtimée par leur peſanteur ſpécifique; objet important au commerce conſidérable des eaux de vie, et qui intéreſſe auſſi la chimie. Je dis qu'on feroit un grand écart, ſi, pour déterminer le rapport des dilatations de l'eſprit de vin, avec les indications du Thermomètre de mercure, qui ſerviroit enſuite à marquer la tem-

pérature

ſhould afterwards mark the temperature of this liquor, one was to take terms very diſtant from each other, as for inſtance the freezing and boiling points; whilſt on the other hand, by keeping within the temperatures in which the trials ſhould be made, one would come ſo near the truth, that the differences would be imperceptible.

This inſtance, in itſelf uſeful, being proper to be applied to every caſe in which we wiſh to compare with one another phyſical effects which depend upon the ſame cauſe, that we may afterwards judge of them all by one, I ſhall dwell upon it a little while to give a full explanation of it.

I ſuppoſe that the experiments intended to eſſay the ſpirituoſity of different liquors diſtilled from wine, by the ſpecific gravities of them, are made between the temperatures which anſwer to 32° and 77° of

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pérature de cette liqueur, on prenoit des termes fort éloignés, comme la congélation et l'eau bouillante: tandis qu'au contraire, en ſe renfermant dans les températures où les épreuves ſeroient faites, on approcheroit ſi fort de la vérité, que les différences ſeroient imperceptibles.

Cet exemple ayant quelque utilité par lui-même, et pouvant être appliqué à toute ſorte de cas où l'on compare entr'eux des effets phyſiques dépendants d'une même cauſe, pour juger enſuite de tous par un ſeul, je m'y arrêterai afin de le mieux éclaircir.

Je ſuppoſerai que les expériences deſtinées à éprouver les degrés de ſpirituoſité des diverſes liqueurs diſtillées du vin, par leur peſanteur ſpécifique, ſe feront entre les températures qui correſpondent à 32° et 77° ſur le Thermomètre de FAHRENHEIT

of FAHRENHEIT's Thermometer, as this takes in all the ordinary cafes. The queftion is, to examine which is the moft convenient method of introducing into this meafure an equation for the differences of the heat; an equation I mean which does not occafion ufelefs difficulties.

These temperatures 32° and 77° upon FAHRENHEIT's Thermometer, correfpond with o and 20° upon the fcale of which I have hitherto fpoken, in which the boiling point is at 80° and the freezing at o. I fhall fpeak of this fcale, becaufe it is the one I made ufe of in my experiments on the progreffions of liquors diftilled from wine in their dilatations by heat, and of which I have given an account in my work abovementioned [(d)].

(*d*) Vol. I. p. 326.

I fuppofe

RENHEIT, ce qui renfermera tous les cas ordinaires. Il s'agit donc d'examiner quelle fera la route la plus convenable, pour introduire dans cette mefure une équation pour les différences de la chaleur, equation qui n'occafione pas de trop grandes difficultés, fans utilité dans la pratique.

Ces températures 32° et 77° fur le Thermomètre de FAHRENHEIT, correfpondent à o et 20° fur l'échelle dont j'ai parlé jufq'ici, où l'eau bouillante eft à 80° et l'eau dans la glace à o. J'employerai encore cette échelle, parce que c'eft celle dont je me fuis fervi dans les expériences que j'ai faites autrefois fur la marche des liqueurs diftillées du vin, dans leurs dilatations pour la chaleur; expériences que j'ai rapportées dans mon ouvrage cité ci-deffus (*d*).

(*c*) Tom. I. p. 326.

Je

I ſuppoſe that, according to the uſual method, ſeeking for the dilatations of the ſpirituous liquor by great differences of heat, one was to compare its bulk in ice that melts and in boiling water, and that not knowing, or not regarding, the different progreſſions of this liquor and quickſilver in their reſpective dilatations, nor the effect which the difference of ſpirituoſity produces in this reſpect, one ſhould conſider theſe progreſſions as proportional. Here are the deviations one would be expoſed to in the limits of the temperatures to which the rule ſhould be applied, I mean between 0 and 20°.

The numbers which are placed in the two columns of the ſpirituous liquors indicate the proportions of the augmentations of their bulks by the temperatures indicated by the quickſilver Thermometer. I have given to the total ſcale of theſe proportions the ſame number of

Je ſuppoſe d'abord que ſuivant l'uſage ordinaire, cherchant les dilatations de la liqueur ſpiritueuſe par de grandes différences de chaleur, on comparât ſes volumes dans la glace qui fond et dans l'eau bouillante; et qu'ignorant ou négligeant la différence de marche de cette liqueur et du mercure dans leurs dilatations, ainſi que l'effet que produit même à cet égard la différence de ſpirituoſité, on regardât ces marches comme proportionelles; voici les écarts dans leſquels on tomberoit, dans les limites des températures où l'on appliqueroit la régle; c'eſt à dire de 0 à 20°.

Les nombres placés dans les deux colonnes des liqueurs ſpiritueuſes, marquent les rapports de leurs augmentations de volume par les températures indiquées ſur le Thermomètre de mercure. J'ai donné à l'échelle totale de ces rapports le même

of equal parts as to the Thermometer, in the ſame interval of temperature, in order that their differences within this interval may be viſible without calculation.

Quickſilver Therm.	Spirit of wine which fires gunpowder.	Brandy, of 2 parts flegm and 3 of this ſpirit of wine.
80	80	80
. . . . .	. . . . .	. . . . .
20	16,5	15,9
15	12,2	11,8
10	7,9	7,7
5	3,9	3,8
0	0	0

One ſees what deviations in general ariſe from the diſtance of the points of compariſon when one comes to apply

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même nombre de parties égales qu'à celle du Thermomètre dans le même intervalle de température, afin que les différences en dedans de cet intervalle s'apperçoivent à l'oeil ſans calcul.

Thermomètre de mercure.	Eſprit de vin qui brule la poudre.	Eau de vie faite de 2 parties de flegme, ſur 3 parties de cet eſprit de vin.
80	80	80
. . . . .	. . . . .	. . . . .
20	16,5	15,9
15	12,2	11,8
10	7,9	7,7
5	3,9	3,8
0	0	0

On voit quels écarts reſultent en général de la diſtance des points de comparaiſon,

apply them to the temperatures in which one preciſely wants the moſt exact proportions. One likewiſe may ſee that the difference of ſpirituoſity only, occaſions very ſenſible ones in the progreſſions of the two ſpirituous liquors, and that conſequently one would commit a double error, if one were to conſider the intermediate relations as proportional to the total relation, eſtabliſhed between quickſilver and one of theſe liquors, only by obſervations made in very different temperatures.

If, on the contrary, the fundamental experiments had been made at the probable limits of the obſervations, that is at o and at 20° of the Thermometer, having then the real dilatation, between theſe two temperatures, of the ſpirituous liquor which ſerved for the experiment, there would be only theſe ſmall deviations, expreſſed by the compariſon

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raiſon, quand on vient à les appliquer aux températures où l'on avoit beſoin préciſément des rapports les plus exacts. On voit auſſi que la différence ſeule de ſpirituoſité, en produit de très ſenſibles dans la marche des deux liqueurs ſpiritueuſes; et que par conſéquent ont tomberoit doublement dans l'erreur, en regardant ces rapports intermédiaires comme proportionnels au rapport total, établi entre le mercure et une ſeule de ces liqueurs par des obſervations à de grandes différences de température.

Si au contraire on eût fait les expériences fondamentales aux limites probables des obſervations, c'eſt à dire à o et à 20° du Thermomètre; ayant alors la dilatation réelle, entre ces deux températures, de la liqueur ſpiritueuſe qui eût ſervi à l'expérience, on n'auroit à craindre que les écarts exprimés par les rapports des nombres

comparifon of the following numbers, in which the total dilatation of the fpirituous liquors is again divided into the fame number of equal parts with that of the quickfilver in the Thermometer.

Therm.	Dilat. of fpirit of wine.	Dilat. of brandy.
20	20	20
15	14,8	14,8
10	9,6	9,7
5	4,7	4,8
0	0	0

The feries of numbers which exprefs the dilatations of the two fpirituous liquors remain in the fame proportions as in the firft cafe, and confequently this is always the refult of the experiment. But thefe proportions come already

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nombres fuivans, où la dilatation totale des liqueurs fpiritueufes eft encore divifée en un même nombre de parties égales, que celle du mercure dans le Thermomètre.

Therm.	Dilat. de l'efprit de vin.	Dilat. de l'eau de vie.
20	20	20
15	14,8	14,8
10	9,6	9,7
5	4,7	4,8
0	0	0

Les fuites des nombres qui expriment les dilatations des deux liqueurs fpiritueufes reftent dans les mêmes rapports que dans le premier cas; et par conféquent c'eft toûjours le refultat de l'expérience. Cependant ces rapports font déja fi près

ready ſo near to the progreſſion of the quickſilver Thermometer itſelf, that the effect of the differences of the ſpirituoſity almoſt entirely vaniſhes; ſo that there would be little error in taking as proportional to the total augmentation of bulk at 20° of a certain liquor diſtilled from wine, the intermediate dilatations of every other liquor of the ſame kind.

It is poſſible, however, ſtill to leſſen theſe errors, without having more than two terms of compariſon by experience, by taking theſe terms within the limits of the probable obſervations, and that for two reaſons. The firſt, that the more numerous obſervations will probably be made nearer the points where true proportions have been found by experience. The other, that the greateſt deviation will be ſtill more leſſened, by throwing part of the errors beyond the two real points of compariſon, in order

ſi près de la marche du Thermomètre même, que l'effet des différences de ſpirituoſité s'évanouit preſque entièrement; et qu'il y auroit peu d'erreur à regarder comme proportionnelle à l'augmentation totale de volume à 20° d'une certaine liqueur diſtillée du vin, les augmentations intermédiares de toute autre liqueur du même genre.

On peut cependant diminuer encore ces erreurs, ſans avoir plus de deux termes de comparaiſon par l'expérience, en prenant ces termes en dedans même des limites des obſervations probables; et cela par deux conſidérations. La prémière que les obſervations les plus nombreuſes ſe trouveront probablement plus près des vrais rapports fixés par l'expérience; l'autre que le plus grand écart diminuera encore, en

order to lessen the accumulation of them within these points.

If, for instance, instead of observing from o to 20° the increase of the bulk of the spirituous liquor which is to serve as a rule, one observes it from 5° to 15°, one will have the following proportions, in which the progression of the two liquors still continue within their real proportions, as I shall shew in the sequel.

Therm.	Dilat. of the spirits of wine.	Dilat. of brandy.
20	20,2	20,1
15	15	15
10	9,8	9,9
5	5	5
0	0,3	0,3

It

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en rejettant une partie des erreurs au delà des deux points réels de comparaison, pour en diminuer l'accumulation entre ces points.

Si par exemple, au lieu d'observer de o à 20° l'augmentation de volume de la liqueur spiritueuse qui doit servir de règle, on l'observe de 5° à 15°, on aura les rapports suivans, où les marches des deux liqueurs restent encore dans leurs proportions réelles, ce que je montrerai dans la suite.

Therm.	Dilat. de l'esprit de vin.	Dilat. de l'eau de vie.
20	20,2	20,1
15	15	15
10	9,8	9,9
5	5	5
0	0,3	0,3

On

It is evident then, that there is no longer any ſenſible error ariſing from the differences of ſpirituoſity, which is already a capital advantage in the caſe propoſed as an example; in which, ſince what one wants to know is the degree of ſpirituoſity of a liquor, one cannot ſuppoſe it known before hand, in order to have regard to it in ſeeking for it. One likewiſe ſees in general, that there is ſcarce any error to apprehend, even in conſidering the augmentation of the bulk of theſe liquors, or the diminution of their ſpecific gravities, as being proportional to the indication of the quickſilver Thermometer.

Here is then the method in which, according to this principle, I would conſtruct the comparative *Areometer*, that is ſuch a one as might be made the ſame every where. I chuſe this example becauſe it will afford me other applications of the general rule.

Project

On voit donc qu'il n'y a plus d'erreur ſenſible réſultante des différences de ſpirituoſité; et c'eſt d'abord un avantage capital dans le cas propoſé pour exemple, où, cherchant à connoître le degré de ſpirituoſité d'une liqueur, on ne peut pas la ſuppoſer d'avance pour y avoir égard en la meſurant. On voit auſſi en général, qu'il n'y a preſque plus d'erreur à craindre, même en regardant l'augmentation de volume de ces liqueurs, ou leur diminution de peſanteur ſpécifique, comme proportionnelles à l'indication du Thermomètre de mercure.

Voici d'après ce principe, comment je conſtruirois l'Aréomètre comparable; c'eſt-à-dire celui qu'on pourroit faire de même partout. Je choiſis cet exemple, parce qu'il me fournira encore d'autres applications de la règle.

Id

Project of a comparable Areometer.

I would uſe an Areometer of the moſt common conſtruction[(f)]. It is an inſtrument nearly reſembling the glaſs of a Thermometer; that is, a tube with a hollow ball at one end. The property of this inſtrument is, that it ſinks the deeper into liquids, the more their ſpecific gravity decreaſe. But that it may become a common meaſure of this ſpecific gravity, certain fixed points and determined degrees muſt be aſcertained upon it.

I would make this Areometer of glaſs, as being the ſubſtance which undergoes the leaſt change of bulk by heat, and the changes of which are the moſt regular, at

(f) See fig. 3. and its explanation.

leaſt

Idée d'un Aréomètre comparable.

J'emploierois la forme d'Aréomètre qui eſt la plus commune(f). C'eſt un inſtrument à peu près ſemblable au verre d'un Thermomètre, c'eſt à dire composé d'une boule creuſe, et d'un tube qui lui eſt joint. La propriété de cet inſtrument eſt de s'enfoncer d'autant plus dans les liquides, que leur peſanteur ſpecifique eſt moindre. Mais pour qu'il devienne une meſure commune de cette peſanteur ſpecifique, il faut qu'il aît des points fixes et des dégrés déterminés.

Je le ferois de verre; comme étant la matière qui éprouve le moins de changement dans ſon volume par la chaleur, et dont les changemens ſont les plus régu-

(f) Voyez la fig. 3. et ſon explication.

liers;

leaſt of all the ſubſtances which are not affected by humidity. I would always uſe flint-glaſs, that its changes in this reſpect might be more uniform in all the Areometers.

Its ball ſhould be one inch and an half in diameter, and there ſhould be at the bottom of it a little hollow cylinder, which ſhould communicate with it, and contain the ballaſt, in order that it might be able to keep upright, at the other end, a branch ſo much the longer; which will be eaſily underſtood. This ball ſhould not be very thick, any more than the ſuperior branch on which its diviſions ſhould be marked. The different thickneſſes of this branch, that is, its different external diameters, will produce the different ſenſibilities of the inſtrument. The leſs the diameter will be, the more will the Areometer

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liers; du moins entre les matières que l'humidité n'affecte pas. Ce verre ſeroit toûjours le flint-glaſs, afin que ſes changemens à cet égard fuſſent plus uniformes dans tous les Aréomètres.

Je donnerois à ſa boule un pouce et demi de diamètre: et elle auroit à ſon fond un petit cylindre creux qui communiqueroit avec elle, et renfermeroit le leſt; afin de pouvoir maintenir de bout, au côté oppoſé, une branche d'autant plus longue; ce qu'on ſentira aiſément. Je ferois cette boule peu épaiſſe, ainſi que le tube ſuperieur, ou la branche ſur laquelle les diviſions devroient être marquées. Les différentes épaiſſeurs de cette branche, c'eſt à dire ſes différens diamètres extérieurs, feront les différentes ſenſibilités de l'inſtrument: plus ſon diamèter

ter ſink by an equal augmentation of the ſpirituoſity of the liquor.

Unleſs the branch be perfectly cylindrical, the meaſure would be irregular. It may be a thin braſs tube ſilvered over, or a ſilver tube, cemented to the ball of glaſs. Such metal tubes are eaſily drawn through holes as wires; ſo that one might be ſure to have them cylindrical. The dilatation of that tube by heat, beſides that it is too inconſiderable to be taken notice of, would combine itſelf with that of the liquor, of which I ſhall ſpeak hereafter.

I would ballaſt the inſtrument with quickſilver, in order to have it always ſtand upright in the ſame manner; and of this I would put in ſuch a quantity that the moſt ſpirituous liquor, being heated as much as it can be in the

mètre ſera petit, plus l'Aréométre s'enfoncera pour une même augmentation de ſpirituoſité de la liqueur.

Cette branche devroit être parfaitement cylindrique; ſans quoi elle introduiroit de l'irrégularité dans la meſure. On pourra la faire d'une tube de cuivre argenté ou d'argent, fort mince, cimenté avec la boule. On fait fort bien ces petits tuyaux de métal à la filière; ainſi on ſeroit ſûr de les avoir cylindriques; et quant à l'effet qu'y produiroit la chaleur, il peut être compté pour rien. D'ailleurs il ſe combinera avec celui que produira cette cauſe ſur la liqueur, et dont je parlerai ci-après.

Je leſterois l'inſtrument avec du mercure, pour qu'il ſe tint toûjours de-bout de la même manière; et je l'y mettrois en telle quantité, que la liqueur la plus ſpiritueuſe, échauffée autant qu'elle pourra l'être dans les expériences, laiſſât enfoncer

the experiments, may let the Areometer ſink nearly to the top of its branch. This branch ſhould at the ſame time be long enough that the leſs ſpirituous liquors, wine for inſtance, reduced to congelation, may let a ſmall part of it be immerſed.

The inſtrument being thus prepared, I would take ſome weak ſpirit of wine dilated with one part of water on ſix parts of ſpirits of wine which fires gun-powder or linen which is ſteeped in. I would then determine the ſpecific gravity of this ſpirit of wine at the temperature of 10° upon my Thermometer, or 54°½ of FAHRENHEIT's, by means of a very nice hydroſtatical balance. This liquor, undetermined at firſt, and which I ſhould call only weak ſpirit of wine, on account of the in-termination of the ſpirit which burns linen, will be de-termined as ſoon as a firſt Areometer ſhall have been conſtructed

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foncer l'Aréomètre juſques près du haut de ſa branche; qui devroit être en même tems aſſez longue, pour que la liqueur la moins ſpiritueuſe, le vin par exemple, réduit à la congélation, en laiſſât encore enfoncer une petite partie.

L'inſtrument ainſi préparé, je prendrois un eſprit de vin foible, compoſé d'1 partie d'eau ſur 6 parties d'eſprit de vin qui brule la poudre, ou qui enflamme le linge dont il eſt mouillé. Je déterminerois la peſanteur ſpecifique de cet eſprit de vin, tandis qu'il feroit à la température 10° de mon Thermomètre, ou 54½° de celui de FAHRENHEIT, en employant pour cette détermination une balance hydroſtatique fort délicate. Cette liqueur, d'abord indéterminée, et que j'appellerai ſeulement eſprit de vin foible, à cauſe de l'indétermination de l'eſprit de vin qui brule le linge, ſera déterminée dès qu'on aura fait un prémier Aréomètre par

conſtructed on this plan. It will be then a ſpirit of wine, which, in the aforeſaid temperature, being eſſayed by the hydroſtatic balance, will weigh ſo much a cube foot: and every inſtrument-maker, who ſhall undertake to conſtruct ſuch Areometers, will be obliged to begin by compoſing this fixed liquor by the help of the hydroſtatic balance, in order to conſtruct his ſtandard. And indeed all I have farther to ſay upon the conſtruction of the ſcale of this inſtrument, relates merely to a ſtandard, to which the Areometers in uſe may be compared in order to form a ſcale.

For this purpoſe they may be dipped ſucceſſively in two liquids of very different ſpecific gravities, and ſuch that the ſtandard may indicate thoſe ſpecific weights by whole numbers of degrees, the difference of which may admit of a diviſion into aliquot parts: it will be very eaſy ſo to modify

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par cette méthode; ce ſera de l'eſprit de vin, qui, étant à la tempèrature ſuſdite, et éprouvé à la balance hydroſtatique, pèſera tant par pied cube. Dès lors tout faiſeur d'inſtrumens qui voudra conſtruire originalement des Aréomètres, devra premièrement compoſer cette liqueur, par l'épreuve de la balance hydroſtatique, pour conſtruire ſon étalon. Car tout ce qui ſuit ne regardera plus en effet qu'un étalon, auquel les Aréomètres d'uſage ſeront ſimplement comparés pour former leur échelle.

A cet effet on les mettra ſucceſſivement enſemble dans deux liqueurs de peſanteur ſpécifique fort différentes, et telles que l'étalon indique ces peſanteurs ſpécifiques par des nombres entiers de degrés, dont la différence ſoit ſuſceptible d'être diviſée en parties aliquotes. Il ſera fort aiſé de compoſer ces deux liqueurs par des

modify thofe two liquids by mixtures. And when the two points at which the intended Areometer ftands in the two liquors, fhall be marked upon it, the interval between them may be divided into the number of degrees indicated by the ftandard. Here the two points of comparifon cannot be too diftant from each other, at leaft if the tubes of the two compared Areometers are cylindrical; for then their intermediate immerfions will always be proportionate to the obferved immerfions. I point out this, in order to give an example of the inftances in which the confiderations, that are the object of this part, do not take place. It is the fame as that in which Thermometers made of the fame liquid are divided by comparifon.

I return to the ftandard Areometer. I would dip it into this known fpirit of wine, whilft it is at the

des mélanges: et quand on aura marqué fur l'Aréomètre à conftruire les deux points où il fe fera tenu dans les deux liqueurs, on en divifera l'intervalle dans le même nombre de dégrés indiqué par l'étalon. Ici les deux points de comparaifon ne fauroient être trop diftans l'un de l'autre; fi du moins les tubes des deux Aréomètres comparés font cylindriques: car alors leurs enfoncemens intermédiares feront toûjours proportionnels aux enfoncemens obfervés. Je le fais remarquer pour donner un exemple des cas où les confidérations qui font l'objet de cette partie n'ont pas lieu. C'eft le même que celui où l'on divife par comparaifon des Thermomètres faits d'un même liquide.

Je reviens à l'Aréomètre étalon. Je le plongerois dans cet efprit de vin connu, tandis qu'il feroit à la température fixée; et je marquerois avec un fil fur fa branche,

the fixed temperature, and would mark upon its branch, with a thread, the point to which it ſinks: afterwards I would mix three parts of water with ſeven parts of this ſame ſpirit of wine, to make a ſort of brandy ſtronger than the common; it would be the *three-fifths* of Languedoc, which conſiſts of two parts water and three parts ſpirit of wine that fires gun-powder. I would again dip the Areometer into it, at the ſame temperature, and would likewiſe mark this new point with a thread.

One may ſee that, according to the principle I have above eſtabliſhed, I take points of compariſon which are within the limits of the greateſt and ſmalleſt ſpirituoſity of the liquors to be tried, in order to obtain a ſcale of equal parts, free from any ſenſible error: and in this caſe that precaution is very neceſſary; for the degrees of ſpirituoſity

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branche, le point où il s'enfonceroit. Puis je mêlerois à 7 parties de cet eſprit de vin, 3 parties d'eau, pour en faire une eau de vie plus forte que l'eau de vie commune; ce ſeroit le *trois quints* de Languedoc, qui doit être 2 parties d'eau ſur 3 parties d'eſprit de vin qui brule la poudre. J'y plongerois de nouveau l'Aréomètre dans la même température, et je marquerois auſſi ce nouveau point par un fil.

On voit que ſuivant le principe que j'ai établi ci-devant, je prends des points de comparaiſon en dedans de la plus grande et de la moindre ſpirituoſité des liqueurs qu'on éprouvera, pour obtenir une échelle en parties égales, ſans erreur ſenſible: et cela eſt bien néceſſaire ici; car les degrés de ſpirituoſité ne ſuivent pas

rituofity do not follow thofe of fpecific gravity, as I fhall explain hereafter.

The points thus indicated upon the branch, having determined principles, will be the fixed points of the Areometer. The interval between them fhall be divided into 30 equal parts, each of which will reprefent $\frac{1}{30}$th of the total effect of the added water upon the fpecific gravity of the liquor. The fequel will fhew, that it is equally for the conveniency of trade, and of the workman who fhall divide the fcale, that I have chofen this number.

I fuppofe that the ftandard will be conftructed in fuch a manner, that the difference of the finkings fhall be confiderable enough for this purpofe, which may be obtained by making the branch thin enough. One may afterwards, if it be thought fit, for Areometers of common

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pas ceux de pefanteur fpécifique, comme je le dirai ci-après.

Les points indiqués ainfi fur la branche, ayant des principes déterminés, feront les points fixes de l'Aréomètre. On en divifera l'intervalle en 30 parties, qui feront des 30mes de l'effet total de l'eau ajoutée, fur la pefanteur fpécifique de la liqueur. On verra dans la fuite que c'eft autant pour la commodité du commerce, que pour celle de l'ouvrier qui divifera l'échelle, que j'ai choifi ce nombre de parties.

Je fuppofe que l'on conftruira l'étalon de manière que la différence d'enfoncement foit affez grande pour cet effet; ce qu'on peut obtenir en faifant la branche affez mince. On pourra enfuite fi l'on veut, pour les Aréomètres d'ufage peu délicat,

mon uſe, and in which it is not neceſſary that the branch ſhould be ſo long, divide the fundamental interval into 15 parts, which will then be double degrees.

Having in this manner fixed points and determined degrees upon the Areometer, the next thing is to chuſe a convenient place for the o of its ſcale; and the beſt will be that by which all the eſſays of the liquors ſhall be expreſſed with the ſame ſign. To effect this, one may take one of the wines of which brandy is moſt commonly made, and, reducing it to the temperature of water in ice, dip the Areometer in it, obſerving how much higher it will ſtand than the inferior fixed point. This exceſs of emerſion, compared with the fundamental ſcale, and reduced to the neareſt number of degrees which will be an aliquot part of it, will be a proportional quantity fixed for ever, which will be added to the ſcale below the inferior

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délicat, et où l'on ne voudra pas la branche ſi longue, diviſer l'intervalle fondamental en 15 parties, qui ſeront alors des doubles dégrés.

Ayant ainſi des point fixes et des dégrés déterminés ſur l'Aréomètre, il faut choiſir une place commode pour ſon zero; et le mieux eſt de le placer de manière que toutes les épreuves des liqueurs puiſſent être exprimées avec le même ſigne. Pour cet effet on pourra prendre un des vins donc on fait le plus communément l'eau de vie, et le réduiſant à la température de l'eau dans la glace, y plonger l'Aréomètre, obſervant de combien il s'enfoncera de moins que le point fixe inférieur. Ce ſurplus d'émerſion, comparé à l'intervalle fondamental, et réduit au nombre le plus prochain de dégrés qui ſe trouvera une partie aliquote de cet intervalle, ſera une quantité fixée pour toûjours, qu'il faudra ajouter à l'echelle au deſſous du

ferior fixed point, and determine the place of o. I ſuppoſe, for inſtance, that this exceſs of emerſion ſhould be about 15°, or the half of the fundamental ſcale: I would then fix it at this number; and in that caſe one ſhould conſtantly add half the fundamental diſtance below the inferior fixed point, and from theſe begin to count the degrees. I mean that the o would be at the bottom of the whole ſcale, the inferior fixed point would be at 15°, the ſuperior at 45°, and the ſcale could be prolonged at the top as much as ſhould be neceſſary for the eſſays of the moſt ſpirituous liquors. It is after this manner that the o of FAHRENHEIT's Thermometer is now determined; and that being placed at 32° below the inferior fixed point, the greater part of the obſervations are expreſſed upon it in poſitive degrees; ſo that it is only in extraordinary caſes that they are affected with the *minus* ſign.

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du point fixe inférieur. Je ſuppoſe par exemple que ce ſurplus d'emerſion ſe trouvât de près de 15°, ou de la moitié de l'échelle fondamentalle; je le fixerois à ce nombre; et ainſi j'ajouterois toûjours une moitié de l'intervalle fondamental au deſſous du point fixe inférieur, pour commencer de là à compter les degrés de l'echelle. Ainſi le o ſeroit tout au bas, le point fixe inferieur ſeroit à 15°; le point fixe ſupérieur à 45°; et l'echelle ſeroit prolongée dans le haut autant qu'il ſeroit néceſſaire pour fournir à l'eſſai des liqueurs les plus ſpiritueuſes. C'eſt ainſi que le zéro du Thermomètre de FAHRENHEIT eſt à preſent déterminé, et que ſe trouvant ainſi placé à 32° au deſſous du point fixe inférieur, la majeure partie des obſervations y eſt exprimée en dégrés poſitifs; et qu'il faut des cas extraordinaires pour qu'elles ſoyent accompagnées du ſigne *moins*.

fign. It would be convenient to pay a regard to this in all inftruments, when no other reafons interfere.

I come now to the correction for the differences of the heat. I would take a liquor of mean fpirituofity, as for inftance a mixture of one part of water and feven parts of the fpirits of wine determined by the hydroftatic balance: into this liquor, reduced to the temperature of 45° of FAHRENHEIT, I would plunge the Areometer already graduated, and obferve the point to which it finks. I would afterwards heat the liquor to 65°, and again obferve the finking of the Areometer. One might likewife make ufe of the fcale of my Thermometer, and obferve the finkings at 5 and 15 of my degrees, which would come fenfibly to the fame.

This

moins. Il feroit commode d'avoir égard à cela dans tous les inftrumens, quand rien d'allieurs ne s'y oppofe.

Je viens à la correction pour les différences de la chaleur. Prenant une liqueur de fpirituofité moyenne, comme par exemple le mélange d'1 partie d'éau à 7 parties de l'efprit de vin fixé par la balance hydroftatique, je plongerois l'Aréomètre, déja gradué, dans cette liqueur réduite à la température de 45° de FAHRENHEIT, et j'obferverois le point où il s'enfonceroit: j'échaufferois enfuite la liqueur à 65°, et j'y obferverois encore l'enfoncement de l'Aréomètre. On pourroit auffi, en employant l'échelle de mon Thermomètre, obferver les enfoncemens à 5 et à 15 de mes dégrés, ce qui reviendroit fenfiblement au même.

Cette

This obſervation being made, one may conceive that it would be eaſy to form a table in which one might expreſs, in degrees of the Areometer, the effects of the differences of heat correſponding to each degree of one or other of the Thermometers, ſetting out from a fixed point; ſince the effect correſpondent to each degree of the Thermometer, will be looked upon as proportionate to that which ſhall have been found in the fundamental obſervation.

But I would prefer another method, which I have recommended in my work[(g)], becauſe I have found it of great uſe in practice; that is, to make a particular ſcale for the Thermometer intended for theſe experiments, by changing the number of the degrees contained between the

(g) Vol. I. p. 390.

Cette obſervation faite, on comprend qu'il ſeroit aiſé de former une table, dans laquelle on exprimeroit, en dégrés de l'Aréomètre, les effets des différences de chaleur, correſpondans à chaque dégré de l'un ou de l'autre des Thermomètres, à partir d'un point fixe; car l'effet correſpondant à chaque dégré du Thermomètre, ſera regardé comme proportionnel à celui qu'on aura trouvé dans l'obſervation fondamentale.

Mais je préférerois une autre méthode, que j'ai recommandée dans mon ouvrage *(g)*, parce que je l'ai trouvée d'une très grande commodité dans la pratique; c'eſt de faire une échelle particulière pour le Thermomètre deſtiné à ces épreuves; en changeant le nombre des degrés renfermés entre ſes points fixes, pour

(g) Tom. I. p. 390.

the fixed points, in order to eſtabliſh an eaſy proportion between them, and the degrees of the Areometer, and that thus one might make the correction without tables. It would be eaſy, for inſtance, to make the degrees of the Thermometer anſwer to quarters of degrees of the Areometer; for in that caſe, reckoning them from a fixed point, one would only have to correct the obſervations made upon the Areometer, by a quarter of the number of degrees indicated upon the Thermometer, which ſeems to me very convenient: and as it is always eaſier to add than to ſubtract, I would place the o of this Thermometer at the point of the greateſt ordinary heat of the air, or about 24° of my Thermometer, and 86° of FAHRENHEIT'S: for then, reckoning the degrees of the Thermometer downwards, one ſhould only add them to the indication of the Areometer; ſince the cooling of the liquor

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pour qu'ils euſſent un rapport ſimple avec ceux de l'Aréomètre, et qu'on pût ainſi ſe paſſer de tables. Il ſeroit fort aiſé, par exemple, que les dégrés du Thermomètre correſpondiſſent à des quarts de dégrés de l'Aréomètre; et alors, les comptant depuis un point fixe, on n'auroit qu'à corriger l'obſervation faite ſur l'Aréomètre, par le quart du nombre des dégrés qu'indiqueroit le Thermomètre; ce qui me paroitroit fort commode. Et comme il eſt toûjours plus aiſé d'additionner que de ſouſtraire, je placerois le zéro de ce Thermomètre au point de la plus grande chaleur ordinaire de l'air, c'eſt à dire aux environs de 24° de mon Thermomètre, ou 86° de FAHRENHEIT; car alors, comptant les dégrés du Thermomètre en deſcendant, il faudroit les ajouter à l'indication de l'Aréomètre;

liquor from this fixed point of temperature would leſſen the effect of the ſpirituoſity in the immediate indication of the Areometer, comparatively with what it ſhould be found at this determined point.

I have here alſo taken, for the compariſon of the indication of the Thermometer with the denſity of the ſame liquor differently warmed, terms which are within the extremes of the common obſervations, becauſe here ſeveral cauſes are combined in the ſame effect. 1. The progreſſion of the dilatations of ſpirituous liquids comparatively to quickſilver. 2. The different progreſſions of the liquids of different degrees of ſpirituoſity. 3. The change of bulk of the inſtrument itſelf in liquors of different temperatures. It became then neceſſary to avoid taking the fundamental terms of compariſon very wide of each other,

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tre; puiſque le réfroidiſſement depuis ce point fixe de température, diminueroit l'effet de la ſpirituoſité ſur l'indication immédiate de l'Aréomètre, comparativement à ce qu'on la trouveroit à ce point déterminé.

J'ai pris encore ici pour la comparaiſon de l'indication du Thermomètre avec la denſité d'une même liqueur différemment chaude, des termes qui ſe trouvent en dedans des extrêmes des obſervations ordinaires; parce qu'ici pluſieurs cauſes ſe combinent dans un même effet, ſavoir, 1°. La marche des dilatations des liqueurs ſpiritueuſes comparativément au mercure. 2°. La différence de marche des liqueurs de différent dégré de ſpirituoſité. 3°. Les changemens de volume de l'inſtrument lui même dans les liqueurs différemment chaudes. Il falloit donc éviter de prendre les termes fondamentaux de comparaiſon à une grande diſtance, de peur de

other, left the error arising from considering the immersions of the Areometer, occasioned by the changes of temperature of the liquor, as being exactly proportionate to the indications of the Thermometer, should thereby be rendered sensible.

Every part of the Instrument being thus determined, it will be easy to construct it every where in an uniform manner. Experiments will then be made, and the degree of spirituosity which liquors in trade, under certain denominations, ought to have, will be fixed: *spirit of wine*, for instance, *brandy* named *three-fifths* in Languedoc, that which is called *proof of Holland*, or any other. These points being known, as the standard of the precious metals fixed by the different States that coin money, there will then be established a proportionate value of the *degrees of spirituosity*, which should be found

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de rendre sensible l'erreur qui resultera toûjours de considérer les enfoncemens de l'Aréomètre provenans des variations de la chaleur de la liqueur, comme exactement proportionnels aux indications du Thermométre.

Toutes les parties de l'Instrument étant ainsi déterminées, il sera aisé de le construire partout d'une manière uniforme. On fera alors des expériences, et l'on fixera à certains points de l'Aréomètre, le dégré de spirituosité que devront avoir les liqueurs attendues dans le commerce sous certaines dénominations; *l'esprit de vin*, par exemple, *l'eau de vie* nommée *trois quints* en Languedoc, celle qu'on nomme *à l'épreuve de Hollande*, ou telle autre: et ces points étant connus, comme on connoit les titres des métaux précieux fixés par les divers Etats qui battent monoye, il s'établira aussi une valeur proportionnelle des *dégrés de spiritu-*

 osité

found more or lefs than the expected degree, as there is a price for the *karat* of gold or *denier* of filver, by which the feller and buyer might always be able to do themfelves juftice. For inftance, every degree lefs than the point fixed for the common *fpirit of wine* would be $\frac{1}{70}$ to be made good by the vender, that is about $1\frac{1}{2}$ *per cent.* in the language of trade, and 1 *per cent.* only on *brandy* named *three-fiths.*

When this Areometer fhould have come into general ufe, the Police of the places in which the trade of fpirituous liquors is carried on, might take cognizance of it, and keep the public ftandard of the Areometer, as they keep the ftandards of weights and meafures. The infpectors of that trade would thus have fixed modes of effaying, and the public all the neceffary fecurity.

There

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efité de plus ou de moins que le dégré attendu; comme il y a un prix pour le *karat* de l'or et le *denier* de l'argent:, par où le vendeur et l'acheteur pourront toûjours fe faire juftice. Par exemple, chaque dégré de moins que le point fixé pour l'*efprit de vin* ordinaire, feroit $\frac{1}{70}$ à bonifier par le vendeur, ou environ $1\frac{1}{2}$ pour cent en terme de commerce; et 1 pour cent feulement fur l'*eau de vie trois quints.*

Quand cet Aréomètre feroit devenu d'un ufage un peu général, la Police des lieux où fe fait le commerce des liqueurs fpiritueufes, pourroit en prendre connoiffance, et conferver l'étalon public de l'Aréomètre, comme elle tient en dépôt ceux des mefures et des poids. Les infpecteurs prépofés auroient ainfi des épreuves fixes, et le Public toute la certitude néceffaire fur cet objet.

Il

There would be little advantage, with reſpect to exactneſs, in making of ſpirit of wine the Thermometer intended for theſe eſſays; though its variation would be more exactly conformable to the effects of heat upon liquors of the ſame ſpecies: for it has been ſeen how far, by the method I propoſed, the differences vaniſh; and, on the contrary, there would be a loſs on two accounts: the one, that this Thermometer would be much leſs ſenſible than the quickſilver one; the other, that it is much more difficult to conſtruct it, when one wants to make it upon fixed principles, and this the rather as good workmen have loſt the habit of making them.

One ſees, moreover, that the ſame Areometer may (*mutatis mutandis*) be uſed to meaſure the ſaltneſs of water. Upon which I ſhall only obſerve, that the manner indicated of fixing the correction for the heat, would be ſtill more

Il y auroit peu à gagner pour l'exactitude, à faire d'eſprit de vin le Thermomètre deſtiné à ces épreuves, quoique ſa marche fût dans le fond plus conforme aux effets de la chaleur ſur des liqueurs de même eſpèce; car on a vu à quel dégré la méthode que je propoſe a fait diſparoitre les différences: et il y auroit à perdre au contraire à deux égards, l'un que ce Thermomètre ſeroit bien moins ſenſible que celui de mercure, l'autre qu'il eſt bien plus difficile à conſtruire, lorſqu'on veut le faire ſur des principes certains; d'autant plus que les bons ouvriers ont perdu l'habitude d'en faire.

On voit au reſte que le même inſtrument peut être employé à meſurer la ſalure de l'eau, *mutatis mutandis.* Sur quoi je ferai remarquer ſeulement, que la manière indiquée de fixer la correction pour la chaleur y ſeroit d'autant plus néceſſaire, qu'il y a plus de différence dans la marche des effets de la chaleur,

more neceſſary in that caſe, as there is a ſtill greater difference in the progreſſions of the effects of heat, between waters differently ſalted, than there is between liquors that have a different degree of ſpirituoſity; as may be ſeen by the experiments upon this ſubject which I have explained in my work (b).

Whatever approximation the method which I have applied to the conſtruction of the Areometer may give towards indicating, by equally diſtant degrees upon the inſtrument, equal differences of ſpirituoſity or ſaltneſs of the liquids into which it is dipped; it will ſtill be true that it will only ſhew equal differences in the ſpecific

(b) Vol. I. p. 271.—One may alſo ſee, in the deſcription of fig. 3, the method of applying this inſtrument to meaſure in general the ſpecific gravity of all liquids in which it can ſink.

gravity

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chaleur, entre les eaux différemment ſalées, qu'entre les liqueurs différemment ſpiritueuſes; comme on peut le voir par les expériences que j'ai rapportées à ce ſujet dans mon ouvrage (b).

Quelque approximation que fourniſſe la méthode que j'ai appliquée à la conſtruction de l'Aréomètre, pour indiqueur, par des dégrés également diſtans ſur l'inſtrument, des différences égales entr'elles de ſpirituoſité ou de ſalure des liquides dans leſquels on le plongera; il ſera toûjours vrai ſans doute, qu'il ne montrera exactement que des differences égales dans la peſanteur ſpécifique de ces

(b) Tom. I. p. 271.—Voyez auſſi dans la deſcription de la fig. 3. le moyen d'employer cet inſtrument pour meſurer en général la peſanteur ſpécifique des liquides où il peut s'enfoncer.

liquides,

gravity of the liquids, to which the equal differences of saltness or spirituosity will not exactly answer.

But the instrument being constructed upon fixed principles, one might afterwards seek for the true laws which the different intensities of these causes follow, when the changes in the specific gravity are equal between them; as I have already done (from an idea of Mr. LE SAGE's) for the real differences of heat correspondant to degrees equally distant upon the Thermometer (i); a determination which would be useful in the particular cases in which the approximation given by the instrument would not be sufficient.

I have not yet executed this instrument; nor indeed is it necessary that I should undertake to do it, in a country where

(i) Vol. I, p. 285.

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liquides, auxquelles ne correspondront pas aussi exactement des différences égales de salure ou de spirituosité.

Mais l'instrument étant construit sur des principes fixes, on pourroit ensuite chercher les vraies loix que suivent les différences d'intensité de ces causes auxquelles correspondent des changemens égaux entr'eux dans la pesanteur spécifique; comme je lai fait (d'après une idée de Mr LE SAGE) pour les différences réelles de chaleur qui produisent des dégrés également distans sur le Thermomètre (i); fixation qui serviroit dans quelques cas particuliers, où l'approximation fournie par l'instrument ne feroit pas suffisante.

Je n'ai pas encore pu exécuter cet Aréomètre; et il est peu nécessaire même que je l'entreprenne, dans un pays où tant d'artistes sont en état de me comprendre

(i) Tom. I. p. 285.

where ſo many artiſts will underſtand me by this deſcription, and may even ſupply what I have omitted. And ſhould any one be deſirous of undertaking it, I would willingly aſſiſt him by communicating to him ſome ideas for the execution, which would have made this paper too long.

Concluſion with reſpect to phyſical meaſures in general.

Though the Areometer is uſeful in itſelf, the chief reaſon of my dwelling upon it was to give an example of the general rule I have before eſtabliſhed.

Here are in this caſe only three phyſical effects, the degrees of which are not proportionate to their apparent cauſes, and which are united under the appearance of one ſingle

prendre ſur cette deſcription, et de ſuppléer même à ce que je pourrois avoir ommis: et je me ferois d'ailleurs un plaiſir d'aider le premier qui voudra l'entreprendre, en lui communiquant quelques idées de détail dans l'exécution, qui auroient trop allongé ce mémoire.

Concluſion ſur les Meſures phyſiques en général.

Quoique l'Aréomètre aït de l'utilité par lui même, je me ſuis principalement arrêté à expliquer ſes principes, pour donner un exemple de la règle générale que j'ai établie.

Voila, dans un ſeul cas, trois effets phyſiques dont les degrés ne ſont pas proportionnels à ceux de leurs cauſes, réunis même ſous l'apparence d'un ſeul, ſavoir

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ſingle effect, namely, the different ſinking of the Areometer. In the firſt place, it will not always ſink in liquors of different denſities in general, proportionally to theſe denſities, on account of the changes of its own bulk by heat, and the poſſible irregularity of its branch. Secondly, it will not ſink in proportion to the changes of temperature of the liquor, becauſe the changes of denſity of the latter will not follow the ſame law as the changes of temperature. I have already mentioned theſe two cauſes of error; but here is a third. The Areometer will not ſink exactly in the inverſe ratio of the quantities of flegm; becauſe the ſpecific gravity of the liquor does not follow the proportion of theſe quantities. It has an increaſing progreſſion; and here the immediate cauſe of this diſproportion, which is evident, may give us an idea of what takes place

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ſavoir l'enfoncement différent de l'Aréomètre. D'abord il ne s'enfoncera pas toûjours dans les liqueurs de différentes denſités en général, proportionnellement à ces denſités; à cauſe de ſes propres changemens de volume par la chaleur, et de l'irrégularité poſſible de ſon tube. Enſuite il ne s'enfoncera pas proportionnellement aux changemens de température de la liqueur; parce que les changemens de denſité de celle-ci ne ſuivront pas la même loi que les changemens de température. J'ai déja indiqué ces deux cauſes d'erreur; mais en voici une troiſième. L'Aréomètre ne s'enfoncera pas exactement en raiſon inverſe des quantités de flegme; parce que la peſanteur ſpécifique de la liqueur ne ſuit pas le rapport de ces quantités; elle a une marche croiſſante. Et ici, la cauſe prochaine de cette diſproportion, qui eſt évidente, peut nous donner une idée de ce qui ſe paſſe dans

place in Nature, and hinders phyfical effects from appearing proportional to their caufes, in our obfervations.

The fpirit and the flegm penetrate each other; that is to fay, the bulk of the mixture is a little lefs than the fums of the two bulks before the mixture; and fo the fpecific gravity, which is the weight or the quantity of matter under a certain bulk, increafes a little in the mixture, comparatively with the mean fpecific gravity of the component parts. This penetration feems to me to give us fome idea of the hidden caufes in bodies, which modify, unknown to us, the effects of the apparent caufes, and prevent the obferved effects being proportional to thefe.

One muft therefore, in order to have equal degrees in the Areometer, without fenfible error upon the fpirituofity

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dans la Nature, et qui empêche les effets phyfiques d'être proportionnels à leurs caufes dans nos obfervations.

L'efprit et le flegme fe pénètrent; c'eft à dire que le volume du mélange eft un peu moindre que la fomme des deux volumes avant le mélange: ainfi la pefanteur fpécifique, qui eft le poids ou la quantité de matière, fous un certain volume, augmente un peu dans le mélange, comparativement à la pefanteur fpecifique moyenne des compofans. Cette pénétration repréfente affez bien ce me femble les caufes cachées dans les corps, qui modifient à notre infu les effets des caufes apparentes, et empêchent que les effets obfervés ne leur foyent proportionnels.

Il faut donc, pour avoir des degrés égaux dans l'Aréomètre fans erreur fenfible fur la fpirituofité qu'il doit mefurer, fixer ces degrés par la comparaifon d'effets obfervés

ofity that it is intended to meafure, fix thefe degrees by the comparifon of effects obferved within the limits of the common obfervations: and this is the fecureft way in practice; for how could one make a fcale follow all thefe different laws?

This is what I propofed to apply to phyfical effects of all kinds which have unequal degrees, by equal differences in the intenfity of their caufes, or by equal degrees of fome co-effect more eafily obferved, and which fhould be made ufe of to determine the other.

In order to make the advantage of this method more confpicuous, I will now apply it to the co-effects the moft different which exift perhaps in Nature, I mean the augmentations of the bulks of quickfilver and water by the fame augmentations of heat.

I will

obfervés en dedans des limites des obfervations ordinaires; et c'eft le chemin le plus fûr dans la pratique; car comment pourroit on faire fuivre à une échelle toutes ces différentes loix.

Voilà ce que je me propofois d'appliquer aux effets phyfiques de tout genre, qui ont des degrés inégaux, par des différences égales d'intenfité de leurs caufes, ou par des degrés égaux de quelque co-effet, plus aifé à obferver, et qui devroit fervir à déterminer les autres.

Pour rendre l'utilité de cette méthode plus frappante, je vais l'appliquer aux co-effets les plus difparates peut-être qu'il y aît dans la Nature; je veux dire les augmentations de volume du mercure et de l'eau, par les mêmes augmentations de la chaleur.

I will only put the ſame caſes I have explained before for ſpirituous liquors: the firſt, in which the actual trial has been made at 80° of the Thermometer, the ſecond, in which it is ſuppoſed to be made at 20°, both compared with 0; and the third, in which the trial is made at 5° and 15°. The deviations of the three caſes between the temperatures of 0 and 20° (reputed to be the limits of the common obſervations) are as follow:

1ſt caſe.

Je poſerai ſimplement les mêmes cas que j'ai expliqués ci-devant pour les liqueurs ſpiritueuſes; le premier où l'épreuve actuelle a été faite à 80° du Thermomètre; le ſecond où elle eſt cenſée faite à 20°; l'un et l'autre comparativement à 0: et le troiſième où cette épreuve eſt faite à 5° et à 15°. Les écarts des trois cas, 3 entre les températures de 0 et 20°, cenſées être les limites des obſervations ordinaires, ſont comme ſuit.

1^e^ cas.

1ſt caſe.		2d caſe.		3d caſe.	
Therm.	Dil. of water.	Therm.	Dil. of water.	Therm.	Dil. of wat.
80	80				
...	....				
20	4,1	20	20	20	27,5
15	1,6	15	7,8	15	15
10	0,2	10	1	10	8
5	−0,4	5	−1,9	5	5
0	0	0	0	0	+7

In the change of the expreſſion of the dilatations of the water in the third caſe, as in the correſponding caſe for the ſpirituous liquors above mentioned, it was neceſſary to conſider as 0 or *x* the bulk of the matter correſponding with 5° upon the quickſilver Thermometer, ſince it is with

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1er cas.		2d cas.		3me cas.	
Therm.	Dilat. de l'eau.	Therm.	Dilat. de l'eau.	Therm.	Dilat. de l'eau.
80	80				
....	....				
20	4,1	20	20	20	27,5
15	1,6	15	7,8	15	15
10	0,2	10	1	10	8
5	−0,4	5	—1,9	5	5
0	0	0	0	0	+7

Dans le changement de l'expreſſion des dilatations de l'eau au 3me cas, comme dans le cas correſpondant ci-devant pour les liqueurs ſpiritueuſes, il a fallu d'abord conſidérer comme *zéro* ou *x* le volume de l'eau correſpondant à 5° ſur le Ther-

with this point that its bulk at the temperature 15° is compared. Making afterwards equal to 15 − 5 = 10 the number of the equal parts which meaſure the augmentation of the bulk of water, inſtead of 1,6 + 0,4 = 2, which was the number in the firſt caſe taken from the experiment, I have changed all theſe terms in the proportion of 2 to 10, which has preſerved the ſame proportions between them. After this the expreſſion of the Thermometer continuing the ſame, that is, its 0 or *x* remaining 5° lower than the inferior points of the actual compariſon, in order to have, without calculation, the deviations within and without thoſe points of compariſon, it was neceſſary to add 5 to all the firſt numbers which expreſs the real dilatations of the water. I might have ſubtracted 5 from each indication of the Thermometer, which would have come to the ſame. It will be eaſily

Thermomètre de mercure; puiſque c'eſt avec ce point que ſon volume à la température 15° eſt comparé. Faiſant enſuite égal à 15 − 5 = 10 le nombre des parties égales qui meſure l'augmentation de volume de l'eau, au lieu de 1,6 + 0,4 = 2 qu'étoit ce nombre dans le premier cas tiré de l'expérience, j'ai changé tous les termes dans le rapport de 2 à 10; ce qui a conſervé les mêmes proportions entr'eux. Après quoi, l'expreſſion du Thermomètre reſtant la même, c'eſt à dire ſon *zéro* ou *z* reſtant de 5° plus bas que le point inférieur de comparaiſon actuelle; pour avoir ſans calcul les déviations au dedans et au dehors de ces points de comparaiſon, il a fallu ajouter 5 à tous les nombres qui expriment les dilatations réelles de l'eau. J'aurois pu retrancher 5 à chaque indication du Thermomètre, ce qui ſeroit revenu au même. On verra aiſément je crois que c'eſt là la route

easily seen, I believe, that this was the road to follow in order to transpose, in the third case, those proportions found by experiment, which immediately constitute the second. I proceeded in the same manner in the example drawn from the two spirituous liquors. As to the second case, as well for these liquors as for the water, it is evident, that the change of the scale which measures their dilatations, occasions no change in the proportions of the terms found by experiment.

I repeat it, I do not believe one has ever observed, in any case, two co-effects of the same cause which follow more disproportioned progressions, than these dilatations of quicksilver and water by the same augmentations of heat: and yet one sees, that by this method (I mean by observing the real proportions of the co-effects within the ordinary limits of the intensities of the causes) one

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route qu'il falloit suivre, pour transporter dans le 3^{me} cas, ces rapports trouvés par l'expérience qui forment immédiatement le premier. J'ai procédé de la même manière dans l'exemple tiré des deux liqueurs spiritueuses. Quant au second cas, tant pour ces liqueurs que pour l'eau, il est évident que le changement de l'échelle qui mesure leurs dilatations, n'en apporte aucun dans le rapport des termes trouvés par l'expérience.

Il ne me semble pas, je le répéte, qu'on aît observé en aucun cas, deux co-effets d'une même cause qui suivent des marches plus disproportionées que ces dilatations du mercure et de l'eau par les mêmes augmentations de la chaleur; et cependant on voit que par cette méthode (je veux dire en prenant par observation des rapports des co-effets au dedans des limites ordinaires des intensités des causes) on

one leſſens much the errors in the other terms, which will reſult from ſuppoſing them to be proportionate to the obſerved proportions; and that one procures a ſenſible exactneſs near the points of actual trial, which are at the ſame time near the greateſt number of the caſes of practice for which one wiſhes to find meaſures.

And if one conſiders co-effects in general, ſetting aſide this extreme diſparity, one will perhaps ſeldom meet with any, which follow laws more different than the correſponding dilatations of quickſilver and brandy: even very frequently they will not deviate more than thoſe of brandy and ſpirits of wine; and in that caſe it has been ſeen, that this method reduces ſo much the deviations by throwing them out of the limits of the ordinary caſes, that it may be uſed without ſenſible error, when thoſe co-effects

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on diminue beaucoup les écarts qu'on fera dans les autres termes en les ſuppoſant proportionnels aux rapports obſervés; et qu'on ſe procure même ſenſiblement l'exactitude, aux environs des points d'épreuve actuelle, qui ſont en même tems les plus près du plus grand nombre des cas de pratique pour leſquels on cherche des meſures.

Et ſi l'on conſidère les co-effets en général, mettant à part cette extrême diſparité, on en trouvera peut-être rarement qui ſuivent des loix plus différentes que les dilatations correſpondantes du mercure et de l'eau de vie; très ſouvent même ils ne s'écarteront pas davantage que celles de l'eau de vie et de l'eſprit de vin; et alors on a vu, que cette méthode y réduit tellement les écarts, en les rejettant hors des limites des cas ordinaires, qu'on pourra l'employer ſans erreur ſenſible,

co-effects are not followed in all their degrees. Whilst, on the other hand, the method of taking the fundamental proportions in points which are very distant, under the idea of lessening the effects of the errors, is exactly that which accumulates a greater quantity of them upon the intermediate cases, which are the most frequent, and often the only ones in the which there is need in practice of knowing the co-effects by one another.

One must not, therefore, seek the power of the Thermometer which corrects watches, invented by the immortal HARRISON, by trying it in the temperatures of artificial congelation and in a stove; for that is the way of destroying a great part of its correcting effect, in the very cases wherein it is most necessary, by accumulating on them the deviations of two co-effects, probably very little proportional, namely, the changes of the elastic force of

sensible, quand on n'aura pas suivi ces co-effets dans tous leurs degrés. Tandis que celle de chercher leurs rapports en des points fort éloignés, dans l'idée de diminuer l'effet des erreurs, est précisément le moyen d'en accumuler le plus sur les cas intermédiares, qui sont les plus fréquens; et souvent les seuls où l'on aît besoin de connoitre les co-effects les uns par les autres.

Il ne faut donc pas, par exemple, chercher le pouvoir du Thermomètre correcteur des montres, inventé par l'immortel HARRISON, en l'éprouvant dans les températures d'une congélation artificielle et d'une étuve: car c'est le moyen de lui ôter une grande partie de cet effet correcteur, dans les cas où il est le plus nécessaire; puisque c'est y accumuler les écarts de deux co-effets probablement très peu proportionnels; savoir les changemens de force élastique d'un ressort spiral,

of a ſpiral ſpring, combined with all the other alterations heat produces in a watch, and the different degrees of bending of a lamella compoſed of two metals differently dilatable by heat. I am apt to believe, that a part of the irregularities which ſtill continue in theſe watches with correcting Thermometers, proceed from not having tried their effects within the limits of the natural temperatures to which the watches are expoſed.

For the ſame reaſon it will not be proper to uſe very great differences of heat in the experiments intended to find the required combination of the two ſubſtances in the pendulum: on the contrary, it will even be better to make them within the limits of the natural variations of heat which the pendulum will meet with in its place: for by that means, though theſe ſubſtances have not probably

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ſpiral, combinés avec toutes les autres altérations que produit la chaleur dans une montre, et les différentes courboures qu' éprouve une lame compoſée de deux métaux differemment dilatables par la chaleur. Auſſi ſuis-je porté à croire, qu'une partie des irrégularités qui reſtent encore dans ces montres à Thermomètres correcteurs, viennent de n'avoir pas cherché leurs effets au dedans des limites des températures naturelles où les montres ſont expoſées.

Par la même raiſon il ne faudra pas employer de très grandes différences de chaleur dans les expériences deſtinées à trouver la combinaiſon convenable des deux matières dans le pendule; et au contraire il conviendra de les faire en dedans même des limites des variations naturelles de chaleur que le pendule éprouvera à ſa place: car par là, quoique ces matières n'ayent probablement pas la

bably the ſame progreſſion by heat, one will not perceive in practice the effects of their differences.

One muſt not neither, from the compared dilatations of air and quickſilver in paſſing from the freezing to the boiling point, conclude the relation of the denſities of the air in the atmoſphere with the height of the quickſilver in the Thermometer. For here, as in the compariſon between ſpirituous liquors and quickſilver, we have a double error to guard againſt, that which may ariſe from the differences in the progreſſion of all air and quickſilver by the variations of the heat, and that which more or leſs exhalations and vapours certainly do produce in the dilatations of the former. I believe, therefore, that to confine one's ſelf, in ſeeking for a common rule, within the limits of the moſt frequent natural variations of heat, and obſerving their effects in the atmoſphere itſelf, will be the

la même marche par la chaleur, on ſera ſenſiblement à l'abri des effets de leurs differences.

Il ne faudra pas non plus chercher, par les rapports des dilatations de l'air et du mercure en paſſant de la glace à l'eau bouillante, ceux des denſités de l'air dans l'atmoſphère avec la marche du Thermomètre. Car ici, comme dans la comparaiſon des liqueurs ſpiritueuſes au mercure, nous avons double erreur à prévenir: celle qui peut reſulter des différences dans les marches de tout air et du mercure par les variations de chaleur, et celle que produiſent ſurement dans la marche du premier, le plus ou le moins de vapeurs et d'exhalaiſons qu'il contient. Se renfermer donc, pour la recherche d'une regle commune, dans l'étendue des variations de chaleur les plus fréquentes, en obſervant leurs effets dans l'atmoſphère

the fureft mean of diminifhing the errors, till fuch time as one fhall be able to follow thefe variations of denfity though all their caufes; enquiries worthy the greateft care of natural philofophers.

For the fame reafons it will not be in the greateft and leaft degrees of heat in the atmofphere that we muft take the fundamental proportions of the refractions with the Thermometer: for unlefs one was likewife to determine by experiment fome of the intermediate proportions, one would probably be expofed to very great errors; confidering firft, in general, that the changes of the denfity of air by heat may poffibly, as I have juft faid, not obferve the fame law as thofe of the quickfilver in the Thermometer; confidering likewife that the changes of the denfity of the atmofpherical air by heat are probably accom-

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fphère même, fera je crois le moyen le plus fûr de diminuer les erreurs, jufqu'à ce qu'on foit en état de fuivre pas à pas ces variations de denfité par toutes leurs caufes; recherches dignes du plus grand foin des phyficiens.

Par les mêmes raifons il ne faudra pas chercher dans les plus grandes et les moindres chaleurs de l'atmofphère, le rapport des réfractions avec le Thermomètre: car à moins de déterminer auffi par l'expérience quelques uns des rapports intermédiares, on feroit probablement fujet à de très grandes erreurs: vu d'abord en général, que les changemens de denfité de l'air par la chaleur, pourroient bien, comme je viens de la dire, ne pas fuivre la même loi que ceux du mercure dans le Thermométre: vu encore que les changemens de denfité de l'air atmofphérique

accompanied with a change of its nature, by the mixture of vapours and exhalations, which may occasion great variations in the law of dilatations; considering above all, that the changes of refringent power and of density are two co-effects of very different nature, the progressions of which may differ more, than those of the densities alone in different bodies. Here then are complications of complications, which may very likely accumulate errors in the intervals of the proportions furnished by experience between the refractions and the indications of the Thermometer, if those proportions were taken in points very far distant. The application of the theory of refractions to the practice of astronomy is as delicate as important, and cannot be viewed in too many lights: which determines me not to insist farther here.

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sphérique par la chaleur, sont probablement accompagnés de changement dans sa nature par le mélange des vapeurs et des exhalaisons, ce qui peut rendre la loi de ses dilatations très variable; vu surtout que les changemens de vertu refringente et de densité, sont deux co-effets de nature bien differente, et dont les marches peuvent s'écarter davantage, que celles des densités seules dans differens corps. Voilà donc des complications de complications, qui pourroient bien accumuler des erreurs dans l'intervalle des rapports fournis par l'expérience entre les réfractions, et les indications du Thermomètre, si ces rapports étoient pris en des points fort éloignes. L'application de la théorie des refractions à la pratique de l'astronomie, est aussi délicate qu'importante, et l'on ne sauroit l'envisager

here on this object, but to consider it by itself in another Paper.

As to physical co-effects in general, and I dare assert it here, in co-effects of all kinds, if one cannot fix all their relations, degree by degree, by immediate and sure observations, one must avoid deducing general rules from relations taken in the extremes. The action of causes, moral as well as physical, whether from the variety of the subjects on which they act, whether from secondary ones which escape our observations, is too complicated, for the observable modifications to increase in the exact proportion of the evident causes; and consequently for the co-effects of these to be proportionate between themselves.

I shall

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sager par trop de faces; ce qui me détermine à ne pas m'étendre d'avantage ici sur cet objet, pour la traiter à part dans un autre Mémoire.

Quant aux co-effets physiques en général, et j'ose le dire ici, dans les co-effets de tout genre, si l'on ne peut pas fixer tous leurs rapports degré par degré par des observations immédiates et sures, il faut éviter de tirer des règles générales, de rapports pris dans les extrêmes. L'action des causes, tant morales que physiques, est trop compliquée, soit par la variété des sujets sur lesquels elles agissent, soit par des causes sécondaires qui échappent à nos observations, pour que les modifications observables croissent en proportion exacte des cause; évidentes; et par conséquent, pour que les co-effets de celles-ci soyent proportionels entr'eux.

Je

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I shall now collect the results of the preceding reflexions with regard to physical measures.

When the inquiry is into general causes, such as heat, the electric fluid, *humor*, light, the weight of the air, the fall of bodies, percussion, &c. causes concerning which we never acquire sufficient light, we must endeavour to find out what are their most simple effects, in order to measure the intensity of them by those effects. In that case it is proper that the fixed terms of the measure be taken at the greatest possible distances. For it being the most simple effect, and consequently that which approaches nearest to follow, by its degrees, those of the intensity of the cause, it will serve as a common measure for all the other effects dependant on it. One must, therefore, ascertain the uniform

Je vais maintenant rassembler ici les résultats des réflexions précédentes à l'égard des Mesures physiques.

Lorsqu'il s'agira de causes générales, comme la chaleur, le fluide électrique, l'*humor*, la lumière, le poids de l'air, la chute des corps, les chocs, &c. causes sur l'action desquelles nous n'acquerrons jamais assez de lumières, il faut chercher quels sont leurs effets les plus simples, afin de mesurer leur intensité par ces effets. Alors sans doute il convient que les termes fixes de la mesure soyent pris aux plus grandes distances possibles. Car s'agissant de l'effet le plus simple, et par conséquent le plus approchant de suivre par ses degrés ceux de l'intensité de la cause, il servira de mesure commune pour tous les autres effet qui en dépendront. Il faut donc assurer la construction uniforme de la mesure; ce

form conſtruction of the meaſure, which cannot be more accurately obtained than by a great diſtance of the fixed points; and attempt, however, by every means poſſible, to find the proportions of this moſt ſimple and moſt regular effect, with its cauſe. It is on this account, that, in my treatiſe on the Thermometer, I have expoſed all the reaſons which lead me to believe that quickſilver is the body whoſe changes of bulk are moſt proportionate to the variations of heat which produce them, in order to aſſure to this liquid the preference as a common meaſure of heat: and that afterwards, as I have ſaid above, I looked for the proportions of its progreſſion with thoſe of heat itſelf.

But as to the co-effects which will be indicated by theſe meaſures of general cauſes, unleſs they can be determined degree after degree by experiment, and the objects

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ce qu'on obtient plus ſurement par une grande diſtance des points fixes; et chercher cependant par tous les moyens poſſibles les rapports de cet effet le plus régulier, avec ſa cauſe. C'eſt par ces raiſons que dans mon traité ſur le Thermomètre, j'ai raſſemblé tous les motifs qui me portent à croire que le mercure eſt celui des corps dont les changemens de volume ſont les plus proportionnels aux variations de la chaleur qui les produiſent; afin d'aſſurer à ce liquide la préfèrence pour la meſure commune de la chaleur: et qu'enſuite, comme je l'ai dit ci-deſſus, j'ai cherché les rapports de ſa marche avec celle de la chaleur elle-même.

Mais quant aux co-effects qui ſeront indiqués par ces meſures des cauſes générales, à moins qu'on ne puiſſe les déterminer degré par degré à l'aide de l'expérience,

objects are delicate enough to make this neceſſary, the ſafeſt, and at the ſame time moſt convenient, method will be always to keep within the limits of the natural caſes, to fix the fundamental points of the proportions; uſing for that purpoſe all the ſupplies of art and ſound logic to come as near to exactneſs as poſſible in fixing theſe baſes. It is this conſideration which ſeems to me to give ſome value to the method of aſcertaining the relative expanſibilities of bodies, which is the ſubject of the firſt part of this paper. If the co-effects are proportionate between them, there will be little loſt in not taking diſtant terms of compariſon, if they are taken exactly. If the co-effects are not proportionate, there will be much gain; and the leſs they are, ſo much the more.

We are obliged to take up with probability in Nature in ſo many reſpects, that it is perhaps of more importance

rience, et que les objets ſoyent aſſes délicats pour qu'il le faille, la méthode la plus ſure, et en même tems la plus commode, ſera toûjours de rentrer en dedans des limites des cas naturels, pour fixer les points fondamentaux des rapports; en employant tout ce que l'art et la bonne logique peuvent fournir de ſécours et de méthodes pour approcher le plus qu'il eſt poſſible de l'exactitude en fixant ces baſes. C'eſt cette conſidération qui me paroit donner du prix à la méthode de fixer les expanſibilités rélatives des corps, qui fait le ſujet de la première partie de ce Mémoire. Si les co-effets ſont proportionnels entr'eux, on perdra peu à ne pas prendre des termes de comparaiſon éloignés, pourvu qu'on les prenne avec exactitude. S'ils ne le ſont pas, on gagnera beaucoup; et d'autant plus, qu'ils le ſeront moins.

Nous ſommes obligés de nous contenter du probable à tant d'égards dans la Nature,

tance to us to inveſtigate the phyſical rules of probability than to attend to its mathematical rules upon hypotheſes.

EXPLANATION OF THE FIGURES.

FIG. I.

a a A rod of a ſubſtance little dilatable by heat (glaſs for inſtance) ſuſpended vertically.

b A bracket, from which hangs that rod.

c Point of ſuſpenſion of the rod. It is the point where the rod is free from the preſſure of the piece which keeps it

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Nature, que chercher les règles phyſiques de la probabilité, nous eſt peut-être plus eſſentiel, que de nous attacher à ſes règles mathématiques ſur des hypothèſes.

EXPLICATION DES FIGURES.

FIG. I.

a a Une branche d'une matière peu dilatable par la chaleur (de verre par exemple) ſuſpendue verticalement.

b Une pièce fixée quelque part, d'où pend cette branche.

c Point de ſuſpenſion de la même branche. C'eſt celui où elle ſe trove dégagée de

it ſuſpended; and it is from that point only that the length of the rod is reckoned. This is the rod which is called *fixed* in the paper.

dd A rod of a more dilatable ſubſtance than the former.

e Point at which the rods are connected, called in the paper *point of union* of the rods.

f Point marked upon the rod *dd* at the middle height of the rod *aa*.

g Another point upon the ſame rod, at the third part of that height.

The rod *dd* is the one which is called *free* in the paper. If then that *free rod* has a dilatability double of that of the *fixed rod*, the point *f* ſhall be *immoveable*, notwithſtanding the variations of the heat. If the firſt dilatability is triple, then the point *d* will be *immoveable*.

FIG.

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de la pièce qui la tient ſuſpendue; et c'eſt de ce point ſeulement que doit ſe compter la longueur de la branche. C'eſt celle qui eſt dite *fixée* dans le Mémoire.

dd Une branche d'une autre matière plus dilatable que la prémière.

e Point où les deux branches ſont goupillées enſemble, nommé dans le mémoire *point de réunion* des branches.

f Point marqué ſur la branche *dd* à la moité de la hauteur de la branche *aa*.

g Autre point marqué ſur la même branche, au tiers de la hauteur de l'autre.

La branche *dd* eſt celle qui eſt ditte *libre* dans le Mémoire. Si donc cette *branche libre* a une dilatabilité double de celle de la *branche fixée*, le point *f* ſera *immobile*, malgré les variation de la chaleur. Si la prémière dilatabilité eſt triple de la dernière, ce ſera le point *d* qui ſera *immobile*.

FIG.

FIG. II.

aa Stand to which the *Pyrometer* is ſuſpended.

b The hook from which it hangs.

ccc The deal-board which is the baſis of the whole apparatus.

dddd Four arms to which the frame *eeee* is fixed.

eeee The frame.

ssss Another frame, which carries the Microſcope.

gg Two croſs pieces, through which paſſes the tube of the Microſcope, and which ſupport it near both ends.

hh The Microſcope.

i Its Micrometer.

k Cork, through which paſſes the glaſs rod, and by which it is kept ſuſpended.

ll The

FIG. II.

aa Le ſupport auquel eſt ſuſpendu le *Pyromètre.*

b Le crochet d'où il pend.

ccc La planche de ſapin qui ſert de baſe à tout l'appareil.

dddd Quatre bras qui ſervent à porter le cadre *eeee.*

eeee Ce cadre.

ffff Le chaſſis qui porte le Microſcope.

gg Deux traverſes dans leſquelles paſſe le tube du Microſcope, pour le ſoutenir près de ſes deux bouts.

hh Le Microſcope lui même.

i Son Micromètre.

k Liège dans lequel la branche de verre eſt tenue.

ll La

ll The glaſs rod.

m A rod of metal, or of any other ſubſtance leſs dilatable than glaſs.

n Point of union, obtained by means of two connected rings, in which both rods are faſtened by ſcrews.

Higher up is another pair of rings, in one of which the metal rod is free, and which rod it ſupports.

op The piece to which the glaſs rod is ſuſpended.

q A ſquare piece fixed to the frame by four ſcrews, behind which is a box, in which, as well as in a groove cut in the baſis in *p*, the piece *op* ſlides.

r A ſcrew, which paſſes through the ſquare piece *q*, whoſe uſe is to move backwards or forwards the piece *q*, in order to bring the ſurface of the metal rod to the focus of the Microſcope.

ssss Four

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ll La branche de verre.

m La branche de métal, ou de toute autre ſubſtance plus dilatable que le verre.

n Le *point de réunion*, produit par deux anneaux accouplés, où chacune des branches eſt fixée par une vis.

Un double anneau ſemblable, mais où la branche de métal paſſe librement, ſe voit plus haut, et ſert à ſoutenir cette dernière branche.

op La pièce à laquelle la branche de verre eſt ſuſpendue.

q Une autre pièce fixée ſur le cadre par 4 vis, derrière laquelle eſt une boite où la piece *op* gliſſe fort juſte, ainſi que dans une mortaiſe faite à la planche *ccc* en *p*.

r Une vis qui paſſe au travers de la pièce *q*, et qui ſert à faire mouvoir la pièce *op* en avant ou en arrière pour amener la ſurface de la branche de métal au foyer du Microſcope.

ssss Quatre

ssss Four ſcrews, with round metal plates behind their heads, which ſerve to preſs the frame of the Microſcope againſt the frame *eeee*: the longitudinal openings, through which paſs the ſcrews, permit the free motion of the firſt frame, when one ſtrikes gently with a hammer to the bottom or the top of one of the ſides.

When one wants the Microſcope higher or lower than the grooves permit, one may change the ſcrews in other holes made on purpoſe in the ſide pieces of the frame *eeee*.

tttt The cylindrical bottle, in which hang the rods, in order to be heated at different degrees by water of various temperatures.

uu Supporters of the bottle.

x Ther-

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ssss Quatre vis, ayant des plaques de métal derrière leurs têtes, qui ſervent à preſſer le chaſſis du Microſcope contre le cadre *eeee*; ſans empêcher cependant que ce chaſſis ne puiſſe monter ou deſcendre (par le moyen des ouvertures longitudinales où paſſent les vis) en frappant des petits coups de marteau deſſous ou deſſus l'un des côtés.

Quand on a beſoin de placer le Microſcope plus haut ou plus bas que les couliſſes ne peuvent le permettre, on change les vis en d'autres trous qui ſont le long des montans du cadre *eeee*.

tttt La bouteille cylindrique dans laquelle pendent les branches pour y être échauffées à différens degrés, par de l'eau à différentes températures.

uu Supports de cette bouteille.

x Ther-

x Thermometer fuſpended in the water.

yy A rod, to the lower end of which is fixed a ſmall plate, to ſtir the water by moving it up and down.

zz A ſyphon, one branch of which is within, and the other without, the bottle, the latter with a cock; ſerving to draw off the quantity of water which is neceſſary for changing the temperature in the bottle.

FIG. III.

a The ball of the *Areometer*, which is of glaſs and empty, except

b The ſmall ciſtern at the bottom, which contains quickſilver.

cc The *branch*, made of a thin metal tube, cemented to the ball.

45,15 Two

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x Thermomètre ſuſpendu dans l'eau.

yy Baguette au bas de laquelle eſt une petite plaque, qui ſert à agiter l'eau en la faiſant mouvoir de bas en haut et de haut en bas.

zz Syphon, dont une des branches eſt dans la bouteille, et l'autre au dehors portant un robinet; ſervant à tirer de la bouteille la quantité d'eau neceſſaire aux changemens de degrès de chaleur.

FIG. III.

a Boule de l'*Aréomètre*, qui eſt de verre et vuide, excepté

b Le petit reſervoir rempli de mercure.

cc La branche, faite d'un tube mince de métal cimenté à la boule.

45,15 Deux

45,15 Two threads tied to the branch, which are the *fixed points* of the Areometer, as intended to try ſpirituous liquors.

The conſtruction of the whole ſcale is explained in the paper.

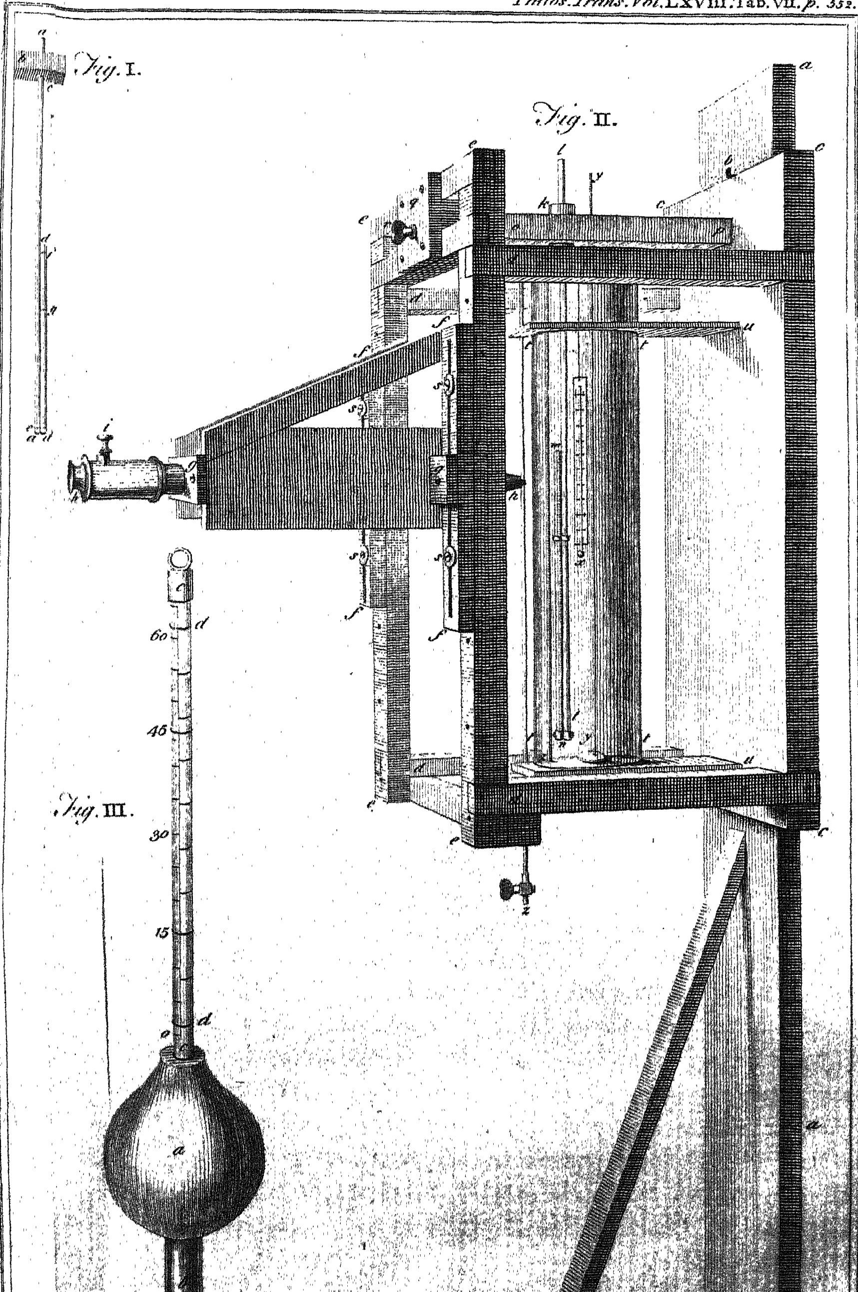
One may apply another ſcale on the oppoſite ſide of the *branch* (ſuch as the arbitrary ſcale in the figure) intended to try merely the ſpecific gravity of the liquids in which the Areometer may be dipped. The particular *fixed points* of this ſcale (as for inſtance *dd)* may be taken in two liquids whatſoever, whoſe ſpecific gravities, tried by the hydroſtatic balance, ſhall have a convenient relation; and the ſpace between thoſe two points will be divided into a convenient number of equal parts.

The

45,15 Deux fils attachés autour de la branche, qui ſont les *points fixes* de l'*Aréomètre*, comme deſtiné à l'épreuve des liqueurs ſpiritueuſes.

La conſtruction de toute cette échelle eſt expliquée dans le Mémoire.

On peut tracer de l'autre côté de la branche une autre échelle (comme l'échelle arbitraire de la figure) marquant ſimplement les peſanteurs ſpécifiques des liquides dans leſquels l'*Aréomètre* ſera plongé. Ses *points fixes* (comme par exemple *dd)* pourront auſſi être marqués par des fils dans deux liqueurs quelconque, où la balance hydroſtatique aura indiqué des peſanteurs ſpecifiques qui ayent entr'elles des rapports ſimples, dont la différence ſera diviſée enſuite en parties égales ſur l'échelle.



de Luc del.   Basire Sc.

The proportions are not determined in this figure, which ſerves only to help the explanation of the principles upon which a *comparable Areometer* might be conſtructed.

Il n'y a rien de déterminé dans les proportions de cette figure, qui ſert uniquement à rendre plus intelligible les principes ſur leſquels on pourroit conſtruire un *Aréomètre comparable.*

XXII. *Abtract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in Rutland*, 1777. *By* Thomas Barker, *Esquire*. *Communicated by Sir* John Pringle, *Bart. P. R. S.*

Read February 26, 1778.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest	Lowest	Mean.	High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	29,82	28,83	29,36	45½	26	36½	45	14	31	1,081
	Aftern.				46	28½	37½	47½	22½	36	
Feb.	Morn.	29,71	28,54	29,23	46	31	37	43½	14	30½	2,415
	Aftern				48	32	38	54½	24½	38	
Mar.	Morn.	29,72	28,49	29,31	56½	38	45	52	26½	38½	1,260
	Aftern.				61½	38½	46	68	35	48	
Apr.	Morn.	29,93	28,91	29,53	52	41½	45	52½	30	40	1,586
	Aftern.				54	42	47	61	43	50	
May	Morn.	29,79	28,84	29,33	59½	46	52½	59	40½	48½	1,981
	Aftern.				62½	51	54½	73	50	59½	
June	Morn.	29,90	29,12	29,48	62½	51½	57	61½	45	53½	2,966
	Aftern.				65	52½	58	70	46	62	
July	Morn.	29,91	28,74	29,42	67	56	60	63½	49	56	3,203
	Aftern.				71	56½	62	77	55	67	
Aug	Morn.	29,97	28,90	29,54	67	53	62	64	50	56	1,290
	Aftern.				68½	59	64	76½	55	67	
Sept.	Morn.	29,90	29,21	29,62	64½	53½	59½	59½	40	50½	0,507
	Aftern.				67½	55	61	74	53	65	
Oct.	Morn.	29,80	28,20	29,32	62	46	53	57	30	45½	4,009
	Aftern.				62	46½	54½	62½	43½	54	
Nov.	Morn.	29,90	28,60	29,48	54	41½	46	53	27	39	1,581
	Aftern.				54½	42½	47	58	35½	46	
Dec.	Morn.	30,00	28,55	29,36	44½	35½	39	44	27	33½	1,720
	Aftern.				45½	36	40	47½	30	37	
											23,599

The

The year began with hard froſt and a large ſnow, breaking ſometimes; but a ſevere ſeaſon, being chiefly froſt and ſnow till about the 20th of February; when, after wet and floods at its going away, it was mild the latter end of February and beginning of March: then cold again; ſhowery the middle of the month, and three days about Lady-day hotter than is uſual ſo early in the year, the thermometer abroad being 68. The ſpring ſeed time was fine, and the ground green, but not much graſs, the weather being in general windy and cold till near May-day: it then grew ſhowery, and though ſtill cool, graſs and grain came on very faſt. Once this year, about June 10th, after ſome dry hot weather, the ground began to burn; but from that time to the end of July, there was ſo much rain that the quantity of graſs and hay was very great, ſome of which was well got, though ſome was ſpoiled; the crops of grain were good, but too rank, and much laid. The harveſt was rather late, but moſt of it very well got, the latter part of the ſummer being clearer, hotter, and drier than it had been before, and it continued hot later than uſual, for the thermometer in the ſhade was 74, near the end of September. There was a great deal of rain in October and November, yet as the ground was dry before, and the rain came by fits, with fine weather between, the wheat feed time was

very favourable, and the ground continued in good order. The weather was open and fine till a little way in December; but a great part of that month was dark, calm, and moftly fair, and frequently fcarce either froft or thaw, and the year ended as it began with froft and fnow.

The earthquake which was felt September 14, in Chefhire, Lancafhire, &c. was obferved by fome people here, both the found and fhake; but as there was a ftrong wind at the time, neither of them were fo much taken notice of as they might probably otherwife have been.

I had a pocket-book lent me laft year, of a clergyman who formerly lived in this country, giving fome account of the weather of the wet year 1725, which I here tranfcribe.

"This year was cold and wet, having rained all the "time, except now and then a day, from the middle of "April till Auguft 27.

"The hay at firft came in ill, through a great flood "June 11 and 12, and almoft conftant rains; the mea-"dow grafs worth little; the rains being but fmall, it "came in better at laft.

"Harveft was very backward, though many, fancying "it would not, fold off their barley early, and were "forced to buy for their families for five or fix weeks at

"leaft:

" leaſt; they were deceived by the height of the corn " and grain, and coldneſs of the weather.

" Auguſt 23. A rain happened at Ketton feaſt of 24 " hours continuance, cauſed a flood on the meadows for " four or five days, ſo the herd had ſcarce a place to feed " on; the tethering graſs for the horſes was all ſpent, " and we were forced to take them to houſe till the corn " was off.

" All garden-ſtuff was a month later than ſome other " years; no berries on the hedges; no weather for pit-" coal carriage; no caterpillars, flies, &c. no kidney-" beans, or very few, being deſtroyed by ſnails and cold.

" The year being moſtly wet and cloudy, things tranſ-" planted wanted no water nor ſhading; no fruits were " well ripened; no grapes at all.

" In Auguſt, wheat between five and ſix ſhillings a " ſtrike (buſhel); barley above four ſhillings and ſix-" pence; maſlin five ſhillings; oats dear.

" Note. The year 1735 proved the ſame, in almoſt " all particulars."

REMARK BY T. BARKER.

I have heard a grazier of this town ſpeak of that year 1725. I think his account of it was this: that it was the warmeſt and forwardeſt ſpring he remembered, till April; peaſe

peaſe and other garden-ſtuff remarkably forward; and after that time the wetteſt ſummer he ever knew.

The forwardneſs of the ſpring at firſt might make the farmers expect an early harveſt, as the account above ſays they did.

The year 1723 was as dry as this was wet.

In the ſpring 1776, there was a remarkable quantity of ſeed upon the elms, of which I ſow'd a conſiderable parcel, both of the upright kind and of the witch elm. I believe, not above one ſeed in five hundred or a thouſand, grew; but thoſe which did made much finer plants than thoſe raiſed from ſuckers, eſpecially the upright kind, two of which were full five feet high in a year and half from ſeed, and as thick at the bottom as my finger. They were ſown on a north border, to keep them from the ſcorching ſun, but the flies deſtroyed ſome of the plants at their firſt coming up. However, notwithſtanding the uncertainty of their growing, I think to ſow ſome more of the ſeed, when there is any to be got.

XXII. *Journal of the Weather at* Montreal. *By Mr.* Barr. *Communicated by* Richard Saunders, *M. D. F. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

Brighton,
4th Sept. 1777.

Read Feb. 26, 1778.

SOME days ago I received the inclofed Journal of the weather, kept by Mr. BARR, for a few of the winter months, at Montreal, with a defire to put it into your hands. I underftand that the winter was thought particularly mild. The thermometer was FAHRENHEIT'S graduated 50 degrees below 0. I obferve that the wind never blew from the north; nor do I know that it ever blew from that quarter while I was in America.

I have the honour to be, &c.

R. SAUNDERS.

FOR DECEMBER, 1776.

Days of the Month.	Thermometer. Morning	Thermometer. Evening	Snow	Wind	Remarks.
	°	°	Inches		
14	+10	− 4		NE	Clear.
15	+ 2	+30	3	NW	Cloudy, the firſt fall of ſnow at night
16	+16	+ 2		NW	Clear.
17	− 4	+22		NW	Clear.
18	+20	+ 8		NW	Clear, the wind high.
19	− 2	at 0		NW	Calm, a fine calm morning.
20	+ 2	+20	a little	NE	Cloudy.
21	+24	+26	ditto	NE	Cloudy.
22	+24	+14	ditto	NW	Clear.
23	+15	+12		NW	Clear.
24	+ 4	+ 6		NW	Clear.
25	at 0	+ 8		NW	Clear, and a little wind. It is worth obſerving, that the wind is never high when the thermometer is low
26	+ 8	+20	ſome	NE	Cloudy.
27	+16	+10	ditto	NE	Cloudy.
28	+11	+16		NE	Cloudy.
29	+16	+18		E	Cloudy.
30	+16	+20	a little	NE	Cloudy.
31	+22	+30		S	Cloudy, ſome rain in the night.

FOR JANUARY, 1777.					
Days of the Month.	Thermometer. Morning	Thermometer. Evening	Snow	Wind	Remarks.
	°	°	Inches		
1	+32¼	+30		S	Cloudy, fome rain.
2	+20	+ 4		NW	Clear, the wind high.
3	− 2	+16		NW	Clear, little wind.
4	+14	+18	a little	E	Cloudy.
5	+ 8	+ 8	fome	NE	Clear.
6	+ 2	+ 2		NE	Clear.
7	at 0	+ 8	fome	NE	Cloudy.
8	at 0	+ 6		NW	Clear.
9	+ 6	+10		NE	Cloudy.
10	+16	+12		NW	Cloudy, little wind.
11	+ 6	+16		NW	Clear.
12	+14	+18		NW	Clear.
13	+14	+12		NW	Cloudy.
14	+ 2	+10	4	NW	Clear, little or no wind.
15	+10	+12	a little	NE	Cloudy.
16	+10	+ 8		NE	Clear.
17	− 2	at 0		NE	Cloudy.
18	− 6	+ 6		NW	Cloudy.
19	+10	+24	4	NW	Cloudy, a little fnow.
20	+30	+30		SW	Clear.
21	+20	+11		W	Clear.
22	+ 6	+ 2		NE	Clear.
23	+ 2	+20		NE	Clear, very little wind.
24	+30	+40	1	SE	Clear, in the morning fnow, and rain in the evening.
25	+40	+31		SE	Cloudy, fome rain.
26	+28	+30		SE	Cloudy.
27	+21	+24		NE	Clear.
28	+16	+18		NE	Clear.
29	+10	+20		NE	Clear.
30	+18	+22		NE	Clear.
31	+10	+16		W	Clear.

FOR FEBRUARY, 1777.					
Days of the Month.	Thermometer. Morning	Evening	Snow	Wind	Remarks.
	°	°	Inches		
1	+16	+ 2		NE	Clear, this night a little ſnow.
2	+32	+22		NE	Cloudy.
3	+12	+10		NE	Cloudy.
4	+ 2	+28	1½	W	Clear, ſnow in the night.
5	+32	+ 8		SW	Cloudy, ſome rain, wind high.
6	− 6	+ 6		NW	Clear.
7	+ 8	+20		NW	Clear.
8	+20	+22	2	SW	Cloudy.
9	+26	+28		NE	Cloudy.
10	+22	+30		SW	Clear.
11	+26	+24	½	NE	Cloudy.
12	+ 6	+ 4		NW	Clear.
13	at 0	+12			Clear, little or no wind.
14	− 2	+20		NE	Clear.
15	+ 6	+12		NE	Clear.
16	+ 6	+20		NE	Clear, a large circle round the moon this evening.
17	+10	+22		NW	Clear.
18	+12	+ 8		NE	Cloudy.
19	at 0	+ 8		NE	Clear, little wind.
20	+ 2	+12		NE	Cloudy, a little ſnow in the night.
21	+12	+30	4	NE	Cloudy.
22	+28	+32		NE	Cloudy.
23	+28	+30		SE	Cloudy.
24	+26	+28		NE	Cloudy.
25	+22	+24		E	Clear.
26	+22	+32		NW	Clear.
27	+22	+12	ſome	NW	Cloudy, very little wind, and weſterly in the evening.
28	+ 4	+ 8	2	NW	Clear.

FOR MARCH, 1777.					
Days of the Month.	Thermometer. Morning	Thermometer. Evening	Snow	Wind	Remarks.
	°	°	Inches		
1	− 4	+ 8		NW	Clear.
2	+ 2	+16		NE	Clear.
3	+ 8	+30		W	Clear.
4	+30	+10		W	Cloudy.
5	+ 6	+14		NW	Clear.
6	+12	+30		NW	Clear.
7	+28	+31		SW	Clear.
8	+26	+36		NW	Clear.
9	+32	+36		SW	Cloudy, with rain.
10	+36	+34		NW	Cloudy, with rain.
11	+28	+36		NW	Clear.
12	+36	+36		SW	Cloudy, with rain.
13	+28	+44		NW	Clear.
14	+32	+42		NW	Clear.
15	+38	+40		NE	Cloudy.
16	+38	+36		E	Cloudy, with thunder and rain.
17	+32	+32		NW	Clear.
18	+27	+32		NW	Clear.
19	+24	+32		NE	Clear.
20	+29	+36		E	Clear, wind high, and heavy rain, ice on the river begins to break up.
21	+33	+32	ſome	S	Cloudy.
22	+30	+27		W	Cloudy.
23	+34	+31	ſome	SW	Cloudy.
24	+32	+30		W	Cloudy.
25	+28	+24		NW	Cloudy.
26	+20	+21		NW	Cloudy.
27	+12	+14		NW	Clear.
28	+12	+18		NW	Clear.
29	+18	+24		W	Cloudy.
30	+28	+34	ſome	NW	Clear, ſome ſnow, with rain at night.
31	+30	+34		E	Cloudy.

XXIII. *Extract of Meteorological Observations made at* Hawkhill, *near* Edinburgh. *By* John M'Gouan. *Communicated by Sir* John Pringle, *Bart. P.R.S.*

Read February 26, 1778.

Lat. 55°. 58'. Long. 3°. 10½'.W. } Long. 12'. 42''. in time, per Astronomical Observations.

Fahrenheit's Thermometer.

		1773.			1774.		
Months	1st and 2d half	at 8 h.	A. M.	at 2 h. P. M.	at 8 h.	A	at 2 h. P. M.
January	1 2	39.06 38.06	38.56	40.30	28.46 29.75	29.10	33.00
February	1 2	32.14 38.00	35.07	40.75	34.14 38.28	36.21	40.43
March	1 2	40.46 43.66	42.06	48.45	34.06 40.18	37.12	43.26
April	1 2	42.40 48.80	45.60	51.10	43.13 43.13	43.13	48.90
May	1 2	44.33 52.81	48.57	53.13	46.73 46.50	46.61	50.84
June	1 2	54.13 56.26	55.19	60.06	54.80 55.40	55.10	59.70
July	1 2	56.33 59.06	57.70	61.93	57.40 57.50	57.45	63.32
August	1 2	60.40 56.12	58.26	64.77	58.13 56.37	57.25	62.21
September	1 2	53.26 49.33	51.29	55.83	52.40 51.00	51.70	57.80
October	1 2	47.20 44.87	46.03	50.67	51.06 45.50	48.28	52.84
November	1 2	41.53 34.93	38.23	42.33	40.63 35.50	38.06	42.00
December	1 2	35.60 37.25	36.42	38.48	37.40 37.25	37.32	39.97
Mediums of years at	8 h. A. M. and		46.08			44.86	
	2 h. P. M.			50.65			49.55

1775

		1775.			1776.	
Months.	1st and 2d half	at 8 h.	A. M.	at 2 h. P. M.	at 8 h.	A. M.
January	1	39.10	37.80	40.80	33.33	29.24½
	2	36.50			25.16	
February	1	37.64	39.07	43.96	36.32	35.66
	2	40.50			35.00	
March	1	39.80	40.05	44.32	37.60	40.86
	2	40.31			44.12	
April	1	44.83	46.83	53.35	43.60	45.90
	2	48.83			48.20	
May	1	52.60	52.74	59.61	47.36	49.29
	2	52.88			51.22	
June	1	55.66	56.60	60.43	55.00	55.50
	2	57.53			56.00	
July	1	58.20	59.13	67.53	58.16	59.36
	2	60.06			60.56	
August	1	59.10	57.65	63.67	58.60	56.73
	2	56.21			54.86	
September	1	53.20	53.26	58.87	55.00	51.80
	2	53.33			48.60	
October	1	48.86	45.30	50.22	48.60	46.99
	2	41.75			45.38	
November	1	38.00	37.96	41.10	45.80	40.97
	2	37.93			36.13	
December	1	41.16	38.58	41.48	42.54	37.77
	2	36.00			33.00	
Mediums of years at	8 h. A. M. and		47.08			45.84
	2 h. P. M.			52.11		

Depth

Depth of rain at Hawkhill, near Edinburgh, in perpendicular inches, for the year 1776.

				Inches
January	—	—	—	3.262
February	—	—	—	2.355
March	—	—	—	1.465
April	—	—	—	1.213
May	—	—	—	.626
June	—	—	—	2.367
July	—	—	—	3.075
Auguſt	—	—	—	2.410
September	—	—	—	2.755
October	—	—	—	1.735
November	—	—	—	2.750
December	—	—	—	2.080
			Total rain	26.093

XXIV. *Extract of a Meteorological Journal for the Year 1777, kept at* Briſtol, *by* Samuel Farr, *M. D.*

Read February 26, 1778.

Months.	Barometer.			
	Higheſt.	Loweſt.	Mean.	Viciſſitude.
January	30.18	29.26	29.83	+ 0.50–1
February	29.93	28.88	29.62	+ 0.43– ½
March	30.05	28.80	29.30	— 0.58– ½
April	30.26	29.30	29.84	— 0.47– ½
May	30.10	29.16	29.61	— 0.39–1
June	30.25	29.55	29.80	— 0.30– ½
July	30.29	29.30	29.84	+ 0.30– ½
Auguſt	30.27	29.35	29.89	— 0.35– ½
September	30.20	29.50	29.93	+ 0.28– ½
October	30.09	28.47	29.65	+ — 0.93–1½
November	30.28	29.04	29.86	— 0.58– ½
December	30.38	28.83	29.56	— 0.49– ½
				+ riſing. — falling.

An

An abridged Table of the WINDS, &c. for BRISTOL, for the Year 1777.

	N	E	W	S	NW	SE	NE	SW	Rain.	Frosty Days.	Fair Days.	Thunder, &c.
Jan.	2½	0	0	2	2½	10	11	4	0.996	12	3½	
Feb.	1	1½	½	1	2	3½	12	6½	1.353	8	10½	
Mar.	1½	1	0	2	1½	5½	11½	9	2.250	5	15	
Apr.	0	½	0	0	0	3½	17	9	1.962	3	13½	
May	½	½	½	3½	1	3	10	12	2.657	0	14	20. N. E.
Jun	½	0	½	3	4	0	7	15	1.838	0	13½	
July	5	0	5½	1½	4	4	4	7	3.285	0	11½	24. N.E. 29. N.E.
Aug.	½	1	0	1	2	3½	3	20	1.887	0	13	8. S. E. 28. N.E.
Sept.	1	3	½	1½	2	8	7	7	0.439	5	17½	27. S. E. 28. S.E.
Oct.	0	½	½	½	3	2½	9	15	3.602	6	14½	6. S. E. 30. S.
Nov.	1	½	½	2	3½	5	3½	14	2.141	8	10½	
Dec.	5	½	0	½	1½	1½	14	6½	0.959	12	7½	
	17½	10½	8½	17½	27	50	109	126	23.169	59	144½	

WEATHER FOR THE YEAR 1777.

January. A froſt to the 11th; wet to 14th, which was dry; 15th wet; 16th and 17th dry; 18th wet, then dry to 23d, and after to 27th; then dry to the end; 31ſt a froſt.

February. The 1ſt and 2d wet; the 3d, 4th, and 5th froſty, with ſnow on the night of the laſt; it was then dry

dry to the 10th; ſnow'd on 11th and 12th; it was then dry to 18th, when it ſnow'd again; and on 19th and 20th was dry; 21ſt it ſnow'd, and after that was wet to 28th.

March. Wet to 3d, that and 4th dry; 5th wet, then dry to 9th, and after to the 13th; then wet to 22d, and on 23d; it was then dry to 29th; the 30th was cloudy, yet dry; the 31ſt was wet.

April. There was no rain till 7th at night, nor after till 11th; 12th was dry; 13th ſtormy; 14th fair; 15th ſhowery; 16th and 17th dry; on 18th it ſnow'd; 19th fair; it was then wet to 26th, but after dry to 29th; 30th dry.

May. The 1ſt was dry; it was then wet to the 5th, but dry again to 9th; the 10th was fair, but afterwards it was wet to 19th; 20th was ſtormy, and wet continued to 26th; 27th was wet; 28th and 29th fair; 30th wet; 31ſt dry.

June. The 1ſt was wet, but it did not rain till after 8th; 9th was dry; 10th and 11th wet; 12th dry; 13th ſtormy, as were 13th, 14th, 15th, 16th, 17th, and 18th dry, but it rained in the night of the laſt; after that ſtormy to 24th, which was fair; it was wet after to the end, except 28th.

July. Was wet every day to 9th, except 2d and 6th; 10th was fair, and every day, except 11th, to 19th; then

wet to 25th; 26th dry; reſt wet.

Auguſt. Was fair till the 5th, and after to the 8th; the 9th, 10th, and 11th was dry; 12th wet; it was then dry to the 21ſt, except in the night of 15th; 21ſt and 22d were ſtormy; the reſt of the month was dry, but rather cloudy, except on 28th and 30th.

September had the 2 firſt days ſtormy; it was then dry and froſty after 13th to the 19th; 20th was fair; and rain fell after and in ſmall quantities only on 25th, 29th, and 30th.

October was alternately fair and wet to the 8th; after that it was dry to 14th; then wet to 19th; then dry to 23d; 24th was wet in the evening; 25th dry; after that wet to the end and ſtormy, except 27th.

November. The 1ſt was dry, but after that it was wet to the 7th, and, except 9th and 12th, again till the 14th, from which it was foggy, but dry, to 18th; that and 19th were wet; it was after that dry and froſty to the 27th; 28th was fair; 29th and 30th were wet.

December. Except that ſome heavy rains fell on the 4th, it was froſty and dry to the 18th, which was wet; 19th was fair; on 20th it ſnow'd, but was afterwards dry to 24th; 25th was dry; 26th wet; the reſt was dry; on 31ſt ſome ſnow fell.

XXV. *Journal of the Quantity of Rain that fell at* Holme, *near* Manchefter, *from* 1765 *to* 1769; *and at* Barowby, *near* Leeds, *from* 1772 *to* 1777. *By* George Lloyd.

Read February 26, 1778.

Quantity of rain at Holme, near Manchefter.

	1765	1766	1767	1768	1769	Average
	Inches	Inches	Inches	Inches	Inches	Inches
January	1.780	0.235	0.440	1.040	2.100	1.109
February	1.300	2.030	3.655	4.400	2.17	2.711
March	3.300	0.800	3.234	2.030	1.15	2.103
April	3.630	2.469	0.375	2.290	1.06	1.965
May	0.900	3.333	2.750	1.070	1.63	1.937
June	1.790	3.813	0.130	5.900	4.245	3.176
July	0.560	1.465	7.840	5.090	2.20	3.431
Auguft	4.200	2.075	2.660	2.947	4.55	3.286
September	2.193	2.760	2.447	5.624	5.92	3.789
October	7.315	3.467	0.720	1.740	1.274	2.903
November	2.790	1.860	3.735	4.925	3.29	3.320
December	1.800	1.455	1.200	3.470	2.925	2.170
Total	31.558	25.762	29.186	40.526	32.514	31.90

Quantity of rain at Barowby, near Leeds.

	1772	1773	1774	1775	1776	1777	Average
	Inches	Inches	Inches	Inches	Inches	Inches	Inches
January	2.24	2.4	2.2	2.0	1.0	1.2	1.84
February	2.79	1.7	2.0	3.5	2.2	0.7	2.15
March	3.49	0.3	1.1	1.6	1.3	1.9	1.62
April	1.38	1.9	1.4	0.9	0.9	2.2	1.45
May	1.20	4.5	1.75	0.3	0.7	1.4	1.64
June	3.20	1.3	2.3	1.0	2.98	3.3	2.34
July	1.44	0.8	3.63	6.1	3.17	3.64	3.0
Auguſt	1.63	1.825	2.0	4.2	5.0	2.76	2.9
September	4.60	4.875	3.5	2.9	3.47	1.34	3.44
Oćtober	2.30	1.875	0.8	4.3	1.35	4.75	2.56
November	3.75	2.6	1.4	2.8	3.87	1.45	2.64
December	0.80	5.0	2.1	1.0	1.1	1.5	1.92
Total	28.82	29.075	24.18	30.6	27.04	26.14	27.50

METEOROLOGICAL JOURNAL

KEPT AT THE HOUSE OF

THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL,

METEOROLOGICAL JOURNAL for January 1778.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	30,0	31,0	29,79		N	1	Cloudy.
	2	0	34,0	32,5	29 84		NNE	1	Fine and frosty.
2	8	0	28,0	31,5	29,98		N by E	1	Frosty.
	2	0	32,0	32,0	29,98		N	1	Fine and frosty.
3	8	0	33,0	31,0	29,99		NE	1	Frosty.
	2	0	34,0	32,5	29,99		N by E	1	Fine and frosty.
4	8	0	32,0	32,0	29,92		NE	1	Frosty.
	2	0	34,5	33,5	29,80		NNW	1	Fair and frosty.
5	8	0	27,0	32,0	29,81		N	1	Frosty.
	2	0	29,0	32,0	29,73		N by W	1	Frosty.
6	8	0	28,0	30,0	29,60		NW	1	Cloudy.
	2	0	32,0	31,0	29,58		N	1	Frosty.
7	8	0	24,0	29,0	29,58		SW	1	Frosty.
	2	0	27,0	29,0	29,57		NW	1	Frosty.
8	8	0	20,0	25,5	29,43		N	1	Frosty.
	2	0	28,0	26,5	29,43		N	1	Frosty.
9	8	0	19,0	24,0	29,69		N	1	Frosty.
	2	0	31,5	26,0	29,70		W by S	1	Frosty.
10	8	0	29,0	28,5	29,60		N by W	1	Foggy.
	2	0	35,0	30,0	29 54		E	1	Fair.
11	8	0	36,0	33,0	29,26	0,040	SW	1	Foggy.
	2	0	43,5	34,5	29,26		W by S	1	Foggy.
12	8	0	47,0	39,0	29,43		S by W	1	Fair.
	2	0	51,0	41,0	29,41		S by W	1	Fair.
13	8	0	47,0	45,0	29,60		SW	1	Fair.
	2	0	49,0	46,0	29,46		S by W	1	Fine.
14	8	0	44,0	45,5	29,62	0,095	SW	2	Fine.
	2	0	49,0	47,5	29,66		S by W	2	Fine.
15	8	0	48,0	48,5	29,70	0,200	S	2	Fine.
	2	0	57,0	49,0	29,74		SSW	1	Rainy.
16	8	0	45,0	49,0	29,95	0,050	E	1	Cloudy.
	2	0	48,0	50,0	29,98		SE	1	Fair.

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for January 1778.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	39,5	48,0	30,07		NNW	1	Foggy.
	2	0	45,0	48,0	30,06		E by N	1	Fair.
18	8	0	44,0	47,0	30,07		E	1	Cloudy.
	2	0	45,0	47,5	30,17		E by N	1	Fair.
19	8	0	36,0	43,0	29 99	0,200	E	1	Cloudy.
	2	0	39,0	43,0	29,96		SE	1	Rainy.
20	8	0	32,0	39,0	29,93	0,027	E	1	Cloudy.
	2	0	36,5	39,5	29,81		E by S	1	Fair.
21	8	0	33,0	36,0	29,70		NE	1	Fair.
	2	0	38,0	37,0	29,68		NE	1	Fair.
22	8	0	35,0	37,0	29,61	0,040	NE	1	Cloudy.
	2	0	34,0	40,0	29,54		NE	1	Fair.
23	8	0	35,0	37,0	29,74		NE	1	Cloudy.
	2	0	39,5	39,5	29,77		SW	1	Fair.
24	8	0	42,5	41,5	29,43	0,108	NW	1	Foggy.
	2	0	42,5	42,5	29,50		N	1	Cloudy.
25	8	0	35,0	35,5	30,15	0,069	NNE	1	Fair.
	2	0	38,0	40,0	30,21		NE	1	Fine.
26	8	0	34,0	37,5	30,25		SSE	1	Fair.
	2	0	36,0	38,0	30,17		S by W	1	Fine.
27	8	0	36,0	37,0	29,875		SSW	1	Fair.
	2	0	39,0	38,0	29,81		SSE	1	Rainy.
28	8	0	37,0	39,0	29,68	0,210	SW	1	Cloudy.
	2	0	44,0	40,0	29,49		N by W	1	Fine.
29	8	0	28,0	36,0	29,80		SW	1	Frofty.
	2	0	35,0	36,5	29,76		W by N	1	Fair.
30	8	0	27,5	35,5	29,78		NW	1	Fine.
	2	0	37,0	36,0	29,87		NW	1	Fine.
31	8	0	29,0	33,0	29,91		NW	1	Fine.
	2	0	38,0	36,0	30,14		W by N	1	Fine.

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	Time.		Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	40,0	36,0	29,93		S by W	1	Fair.
	2	0	44,0	38,0	29,77		SW	2	Cloudy.
2	8	0	38,0	37,0	29,52	0,085	SW	1	Fine.
	2	0	46,0	41,0	29,46		W by S	1	Fine.
3	8	0	35,5	39,0	29,56		NW	1	Fair.
	2	0	42,0	40,5	29,61		NW	2	Fine.
4	8	0	29,0	36,0	29,83		W by N	1	Fine.
	2	0	36,0	36,5	29,80		W by N	1	Cloudy.
5	8	0	31,0	34,0	29,88		NWbyN	1	Fair.
	2	0	35,5	35,0	29,88		W by N	1	Fine.
6	8	30	31,5	33,5	29,79		NW	1	Foggy.
	2	0	38,0	35,0	29,77		NW	1	Fair.
7	8	0	30,0	35,0	29,84		SW	1	Foggy.
	2	0	39,0	36,5	29,87		SW by S	1	Fine.
8	8	0	28,0	33,5	29,99		NE	1	Frosty.
	2	0	34,5	37,0	29,98		N	1	Fine and frosty.
9	8	0	31,5	32,5	29,93		NE	2	Fair.
	2	0	32,0	33,0	29,91		N by E	2	Cloudy.
10	8	0	32,0	33,0	29,95		E	1	Cloudy.
	2	0	34,0	34,0	29,95		NE	1	Rain.
11	8	0	34,0	34,5	29,96	0,088	NE by N	1	Cloudy.
	2	0	36,0	35,5	30,06		NE by N	1	Cloudy.
12	8	0	33,0	30,0	29,75	0,067	NW	1	Snow.
	2	0	33,5	35,5	29,67		NNE	2	Snow.
13	8	30	33,0	35,0	29,74	0,110	NE	1	Snow.
	2	0	35,5	35,5	29,74		NE	1	Snow.
14	8	0	31,0	34,5	29,91	0,081	NE by N	1	Fair.
	2	0	36,0	37,5	29,85		NE	1	Fine.
15	8	0	34,0	36,0	29,46		SW	1	Foggy.
	2	0	38,0	37,5	29,42		NW b N	1	Fine.
16	8	0	29,0	33,5	29,42		N by W	2	Fine.
	2	0	36,5	35,0	29,42		N by W	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	33,0	34,5	29,30		SE	1	Snow.
	2	0	39,0	36,0	29,26		SSW	1	Fine.
18	8	0	30,5	34,5	29,26	1,079	SE	1	Fine.
	2	0	35,0	36,0	29,26		N by W	1	Fine.
19	8	0	27,0	32,5	29,45		N	1	Snow.
	2	0	31,0	34,0	29,49		NE by N	1	Fine.
20	8	0	27,0	30,0	29,43		ENE	1	Frosty.
	2	0	33,5	33,5	29,31		NE	1	Frosty.
21	8	0	29,0	32,0	29,26		NE	2	Snow.
	2	0	34,0	33,0	29,21		ENE	2	Cloudy.
22	8	30	40,0	36,0	29,99	0,530	SE	1	Rain.
	2	0	49,0	39,0	29,08		WSW	1	Fair.
23	8	30	47,0	40,0	29,33	0,177	SW	1	Fair.
	2	0	53,0	43,0	29,43		SSW	2	Fine.
24	8	0	48,0	45,5	29,32	0,051	SW	2	Fair.
	2	0	53,5	48,0	29,43		SW	2	Fine.
25	8	0	44,5	47,0	29,87		S	1	Fair.
	2	0	51,0	49,0	29,93		SW by S	1	Fair.
26	8	0	46,5	48,0	29,94		S	1	Fine.
	2	0	56,5	53,5	29,94		SSE	1	Fine.
27	8	0	44,5	48,5	29,85		S	1	Fine.
	2	0	59,0	52,0	29,83		SSW	1	Fine.
28	8	30	43,0	48,0	30,04	0,020	NE	1	Fine.
	2	0	51,0	50,0	30,04		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Mar. 1	8	0	42,0	45,0	29,96		ENE	1	Fair.
	2	0	51,0	48,0	29,89		NE	1	Fine.
2	8	0	47,5	48,0	29,81		E	1	Foggy.
	2	0	61,0	51,0	29,75		SW	1	Fine.
3	8	0	50,0	51,5	29,78	0,230	WSW	1	Rain.
	2	0	58,0	54,0	29,85		SW	1	Fair.
4	8	0	48,0	52,0	29,94		S	1	Fair.
	2	0	56,0	54,5	29,94		SE	1	Fair.
5	8	0	45,5	51,5	29,76		ENE	1	Rain.
	2	0	50,0	52,0	29,72		NE by E	1	Fair.
6	7	30	40,5	48,0	29,81		NW	1	Fair.
	2	30	45,5	48,0	29,79		WNW	1	Fair.
7	8	30	33,0	39,0	29,83		NE	1	Fine.
	2	0	41,0	41,5	29,80		NE	1	Fine.
8	8	0	31,5	36,0	29,59		SW	1	Foggy.
	2	0	42,0	42,0	29,51		NE	1	Fair.
9	8	0	37,0	39,5	29,37		NW	1	Fair.
	2	0	39,0	40,0	29,39		NE	1	Cloudy.
10	7	30	31,5	37,5	29,59	0,063	NNW	1	Fair.
	2	0	39,0	38,5	29,66		NE	1	Fine.
11	7	30	31,0	35,5	29,76		NE	1	Fine.
	2	0	41,5	38,5	29,75		ENE	1	Fair.
12	7	30	31,0	35,5	29,72		NE	1	Fine.
	2	0	40,5	38,0	29,72		NE	1	Fair.
13	7	30	32,5	35,5	29,77		S by E	1	Fine.
	2	0	42,5	38,5	29,73		NW	1	Fine.
14	7	30	38,0	38,5	29,66	0,383	N by W	1	Fair.
	2	0	46,0	43,0	29,74		N by W	1	Fine.
15	7	30	45,0	42,0	29,71	0,205	SW	1	Rain.
	2	0	51,0	46,0	29,65		SW	1	Fair.
16	8	30	47,0	45,0	29,01	0,148	SSW	2	Rain.
	2	0	50,0	49,0	29,97		SW	2	Rain.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Mar. 17	7	30	42,0	47,0	29,33	0,106	W by S	2	Fair.
	2	0	49,0	47,0	29,62		NW	1	Fine.
18	7	0	49,5	48,0	29,48	0,065	SW	1	Fair.
	2	0	57,5	54,0	29,53		SW	2	Fine.
19	7	0	50,5	51,0	29,47	0,020	SW	2	Rain.
	2	0	56,0	54,5	29,29		SW	2	Fine.
20	7	0	45,5	48,5	29,39	0,118	SW	3	Rain.
	2	0	52,0	52,0	29,51		W by S	2	Fine.
21	7	0	41,0	48,0	29,74	0,025	WSW	1	Fine.
	2	0	53,5	50,5	29,76		W	1	Fine.
22	7	0	37,5	46,0	29,96		SW	1	Fine.
	2	0	52,0	49,0	29,98		NW	1	Fine.
23	7	0	40,0	45,0	30,15		NE	1	Cloudy.
	2	0	47,5	47,0	30,09		S by W	1	Cloudy.
24	7	0	46,0	46,5	30,04	0,025	SW	1	Fair.
	2	0	47,5	47,0	30,09		SE	1	Fine.
25	7	0	51,5	52,0	29,88		S by W	1	Fine.
	2	0	69,0	56,5	29,81		S by W	1	Fine.
26	7	0	53,0	57,0	29,88		S	1	Fine.
	2	0	72,0	64,0	29,94		SSE	1	Fine.
27	7	0	52,0	59,5	29,95		ENE	1	Fine.
	2	0	70,5	65,0	29,96		S	1	Fine.
28	7	0	43,0	54,0	30,03		NE	1	Fair.
	2	0	46,0	54,0	30,03		NE	1	Fair.
29	7	0	38,0	46,0	30,09		NE	1	Cloudy.
	2	0	45,0	47,0	30,09		NE	1	Fair.
30	7	0	36,0	42,0	30,11		N by E	1	Fair.
	2	0	45,5	44,5	30,04		NNE	2	Fair.
31	7	0	35,5	40,5	29,91		NE	2	Cloudy.
	2	0	44,5	43,5	29,95		SE	2	Fair.

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	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
April 1	7	0	35,0	38,5	29,96		NE	2	Cloudy.
	2	0	35,0	39,5	29,96		NE	2	Fine.
2	7	0	41,0	42,0	30,08		NE	2	Fine.
	2	0	47,0	45,0	30,09		NE	1	Fair.
3	7	0	43,0	44,0	30,10		N by W	1	Fine.
	2	0	52,0	47,5	30,12		NE	2	Fine.
4	7	0	40,5	43,5	30,20		NNE	1	Fair.
	2	0	50,5	46,5	30,23		NE	1	Fine.
5	7	0	34,5	41,0	30,35		NE	1	Fine.
	2	0	46,0	44,0	30,35		E by S	1	Fine.
6	7	0	33,0	38,5	30,29		N by E	1	Fine.
	2	0	48,5	43,0	29,24		NE	1	Fine.
7	7	0	37,0	40,0	30,13	0,010	NE	1	Fair.
	2	0	47,0	44,0	30,13		NE	1	Cloudy.
8	7	0	37,5	40,0	30,13		NE	1	Fine.
	2	0	46,5	43,5	30,13		NE	2	Fine.
9	7	0	40,0	41,0	30,05		ENE	2	Fine.
	2	0	49,0	44,0	30,07		ENE	2	Fine.
10	7	0	42,0	44,0	30,12		E by S	1	Fine.
	2	0	59,0	49,0	30,06		E by S	1	Fine.
11	7	0	49,5	48,5	29,98		E by S	1	Fine.
	2	0	62,0	53,0	29,99		SW	1	Fair.
12	7	0	51,5	53,0	29,83	0,130	S by W	1	Cloudy.
	2	0	60,0	56,5	29,79		SW	2	Fine.
13	7	0	48,0	51,5	29,85	0,038	SW	2	Fine.
	2	0	57,5	55,0	29,88		WSW	2	Fine.
14	7	0	42,0	49,0	30,06		NE	2	Fair.
	2	0	43,0	49,0	30,06		NE	2	Fair.
15	7	0	42,5	47,0	30,19	0,045	NNE	2	Fair.
	2	0	47,0	49,0	30,23		NE	1	Rain.
16	7	0	30,0	46,0	30,26	0,010	NE	1	Cloudy.
	2	0	49,0	48,0	30,20		NE	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	37,0	44,0	29,92		NE	1	Fair.
	2	0	51,0	46,0	29,73		ENE	1	Fine.
18	7	0	38,0	43,0	29,60		NE	2	Fair.
	2	0	45,0	45,0	29,60		NE	2	Cloudy.
19	7	0	35,0	40,0	29,79		NNE	1	Fine.
	2	0	49,5	43,5	29,84		E by N	2	Fine.
20	7	0	41,0	41,0	30,04		SE	1	Fine.
	2	0	52,0	45,0	29,98		SE	1	Fine.
21	7	0	48,5	46,0	29,51	0,203	S by W	2	Rain.
	2	0	56,0	50,0	29,50		SW	2	Rain.
22	7	0	57,0	53,5	29,45	0,010	SW	3	Rain.
	2	0	60,0	56,0	29,48		SW	3	Fair.
23	7	0	53,0	55,0	29,64	0,032	SW	2	Fair.
	2	0	61,0	56,5	29,55		SW	2	Fine.
24	7	0	45,0	53,0	29,62	0,075	W by N	3	Fair.
	2	0	63,0	58,5	29,76		W by N	3	Fine.
25	7	0	42,0	49,0	30,08	0,011	W	1	Fine.
	2	0	55,0	56,5	30,09		W	2	Fair.
26	7	0	41,5	47,5	30,23	0,023	W by S	1	Fair.
	2	0	54,5	50,5	30,18		W by N	1	Fine.
27	7	0	40,0	45,0	30,18		NE	1	Fine.
	2	0	51,0	48,5	30,16		NE	1	Fine.
28	7	0	43,0	46,5	30,13		E by S	1	Fair.
	2	0	56,0	50,0	29,98		NE	1	Fair.
29	7	0	46,0	47,0	29,84		E by S	1	Fine.
	2	0	55,5	53,5	29,80		E by S	2	Fair.
30	7	0	47,0	50,0	29,58	0,200	E by S	1	Rain.
	2	0	59,0	53,5	29,59		SE	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 1	7	0	50,5	53,5	29,55	0,137	SSW	1	Cloudy.
	2	0	64,0	56,5	29,47		S by E	1	Fine.
2	7	0	55,0	56,5	29,34		S by E	1	Fair.
	2	0	59,5	58,5	29,34		SSW	1	Fair.
3	7	0	54,0	57,0	29,33	0,075	WNW	2	Fine.
	2	0	61,5	59,0	29,39		SSW	2	Fair.
4	7	0	51,0	56,0	29,41	0,216	SE	1	Rain.
	2	0	62,0	58,5	29,38		SW	1	Fine.
5	7	0	53,5	57,0	29,67	0,030	S by W	1	Fair.
	2	0	61,0	58,5	29,76		SW	1	Fine.
6	7	0	52,0	56,5	29,81	0,030	SE	1	Fair.
	2	0	65,0	59,0	29,74		SE	1	Fine.
7	7	0	51,5	56,5	29,85	0,033	W by N	2	Fine.
	2	0	58,5	59,0	29,91		W by N	1	Rain.
8	7	0	51,0	55,5	30,15	0,011	SW	1	Fine.
	2	0	66,0	59,0	30,13		SW	1	Fine.
9	7	0	54,5	55,0	29,93		NE	1	Fair.
	2	0	62,0	59,5	29,92		NE	1	Fair.
10	7	0	47,5	54,5	29,88	1,490	NE	2	Fair.
	2	0	60,0	57,0	30,02		NNE	1	Fine.
11	7	0	53,0	55,0	30,08		N by W	1	Fine.
	2	0	60,0	58,0	30,04		NW	1	Fair.
12	7	0	47,0	54,0	30,00	0,010	NW	1	Fine.
	2	0	58,0	55,5	30,03		NW	1	Fine.
13	7	0	52,5	54,0	30,00	0,010	SSW	1	Fine.
	2	0	60,0	57,0	29,86		SSW	2	Fair.
14	7	0	49,5	55,0	29,55	0,040	WSW	1	Fair.
	2	0	59,0	57,5	29,48		SW	1	Fair.
15	7	0	47,5	55,0	29,35	0,271	SW	1	Rain.
	2	0	52,0	56,0	29,35		SW	1	Rain.
16	7	0	48,0	51,5	29,40	0,238	NE	1	Rain.
	2	0	53,5	55,0	29,44		NE by N	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
May 17	7	0	46,5	51,5	29,60	0,157	NE by E	1	Fair.
	2	0	54,5	54,0	29,67		NE by N	1	Fair.
18	7	0	44,0	52,0	29,77		NW	1	Fair.
	2	0	60,5	54,0	29,77		WSW	1	Fine.
19	7	0	51,5	53,0	29,77		SE	1	Fine.
	2	0	58,0	56,0	29,73		NE	1	Fair.
20	7	0	47,0	54,0	29,60	0,728	NE	1	Rain.
	2	0	53,0	55,0	29,69		NE	1	Fair.
21	7	0	46,5	52,0	29,77	0,410	NNW	1	Rain.
	2	0	59,0	56,0	29,86		NNW	1	Fair.
22	7	0	50,5	54,5	29,82		S by W	1	Fair.
	2	0	63,0	58,0	29,80		WSW	2	Fine.
23	7	30	51,5	55,5	29,78	0,180	SE	1	Fine.
	2	0	63,0	58,5	29,83		SW	1	Fine.
24	7	0	53,0	57,5	29,63		S	1	Rain.
	2	0	57,5	58,0	29,55		NW	1	Fair.
25	7	0	51,5	57,0	29,57	0,010	SW b W	1	Fine.
	2	0	63,5	62,0	29,57		SW	1	Fine.
26	7	0	52,0	57,0	29,77	0,038	NNE	1	Fine.
	2	0	53,0	58,0	29,85		NE	2	Rain.
27	7	0	53,0	58,0	29,91	0,488	SW	1	Fine.
	2	0	66,5	60,5	29,95		SW	1	Fine.
28	7	0	58,0	60,5	30,06		N by E	1	Fine.
	2	0	67,5	63,5	30,12		NW	1	Fine.
29	7	0	51,5	53,0	30,20		NE	1	Fair.
	2	0	61,5	60,5	30,20		SE	1	Fine.
30	7	0	57,0	60,0	30,12		E	1	Fine.
	2	0	64,0	59,0	30,06		SE	1	Fine.
31	7	0	59,0	60,5	30,13		SE	1	Fine.
	2	0	70,5	64,0	30,09		SE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
June 1	7	0	60,5	64,0	30,02		S	1	Fine.
	2	0	71,0	67,0	30,00		WSW	1	Fine.
2	7	0	62,0	65,5	30,08		W	1	Fair.
	2	0	72,0	68,0	30,13		WSW	1	Fair.
3	7	0	63,0	68,0	30,22		S by W	1	Fair.
	2	0	76,0	70,0	30,22		SE	1	Fine.
4	7	0	62,0	68,5	30,18		S by W	1	Fine.
	2	0	75,0	78,0	30,09		SW	1	Fine.
5	7	0	57,5	65,0	30,15		N by W	1	Fine.
	2	0	66,0	64,0	30,20		NW	1	Fine.
6	7	0	51,0	57,5	30,33		NNE	1	Fine.
	2	0	66,0	62,5	30,38		NE	1	Fine.
7	7	0	54,5	58,5	30,31		ENE	1	Fine.
	2	0	67,0	62,0	30,19		NW	1	Fine.
8	7	0	53,0	58,5	30,01		NW	1	Fine.
	2	0	60,0	61,5	29,98		NE	1	Fine.
9	7	0	50,0	56 0	29,97		N by W	2	Fine.
	2	0	55,5	57,0	29,95		NNE	2	Fair.
10	7	0	47,5	52,5	29 92		NNW	1	Fair.
	2	0	54,0	54,5	29,88		NW	1	Cloudy.
11	7	0	49,5	52,5	29,77	0,131	E by S	1	Fair.
	2	0	52,0	53,5	29,75		SE	1	Rain.
12	7	0	47,5	52,5	29,76	0,110	NNW	1	Fair.
	2	0	60,0	56,0	29,76		NW	1	Fine.
13	7	0	56,0	56,5	29,76		SSW	1	Fine.
	2	0	64,0	58,5	29,72		SW	2	Fair.
14	7	0	55,0	58,0	29,65	0,174	WSW	2	Fine.
	2	0	64,0	52,0	29 76		SW	1	Fine.
15	7	0	57,0	59,5	29,81	0,169	S by W	1	Cloudy.
	2	0	61,0	61,5	29,83		S by W	2	Raid.
16	7	0	58,5	61,5	29,86		S by W	1	Cloudy.
	2	0	63,0	63,0	29,84	0,049	S by W	2	Rain.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
June 17	7	0	58,0	61,0	29,73	0,215	SW	1	Cloudy.
	2	0	63,5	64,0	29,81		SW	1	Fine.
18	7	0	54,5	60,0	30,10		WSW	1	Fair.
	2	0	66,5	62,5	30,17		WSW	1	Fine.
19	7	0	57,0	61,5	30,14		SSW	1	Cloudy.
	2	0	61,0	63,0	30,05		SW	2	Cloudy.
20	7	0	57,5	62,0	29,74	0,377	SW	1	Cloudy.
	2	0	63,0	65,0	29,66		SW	1	Fine.
21	7	0	57,5	62,0	29,72	1,109	W	1	Fine.
	2	0	67,0	65,0	29,77		W	1	Cloudy.
22	7	0	60,0	64,0	29,74	0,176	W by S	1	Fine.
	2	0	62,5	64,5	29,74		SW	1	Rain.
23	7	0	52,5	60,5	29,83	0,344	N by E	1	Cloudy.
	2	0	59,0	62,5	29,90		E by S	1	Cloudy.
24	7	0	53,5	60,0	29,97	0,405	NE	1	Fine.
	2	0	60,0	61,0	30,11		NE	1	Fine.
25	7	0	53,5	59,0	30,35	0,260	NNW	1	Fine.
	2	0	65,0	61,0	30,42		NW	1	Cloudy.
26	7	0	61,5	62,0	30,14		SW	1	Fine.
	2	0	70,0	64,0	29,99		SW	1	Fine.
27	7	0	56,5	62,5	29,76	0,010	SW b W	1	Fine.
	2	0	62,0	62,5	29,77		NW	1	Cloudy.
28	7	0	55,0	60,5	29,99	0,133	NW	1	Fair.
	2	0	66,5	62,0	29,85		SW	1	Fine.
29	7	0	57,0	61,0	29,89		W	1	Fair.
	2	0	68,0	64,0	29,83		SW	2	Fair.
30	7	0	57,0	61,5	29,70	0,092	W by S	1	Fair.
	2	0	68,0	64,0	29,78		SW	1	Fine.

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	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
July 1	7 0	58,0	61,5	29,70	0,372	SSE	1	Cloudy.
	2 0	61,0	62,5	29.57		E by S	1	Cloudy.
2	7 0	58,5	61,5	29,68	0,702	WSW	1	Fair.
	2 0	68,0	63,5	29,72		SW	1	Fair.
3	7 0	61,0	63,0	29,72	0,120	SW	1	Fine.
	2 0	63,0	64,5	29,66		SE	1	Rain.
4	7 0	57,0	63,0	29,47	0,480	S by E	2	Fine.
	2 0	64,0	64,0	29,58		SW	1	Fine.
5	7 0	55,0	60,5	29,53	0,154	S by W	2	Rain.
	2 0	64,5	63,5	29,61		S by W	2	Fair.
6	7 0	58,5	62,0	30,00	0,031	SW	1	Fair.
	2 0	63,0	63,5	30,07		WSW	1	Fair.
7	7 0	60,0	62,0	30,05		SW	1	Fine.
	2 0	57,0	62,0	29,88		SW	1	Rain.
8	7 0	55,0	59,5	29,82	0,291	W	1	Cloudy.
	2 0	61,0	62,0	29,85		N	1	Fine.
9	7 0	55,0	58,5	30,14	0,023	NNE	1	Cloudy.
	2 0	58,0	59,0	30,23		NW	1	Cloudy.
10	7 0	54,5	58,0	30,33		N	2	Fair.
	2 0	59,5	60,5	30.39		N by W	1	Fair.
11	7 0	64,0	61,5	30,39		SW	1	Fair.
	2 0	75,0	65,0	30,37		NW	1	Fine.
12	7 0	65,5	66,0	30,34		SSW	1	Fair.
	2 0	74,0	68,0	30,34		SW	1	Fine.
13	7 0	66,0	68,5	30,37		NNW	1	Fine.
	2 0	78,0	70,0	30,39		NW	1	Fine.
14	7 0	64,5	68,5	30,40		W by N	1	Fine.
	2 0	80,0	73,0	30,45		SW	1	Fine.
15	7 0	61,5	70,0	30,34		W by N	1	Fine.
	2 0	76,0	72,0	30,34		NW	1	Fine.
16	7 0	67,5	72,0	30,29		S	1	Fine.
	2 0	81,0	74,0	34,22		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	0	66,0	72,5	30,04		S	0	Fine.
	2	0	84,0	74,0	29,97		SW	1	Fine.
18	7	0	64,5	73,5	29,92		N	1	Fine.
	2	0	79,0	75,5	29,90		NW	1	Fine.
19	7	0	65,0	72,5	29,81		WNW	1	Fine.
	2	0	73,0	73,5	29,81		NW	1	Fair.
20	7	0	58,5	69,0	29,60	1,120	N	1	Rain.
	2	0	65,5	70,5	29,73		NW	1	Fair.
21	7	0	57,0	63,5	29,56	1,194	NE	1	Rain.
	2	0	60,0	67,5	29,59		NE	1	Fair.
22	7	0	56,0	63,5	29,87	0,235	NE by N	2	Fine.
	2	0	67,0	66,0	29,88		SE	1	Fine.
23	7	0	63,0	64,5	29,73	0,045	SSE	1	Cloudy.
	2	0	73,0	68,5	29,67		SW	1	Cloudy.
24	7	0	58,0	67,5	29,58		SW	1	Fair.
	2	0	68,0	67,5	29,55		SW	1	Fair.
25	7	0	53,5	62,5	29,71	0,173	NNE	1	Cloudy.
	2	0	62,0	64,5	29,79		NNE	1	Cloudy.
26	7	0	54,0	60,5	29,94		N by W	2	Fine.
	2	0	60,0	62,0	29,95		NW	1	Cloudy.
27	7	0	56,5	60,5	29,91		W by N	1	Fair.
	2	0	65,5	63,5	29,89		SW	1	Fair.
28	7	0	57,0	61,0	29,89	0,010	SW b. W	1	Rain.
	2	0	68,0	63,5	29,85		SW	1	Fine.
29	7	0	58,5	62,5	29,68	0,266	SE by S	1	Fair.
	2	0	60,0	62,5	29,54		SE	1	Cloudy.
30	7	0	57,5	62,5	29,52	0,181	SW	1	Cloudy.
	2	0	67,0	64,5	29,51		SE	1	Cloudy.
31	7	0	58,0	62,0	29,31	0,300	SE	3	Rain.
	2	0	62,0	64,0	29,46		SE	2	Rain.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	57,0	62,0	29,61	0,366	SW by S	1	Fair.
	2	0	69,0	65,0	29,65		SW	1	Fine.
2	7	0	58,5	63,0	29,86	0,010	W by N	1	Fair.
	2	0	62,0	63,5	29,98		NW	1	Cloudy.
3	7	0	55,5	61,5	30,17		NNW	1	Fine.
	2	0	67,0	63,5	30,23		NW	1	Fine.
4	7	0	57,0	62,5	30,24		S by W	1	Fine.
	2	0	71,5	65,0	30,18		SW	1	Fine.
5	7	0	58,5	63,5	30,04		SW	1	Fair.
	2	0	65,5	65,0	29,97		SW	1	Fair.
6	7	0	56,5	62,0	29,95		S by W	1	Fair.
	2	0	70,0	65,0	30,01		SW	1	Fair.
7	7	0	58,0	63,5	30,09		SW b S	1	Fine.
	2	0	72,5	67,5	30,05		SW	1	Fine.
8	7	0	63,5	65,5	29,84		SSW	1	Fair.
	2	0	82,0	70,0	29,84		SSW	1	Fair.
9	7	0	67,0	69,0	29,63		SW	1	Fine.
	2	0	69,0	71,0	29,78		SW	1	Fair.
10	7	0	59,5	64,5	29,98		SW	1	Fine.
	2	0	70,0	68,0	30,04		SW	1	Fine.
11	7	0	62,0	66,0	29,97		SSE	1	Fine.
	2	0	75,5	68,5	29,94		SW	1	Fine.
12	7	0	65,5	68,5	30,01		SW	1	Fair.
	2	0	77,0	72,0	30,01		SW b W	1	Fair.
13	7	0	62,0	69,5	30,22	0,020	NE	1	Cloudy.
	2	0	73,0	71,5	30,29		NE	1	Fine.
14	7	0	62,5	70,0	30,36		WSW	1	Fine.
	2	0	78,0	72,0	30,35		SW	1	Fine.
15	7	0	60,5	70,0	30,26		W by S	1	Fine.
	2	0	76,5	72,5	30,22		WNW	1	Fine.
16	7	0	61,0	70,0	30,15		SSW	1	Cloudy.
	2	0	78,0	74,0	30,15		W by N	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Aug. 17	7	0	62,0	71,5	30,14		NW	1	Fair.
	2	0	74,0	72,5	33,10		NW	1	Fine.
18	7	0	62,0	69,5	30,07		NE	1	Fair.
	2	0	70,0	70,5	30,16		NE	1	Fine.
19	7	0	59,0	66,5	30,09		NE	1	Fair.
	2	0	75,0	70,0	30,05		SE	1	Fine.
20	7	0	59,0	67,0	30,05		ENE	1	Fine.
	2	0	80,0	70,0	30,05		NE	1	Fine.
21	7	0	64,5	68,5	29,83		SW by S	1	Fair.
	2	0	68,0	69,0	29,78		SW	1	Cloudy.
22	7	0	66,0	66,0	29,86	0,062	SW	2	Fine.
	2	0	69,0	68,0	29,93		W by S	2	Cloudy.
23	7	0	61,0	65,5	30,09		WSW	1	Fair.
	2	0	69,0	67,0	30,12		SW	1	Fair.
24	7	0	58,5	65,0	30,05		SW	0	Fair.
	2	0	74,0	68,0	29,98		SW	2	Fine.
25	7	0	63,0	67,0	29,95		SW	1	Cloudy.
	2	0	74,0	70,0	30,03		SW	1	Cloudy.
26	7	0	64 0	69,0	30,20		SW	1	Fair.
	2	0	76,0	71,0	30,31		SW	1	Fair.
27	7	0	56,5	66,5	30,45		NE	1	Fine.
	2	0	73,0	70,0	30,43		SE	1	Fine.
28	7	0	59,5	66,5	30,26		E by S	1	Fine.
	2	0	71,0	70,0	30,14		SE	1	Fine.
29	7	0	64,0	68,5	29,95	0,032	W by S	1	Cloudy.
	2	0	70,0	69,0	30,02		NW	1	Fine.
30	7	0	55,0	65,0	29,93		NE	1	Cloudy.
	2	0	59,5	66,0	29,73		SSE	1	Rain.
31	7	0	56,0	54,5	29,50	0,585	WNW	2	Fair.
	2	0	60,5	64,5	29,78		NW	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	48,0	60,5	30,15		W by N	1	Fine.
	2	0	64,5	62,5	30,12		WNW	2	Fair.
2	7	0	56,5	60,5	29,87		WSW	1	Cloudy.
	2	0	66,0	63,5	29,83		W by S	2	Fair.
3	7	0	51,5	59,5	29,76	0,092	NW	1	Fine.
	2	0	62,0	60,5	29,94		NNW	1	Fine.
4	7	0	52,5	59,5	30,03	0,010	N by W	1	Cloudy.
	2	0	63,5	61,5	30,01		NW	1	Cloudy.
5	7	0	59,5	61,5	30,16	0,103	NW	1	Cloudy.
	2	0	66,0	63,5	30,22		NW	1	Cloudy.
6	7	0	57,0	62,0	30,28		NW	1	Fair.
	2	0	73,0	66,0	30,28		NW	1	Cloudy.
7	7	0	61,0	65,0	30,25		SSW	1	Fine.
	2	0	75,0	68,0	30,24		NE	1	Fine.
8	7	0	58,5	65,5	30,20		SSE	1	Fair.
	2	0	75,0	69,0	30,18		S by W	1	Fair.
9	7	0	56,5	65,0	30,13		S by W	1	Fine.
	2	0	76,0	70,0	30,16		SW	1	Fine.
10	7	0	56,5	66,0	30,25		NW	1	Fine.
	2	0	67,0	67,0	30,28		N by W	1	Fine.
11	7	0	53,0	64,0	30,33		SW	1	Fine.
	2	0	70,0	66,0	30,29		NW	1	Fine.
12	7	0	55,5	64,0	30,29		N by E	1	Fair.
	2	0	70,0	65,5	30,32		NE	1	Fine.
13	7	0	57,5	63,0	30,27		NE	1	Fair.
	2	0	65,0	65,0	30,27		NE	1	Fine.
14	7	0	54,5	61,0	30,25		ENE	1	Fine.
	2	0	64,0	62,5	30,25		ENE	2	Fine.
15	7	0	57,5	55,5	30,33		NE by N	1	Fine.
	2	0	63,0	60,0	30,35		NE	2	Fine.
16	7	0	43,0	53,0	30,29		NE	1	Cloudy.
	2	0	66,0	59,5	30,22		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	47,0	54,5	30.19		S by W	1	Fine.
	2	0	68,0	60,0	30,19		SW	1	Fine.
18	7	0	56,5	59,0	30,15		SE	1	Cloudy.
	2	0	72,0	67,5	30,12		SSE	1	Fine.
19	7	0	51,5	60,0	30,08		SE	1	Fair.
	2	0	70,0	62,5	30,01		SE	1	Fine.
20	7	0	57,0	61,5	30,05		NE	1	Fair.
	2	0	61,5	61,5	30,15		NE	1	Fine.
21	7	0	47,0	56,5	29,96		NE	1	Fine.
	2	0	59,0	60,5	29,84		NE	1	Fine.
22	7	0	51,0	58,5	29,69		W by S	1	Fine.
	2	0	60,0	60,5	29,79		NW	1	Fine.
23	7	0	42,5	55,5	29.99		SSW	1	Fine.
	2	0	60,0	58,0	30,03		NW	1	Fine.
24	7	0	48,0	54,5	29,97		ESE	1	Foggy.
	2	0	63,0	57,0	29,92		SW	1	Fine.
25	7	0	49,5	55,5	29,82		E by N	1	Fair.
	2	0	67,5	58,5	29.82		SW	1	Fine.
26	7	0	56,0	58,0	29,74		SE	1	Fine.
	2	0	75,0	66,0	29,71		SE	1	Fine.
27	7	0	60,5	64,0	29,90	0,220	S by E	1	Fair.
	2	0	72,0	66,5	29,91		S by E	1	Fine.
28	7	0	62,5	65,5	29,99		E	1	Cloudy.
	2	0	76,5	70,0	30,00		ENE	1	Fine.
29	7	0	60,0	66,5	29,94		NE	1	Fine.
	2	0	69,5	68,5	30,02		SW	1	Cloudy.
30	7	0	57,0	65,5	30,10	0,073	SE	1	Fine.
	2	0	67,0	68,0	30,06		SE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Oct. 1	7	0	60,5	65,5	29,74	0,053	SE	1	Rain.
	2	0	62,0	66,0	29,58		SE	1	Rain.
2	7	0	52,0	63,0	29,54	0,462	S by E	1	Fair.
	2	0	62,5	63,5	29,56		NW	1	Fine.
3	7	0	48,0	59,5	29,65	0,016	W	1	Fair.
	2	0	61,5	61,5	29,75		SW	1	Fine.
4	7	0	56,0	59,0	29,59	0,017	SW	2	Rain.
	2	0	64,5	61,0	29,60		SW	2	Fair.
5	7	0	55,0	59,5	29,84	0,014	SW	2	Fine.
	2	0	65,5	61,5	29,92		SW	2	Fine.
6	7	0	55,0	59,5	30,03		E by S	1	Cloudy.
	2	0	67,5	62,5	29,99		ESE	1	Fair.
7	7	0	60,0	61,0	29,79	0,014	S by W	2	Rain.
	2	0	64,0	63,5	29,79		SW	2	Rain.
8	7	0	50,5	59,5	30,03	0,025	W by S	1	Fair.
	2	0	60,5	61,0	30,09		WSW	1	Fine.
9	7	0	49,0	57,0	30,16		SSW	1	Fair.
	2	0	63,5	60,0	30,14		SE	1	Fine.
10	7	0	47,5	55,0	30,08		SE	1	Cloudy.
	2	0	63,0	58,0	30,03		SE	1	Fine.
11	7	0	53,0	57,0	29,97		NE	1	Foggy.
	2	0	64,0	59,5	29,93		S by W	1	Fair.
12	7	0	50,5	57,5	29,93		NW	1	Cloudy.
	2	0	58,0	58,5	29,92		NW	1	Cloudy.
13	7	0	49,5	56,0	29,88		N by W	1	Fair.
	2	0	58,0	57,0	29,84		NNE	1	Fair.
14	7	0	51,5	56,0	29,75	0,295	NE	2	Cloudy.
	2	0	56,5	56,5	29,71		NE	2	Cloudy.
15	7	0	54,0	56,5	29,54	0,874	NE	2	Rain.
	2	0	69,0	58,0	29,60		SE	1	Cloudy.
16	7	0	54,0	37,0	29,65	0,107	SE	1	Fair.
	2	0	61,0	59,5	29,70		S by W	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Oct. 17	7	0	53,0	57,5	29,71	0,087	SW	1	Foggy.
	2	0	60,0	59,0	29,74		SW	1	Fair.
18	7	0	51,0	57,5	29,93	0,992	N by E	1	Fair.
	2	0	58,0	58,5	30,02		NE	1	Fine.
19	7	0	44,0	63,5	30,14	0,018	NW	1	Fine.
	2	0	52,0	54,0	30,25		NE	1	Fair.
20	7	0	44,5	51,0	30,20		ENE	1	Fine.
	2	0	56,5	54,0	30,18		NE	2	Fine.
21	7	0	39,5	47,0	30,22		E by S	2	Fair.
	2	0	47,0	47,0	30,16		ENE	2	Fine.
22	7	0	35,5	43,5	30,11		ENE	1	Fair.
	2	0	45,0	44,5	30,04		SE	1	Fine.
23	7	0	40,0	42,5	29,81		E by N	1	Fair.
	2	0	52,0	45,0	29,75		SE	1	Fine.
24	7	0	44,5	46,5	29,78	0,034	SSW	1	Rain.
	2	0	52,0	48,0	29,81		SW	1	Fine.
25	7	0	47,0	46,0	29,85		SW	1	Fair.
	2	0	56,5	53,0	29,82		SW	2	Fair.
26	7	0	53,0	51,0	29,62		SW	1	Fair.
	2	0	59,0	53,0	29,48		SW	3	Cloudy.
27	7	0	43,0	51,5	29,58	0,010	SW	1	Cloudy.
	2	0	51,0	52,0	29,63		SW	1	Fair.
28	7	0	34,5	46,5	29,78		SSW	1	Fair.
	2	0	51,0	48,0	29,69		SW	1	Fine.
29	7	0	51,0	50,0	29,28	0,223	S by W	2	Cloudy.
	2	0	51,0	51,0	29,16		NE	2	Rainy.
30	7	0	53,5	51,0	29,78	0,497	S by E	3	Rain.
	2	0	53,5	53,0	29,71		SSW	3	Rain.
31	7	0	45,0	52,0	29,29	0,740	SSW	1	Fine.
	2	0	51,0	52,5	29,46		SW	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Nov. 1	8	0	37,0	47,0	29,70		NW	1	Foggy.
	2	0	45,0	46,0	29,72		NW	1	Fine.
2	8	0	45,0	46,0	29,71		S by E	1	Fine.
	2	0	53,0	48,0	29,70		SW	1	Fine.
3	8	0	54,0	51,0	29,72		S by E	1	Cloudy.
	2	0	60,0	53,0	29,77		SE	1	Cloudy.
4	8	0	58,0	56,5	29,81		SW	1	Cloudy.
	2	0	60,0	58,0	29,86		NW	1	Rain.
5	8	0	45,0	53,0	30,23		SSW	1	Fine.
	2	0	55,0	54,5	30,23		WSW	1	Fine.
6	8	0	41,0	50,0	30,17		SW	1	Fine.
	2	0	52,5	51,5	30,17		W by N	1	Fair.
7	8	0	36,5	47,5	30,27		WSW	1	Foggy.
	2	0	47,0	48,5	30,22		SW	1	Fine.
8	8	0	46,5	46,5	29,74	0,053	SW b W	1	Rain.
	2	0	50,5	47,5	29,51		SW b W	1	Fair.
9	8	0	34,5	44,0	29,54	0,023	NW	1	Fine.
	2	0	45,0	44,5	29,64		NW	1	Fine.
10	8	0	36,0	41,0	29,80		NWbW	1	Fair.
	2	0	49,0	43,5	29,74		SW	1	Cloudy.
11	8	0	49,0	46,0	29,66	0,049	SW	1	Fair.
	2	0	55,0	48,0	29,65		SW	1	Fine.
12	8	0	49,0	49,0	29,78	0,206	SE	1	Rain.
	2	0	55,0	52,5	29,75		SE	1	Fine.
13	8	0	44,0	47,5	29,62		SE	1	Fair.
	2	0	55,0	51,0	29,57		SE	1	Fine.
14	8	0	37,0	46,5	29,87	0,217	SW	1	Fair.
	2	0	54,0	48,0	29,97		SW	1	Fine.
15	8	0	35,0	43,0	30,13		NNW	1	Fair.
	2	0	46,0	44,5	30,15		NW	1	Fine.
16	8	0	31,0	40,5	30,21		N by W	1	Fair.
	2	0	40,0	41,0	30,24		NW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 17	8	0	38,0	40,0	30,35		W by N	1	Foggy.
	2	0	50,0	42,0	30,38		NW	1	Fine.
18	8	0	41,5	42,0	30,30		SW	1	Fair.
	2	0	52,0	44,5	30,30		SW	1	Fair.
19	8	0	48,0	44,0	30,25		WSW	1	Foggy.
	2	0	52,5	46,5	30,15		SW	1	Fine.
20	8	0	41,5	46,5	30,20		W by N	1	Fair.
	2	0	48,5	47,5	30,20		NW	1	Fine.
21	8	0	49,0	49,0	30,20	0,015	SW by S	1	Cloudy.
	2	0	54,5	50,5	30,17		SW	1	Cloudy.
22	8	0	48,0	50,0	30,22		W by S	1	Fair.
	2	0	52,0	51,5	30,15		W by S	1	Fair.
23	8	0	45,0	50,0	30,01	0,063	N by W	1	Fair.
	2	0	48,0	49,5	30,03		NW	1	Fine.
24	8	0	36,0	44,5	30,11		NW	1	Fair.
	2	0	43,0	44,0	30,17		NW	1	Fine.
25	8	0	37,0	41,5	30,26		W by N	1	Fair.
	2	0	45,0	42,0	30,27		W by N	1	Fair.
26	8	0	42,0	43,0	30,21		SW	1	Fair.
	2	0	48,5	45,5	30,15		SW	1	Fair.
27	8	0	46,5	46,0	29,91		NW	1	Fair.
	2	0	50,0	47,5	29,82		SW	1	Fine.
28	8	0	39,0	46,0	29,69	0,133	NWbW	2	Fair.
	2	0	41,0	46,0	29,91		NNW	2	Fine.
29	8	0	32,0	40,5	30,05		SSW	1	Foggy.
	2	0	42,0	42,0	29,94		SW	1	Fair.
30	8	0	49,5	43,5	29,26	0,115	SSW	2	Fine.
	2	0	51,5	45,5	29,12		SW	3	Rain.

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	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Dec. 1	8	0	37,5	40,0	29,50	0,203	SW	1	Fair.
	2	0	46,0	46,5	29,77		SW	1	Fine.
2	8	0	39,5	43,5	29,81		SW b W	1	Fair.
	2	0	43,5	44,5	29,83		SW	1	Fine.
3	8	0	34,0	40,5	29,87		W by S	1	Foggy.
	2	0	44,0	41,5	29,64		SE	1	Fair.
4	8	0	48,0	45,0	28,98	0,228	WSW	2	Cloudy.
	2	0	42,0	44,5	29,01		SW	1	Cloudy.
5	8	0	34,0	39,5	29,48	0,143	NW	1	Fair.
	2	0	37,0	39,5	29,48		W by N	1	Fair.
6	8	0	30,5	36,0	29,60		WNW	1	Fair and frosty.
	2	0	36,0	38,0	29,48		NW	1	Fine.
7	8	0	34,0	35,5	29,64		NW	1	Frosty.
	2	0	38,0	38,0	29,69		NW	2	Rain.
8	8	0	36,5	36,5	29,86	0,027	N by W	1	Cloudy.
	2	0	41,0	38,5	29,90		NW	1	Cloudy.
9	8	0	35,5	37 5	30,02		N by W	1	Fair.
	2	0	40,0	42,0	30,17		NW	1	Cloudy.
10	8	0	35,5	38,5	30 31		NNE	1	Foggy.
	2	0	36,0	40,5	30,44		SW	1	Foggy.
11	8	0	28,0	36,0	30,55		S by W	1	Foggy.
	2	0	43,0	40,0	30,54		SW	1	Fair.
12	8	0	48,0	43,5	30,48		SW	1	Cloudy.
	2	0	50,0	45,0	30,40		SW	1	Fair.
13	8	0	44,0	46,0	30,39		SSW	1	Fair.
	2	0	48,0	48,0	30,45		SW	1	Fine.
14	8	0	38,0	44,0	30,20		E by N	1	Cloudy.
	2	0	39,5	48,0	30,11		SSE	1	Foggy.
15	8	0	34,0	40,0	30,10		NNE	1	Cloudy.
	2	0	34,0	40,0	30,12		SW	1	Cloudy.
16	8	0	43,0	41,5	30,07		NNE	1	Cloudy.
	2	0	43,5	42,0	30,07		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	34,0	38,0	30,14		NNW	1	Fair.
	2	0	39,5	39,5	30,10		NNW	1	Fair.
18	8	0	36,0	38,0	30,01		N by E	1	Fair.
	2	0	38,0	39,0	29,98		N by W	1	Fine.
19	8	0	35,5	38,0	30,04		NNE	1	Foggy.
	2	0	39,0	39,0	30,00		NE	1	Fair.
20	8	0	34,0	37,0	29,95		NE	1	Foggy.
	2	0	34,0	37,5	29,90		ESE	1	Foggy.
21	8	0	33,0	36,5	29,79	0,037	N by E	1	Cloudy.
	2	0	35,0	36,5	29,73		N by W	1	Cloudy.
22	8	0	29,0	34,0	29,69		W by N	1	Fair.
	2	0	35,0	35,5	29,61		S by W	1	Fair.
23	8	0	32,5	35,0	29,58	0,030	E by S	1	Foggy.
	2	0	36,5	35,5	29,53		SE	1	Rainy.
24	8	0	38,5	37,0	29,08	0,057	SE	1	Fair.
	2	0	42,0	38,5	29,98		SE	1	Rainy.
25	8	0	39,0	39,0	29,25	0,058	SW	1	Cloudy.
	2	0	40,0	40,0	29,31		SW	1	Cloudy.
26	8	0	37,5	40,0	29,21		SSW	1	Cloudy.
	2	0	39,0	45,0	29,15		SW	1	Cloudy.
27	8	0	33,0	38,5	29,28	0,018	N by W	1	Fair.
	2	0	38,0	39,5	29,39		NNE	1	Fair.
28	8	0	35,0	38,5	29,68		N by W	1	Fair.
	2	0	38,0	38,5	29,68		NW	1	Fine.
29	8	0	34,0	37,5	29,59		NNE	1	Fair.
	2	0	37,0	38,0	29,54		NW	1	Fair.
30	8	0	34,5	36,0	29,45		NNE	1	Fair.
	2	0	36,0	36,0	29,51		NE	2	Fine.
31	8	0	33,0	34,5	29,59		NE	2	Cloudy.
	2	0	35,0	34,50	29,60		NE	2	Fair.

777.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Inches.
nuary	57,0	19,0	36,2	49,0	24,0	37,3	30,25	29,26	29,766	1,029
bruary	59,0	27,0	38,4	53,5	30,0	39,6	30,06	29,08	29,68	1,288
arch	72,0	31,0	46,8	65,0	35,5	47,0	30,15	29,29	29,98	1,388
pril	63,0	30,0	47,1	58,5	38,0	47,3	30,35	29,45	29,93	0,787
ay	70,5	44,0	56,0	64,0	51,5	54,2	30,20	29,33	29,75	4,602
ne	76,0	47,5	60,0	78,0	52,0	61,4	30,42	29,65	30,10	3,754
ly	84,0	54,0	63,6	75,5	58,0	65,1	30,45	29,31	29,89	5,697
ugust	80,0	55,0	65,9	74,0	54,5	67,3	30,45	29,50	30,19	1,075
ptember	76,5	42,5	60,8	70,0	53,0	62,3	30,35	29,69	30,09	0,498
ctober	69,0	34,5	53,6	66,0	42,5	55,2	30,25	29,16	30,13	3,578
ovember	60,0	31,0	46,0	58,0	40,0	46,8	30,38	29,12	29,96	0,874
ecember	50,0	28,0	37,7	48,0	34,0	39,5	30,55	28,98	29,86	0,801
hole Year			51,0			53,0			29,943	25,371

VARIATION-NEEDLE.

	7 h. A. M.	12 h. M.	2 h. P. M.	10 or 11 h. P. M.	Daily Mean.
	° ′	° ′	° ′	° ′	° ′
July 11	22 50	22 15	22 11	22 7	22 23
12	22 58	22 9	22 14	22 6	22 24
13	22 11	22 13	22 14	22 10	22 12
14	22 13	22 18	22 18	22 12	22 15
15	22 14	22 16	22 18	22 9	22 12
16	22 5	22 15	22 6	22 0	22 6
17	22 15	22 15	22 16	22 4	22 12
18	21 57	22 10	22 9	22 0	22 4
19	22 12	22 11	22 16	22 8	22 12
20	22 0	22 16	22 16	22 0	22 8
21	21 59	22 12	22 15	22 5	22 8
22	22 0	22 21	22 15	22 10	22 11
23	22 12	22 13	22 15	22 14	22 13
24	22 5	22 10	22 15	22 5	22 8
Means	22 14	22 14	22 14	22 7	

Mean of all 22 12.

DIPPING NEEDLE.

	7 h. A. M.	12 h. M.	2 h. P. M.	10 or 11 h. P. M.	Mean.	
	° ′	° ′	° ′	° ′	° ′	
July 11	72 5	72 10	71 40	72 0		
12	72 15	72 10	72 20	72 10	72 9	Eaſt mark uppermoſt.
13	72 15	72 10	72 20	72 10		
14	72 20		72 5	72 10		
15	73 5	73 0	72 55	72 50	72 59	Weſt.
16	73 0	72 55	72 50	73 10		
17	73 0	73 0	73 0	71 45		
18	71 45	71 45	71 55	72 0	71 43	Eaſt mark down.
19	71 55	71 50	71 50	71 40		
20	71 55	71 53	72 15	72 0		
21	71 50	71 50	72 45	72 50		
22	72 45	72 55	72 50	72 55		
23	72 50	72 50	73 10	72 50	72 51	Weſt.
24	72 40	72 55	72 50	72 50		
					72 25	

THE END OF PART I. VOL. LXVIII.

Errata in the Meteorological Journal for the year 1776

Vol. LXVII. Part I. Page 382.

Rain in January, 1776,	*for*	1,157,	*read*	1,167.
——— November,	—	1,191,	—	2,191.
——— whole year,	—	20,354,	—	21,364.

PHILOSOPHICAL TRANSACTIONS.

PART II.

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXVIII. For the Year 1778.

PART II.

LONDON,

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MDCCLXXIX.

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XLVI.

PHILOSOPHICAL TRANSACTIONS.

XXVI. *An Account of the Iſland of* St. Miguel. *By Mr.* Francis Maſſon, *in a Letter to Mr.* William Aiton, *Botanical Gardener to His Majeſty. Communicated by* Joſeph Banks, *Eſq. F. R. S.*

St. Miguel,
Auguſt 10, 1777.

SIR,

Read April 2, 1778.

I HAVE viſited the greateſt parts of this iſland, and find that its productions differ greatly from thoſe of Madeira, inſomuch that none of the trees of the latter are found here, except the *faya*: it has

has a nearer affinity to Europe than Africa. The mountains are covered with the *erica vulgaris*, and an elegant ever-green ſhrub very like a *phillyrea*, which gives them a moſt beautiful appearance. Notwithſtanding this iſland has been frequently viſited by Europeans, I cannot help communicating to you a few of its ſingularities.

It is one of the principal and moſt fertile of the Azorian iſlands lying nearly Eaſt to Weſt; its length is about eighteen or twenty leagues; its breadth unequal, not exceeding five leagues, and in ſome places not more than two. It contains about eighty thouſand inhabitants.

Its capital the city of Ponta del Guda, which contains about twelve thouſand inhabitants, is ſituated on the South-ſide of the iſland, on a fine fertile, plain country, pretty regularly built; the ſtreets ſtrait, and of a good breadth. It is ſupplied with good water, which is brought about the diſtance of three leagues from the neighbouring mountains. The churches and other religious edifices are elegant and well built for ſuch an iſland. There is a large convent of Franciſcan friars and one of the order of St. Auguſtin, four convents for profeſſed nuns, and three Recolhimentos for young women and widows who are not profeſſed. The veſſels anchor in an open road; but it is not dangerous, as no

wind

wind can prevent their going to ſea in caſe of ſtormy weather.

The country round the city is plain for ſeveral miles, well cultivated, and laid out with good taſte into ſpacious fields, which are ſown with wheat, barley, Indian corn, pulſe, &c. and commonly produce annually two crops; for as ſoon as one is taken off, another is immediately ſown in its place. The ſoil is remarkably gentle and eaſy to work, being for the moſt part compoſed of pulverized pumice ſtone. There are in the plains a number of pleaſant country ſeats, with orchards of orange trees, which are eſteemed the beſt in Europe.

The ſecond town is Ribeira Grande, ſituated on the North-ſide of the iſland, containing about as many inhabitants as the city; a large convent of Franciſcan friars, and one of nuns. It gives title to a count, called the Conde Ribeira Grande, who firſt inſtituted linen and woollen manufactories in the iſland.

The third town is Villa Franca, on the South-ſide of the iſland, about ſix leagues Eaſt of Ponta del Guda. It has a convent of Franciſcan friars, and one of nuns, which contains about three hundred. Here, about half a mile from the ſhore, lies a ſmall iſland (Ilhao) which is hollow in the middle, and contains a fine baſon with only one entrance into it, fit to hold fifty ſail of veſſels

ſecure from all weather; at preſent it wants cleaning out, as the winter's rain waſhes down great quantities of earth into it, which has greatly diminiſhed its depth. But veſſels frequently anchor between this iſland and the main.

Beſide theſe towns are ſeveral ſmaller, *viz.* Alagoa, Agoa de Pao, Brelanha, Fanaes de Ajuda, and a number of hamlets, called Lugars, or Places.

About four leagues North-eaſt from Villa Franca lies a place called the Furnas, being a round deep valley in the middle of the Eaſt part of the iſland, ſurrounded with high mountains, which, though ſteep, may be eaſily aſcended on horſeback by two roads. The valley is about five or ſix leagues in circuit, the face of the mountains, which are very ſteep, is intirely covered with beautiful ever-greens, *viz.* myrtles, laurels, a large ſpecies of bilberry, called *uva de ſerra*[(a)], &c. and numberleſs rivulets of the pureſt water run down their ſides. The valley below is well cultivated, producing wheat, Indian corn, flax, &c. The fields are planted round with a beautiful ſort of poplars, which grow into pyramidal forms, and by their careleſs, irregular diſpoſition, together with the multitude of rivulets, which run in all directions through the valley, a number of boiling fountains, throwing up

(a) Mountain grapes.

clouds

clouds of ſteam, a fine lake in the South-weſt part about two leagues round, compoſe a proſpect the fineſt that can be imagined. In the bottom of the valley the roads are ſmooth and eaſy, there being no rocks but a fine pulverized pumice ſtone that the earth is compoſed of.

There are a number of hot fountains in different parts of the valley, and alſo on the ſides of the mountains: but the moſt remarkable is that called the Caldeira, ſituated in the Eaſtern part of the valley, on a ſmall eminence by the ſide of a river, on which is a baſon about thirty feet diameter, where the water continually boils with prodigious fury. A few yards diſtant from it is a cavern in the ſide of the bank, in which the water boils in a dreadful manner, throwing out a thick, muddy, unctuous water ſeveral yards from its mouth with a hideous noiſe. In the middle of the river are ſeveral places where the water boils up ſo hot, that a perſon cannot dip his finger into it without being ſcalded; alſo along its banks are ſeveral apertures, out of which the ſteam riſes to a conſiderable height ſo hot that there is no approaching it with one's hand: in other places, a perſon would think, that a hundred ſmiths bellows were blowing altogether, and ſulphureous ſteams iſſuing out in thouſands of places, ſo that native ſulphur is found in every chink, and the ground covered with it

like

like hoar froſt; even the buſhes that happen to lay near theſe places are covered with pure brimſtone, condenſing from the ſteam that iſſues out of the ground, which in many places is covered over with a ſubſtance like burnt allum. In theſe ſmall caverns, where the ſteam iſſues out, the people often boil their yams (*inhames*).

Near theſe boiling fountains are ſeveral mineral ſprings; two, in particular, whoſe waters have a very ſtrong mineral quality, of an acid taſte and bitter to the tongue.

About half a mile to the Weſtward, and cloſe by the river ſide, are ſeveral hot ſprings, which are uſed by ſick people with great ſucceſs. Alſo on the ſide of a hill, Weſt of St. Ann's church, are many others, with three bathing houſes, which are moſt commonly uſed. Theſe waters are very warm, although not boiling hot; but at the ſame place iſſue ſeveral ſtreams of cold mineral water, by which they are tempered, according to every one's liking.

About a mile South of this place, and over a low ridge of hills, lies a fine lake about two leagues in circumference, and very deep, the water thick, and of a greeniſh colour. At the North-end is a plain piece of ground, where the ſulphureous ſteams iſſue out in many places, attended with a ſurprizing blowing noiſe. I could obſerve

ſerve ſtrong ſprings in the lake, but could not determine whether they were hot or cold: this lake ſeems to have no viſible evacuation. The other ſprings immediately form a conſiderable river, called Ribeira Quente(b), which runs a courſe about two or three leagues, through a deep rent in the mountains, on each ſide of which are ſeveral places where the ſmoak iſſues out. It diſcharges itſelf into the ſea on the South ſide, near which are ſome places where the water boils up at ſome diſtance in the ſea.

This wonderful place had been taken little notice of, until very lately; ſo little curioſity had the gentlemen of the iſland, that ſcarcely any of them had ſeen it, until of late ſome perſons afflicted with very virulent diſorders, were perſuaded to try its waters, and found immediate relief from them. Since that time it has become more and more frequented; ſeveral perſons who had loſt the uſe of their limbs by the dead palſy have been cured; and alſo others who were troubled with eruptions on their bodies.

A clergyman, who was greatly afflicted with the gout, tried the ſaid waters, and was in a ſhort time perfectly cured, and has had no return of it ſince. When I was there, ſeveral old gentlemen, who were quite worn out

(b) Hot river.

with the ſaid diſorder, were uſing the waters, and had received incredible benefit from them; in particular, an old gentleman, about ſixty years of age, who had been tormented with that diſorder more than twenty years, and often confined to his bed for ſix months together: he had uſed theſe waters about three weeks, had quite recovered the uſe of his limbs, and walked about in the greateſt ſpirits imaginable. A friar alſo who had been troubled with the ſaid diſorder about twelve years, and reduced to a cripple, by uſing them a ſhort time was quite well, and went a hunting every day. There are many other inſtances of the efficacy of theſe waters, which, for the ſake of brevity, I muſt here omit.

There are ſeveral other hot ſprings in the iſland, particularly at Ribeira Grande; but they do not poſſeſs the ſame virtues, at leaſt not in ſo great a degree.

The Eaſt and Weſt part of the iſland riſes into high mountains; but the middle is low, interſperſed with round conic hills, all of which have very recent marks of fire; all the parts below the ſurface conſiſting of melted lava laying very hollow.

Moſt of the mountains to the Weſtward have their tops hollowed out like a punch-bowl, and contain water. Near the Weſt end is an immenſe deep valley, like the Furnas,

Furnas, called the Sete Cidades[c]. This valley is ſurrounded with very abrupt mountains, about ſeven or eight leagues round; in the bottom is a deep lake of water, about three leagues in circuit, furniſhed with great number of water fowls. This water has no mineral quality; neither are there any hot ſprings in the valley. All theſe mountains are compoſed of a white crumbly pumice ſtone, which is ſo looſe, that, if a perſon thruſt a ſtick into the banks, whole waggon loads of it will tumble down. The inhabitants of the iſland relate a ſtory, that he who firſt diſcovered it obſerved an extraordinary high peak near the Weſt-end; but the ſecond time he viſited it no ſuch peak was to be ſeen, which he ſuppoſed muſt have certainly ſunk; but, however improbable this ſtory may be, at ſome period or another it muſt have certainly been the caſe.

If you ſhould think the account of the mineral waters of any ſervice to the public, they are very welcome to it; and, ſhould any perſon venture ſo far for his health, a ſmall ſtock of the ſuperfluities of life only need to be laid in, as the iſland yields every neceſſary. The climate is very temperate: the thermometer ſince I have been here has been no higher than 77°, commonly from 70° to 75°.

(c) Seven Cities.

 I have

I have ſent you twelve or thirteen bottles as a ſpecimen, which are as follows:

N° 1. From a cold fountain giving a ſtrong acid water, lying South-eaſt from the boiling Caldeira.

N° 2. From a ſtrong cold mineral fountain, about twenty yards nearer the Caldeira.

N° 3. From a cold mineral ſpring in the valley of Foze de Pont.

N° 4. A hot mineral water, from the bathing place near the river.

N° 5. A hot mineral water, from the upper bathing place.

N° 6. From the great Caldeira.

N° 7. From a thick boiling fountain near the Caldeira.

Alſo earth from all theſe fountains, with their correſponding numbers.

XXVII. *An Account of a remarkable Imperfection of Sight. In a Letter from* J. Scott *to the Rev. Mr.* Whiſſon, *of* Trinity College, Cambridge. *Communicated by the Rev.* Michael Lort, *B. D. F. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Old Bond-ſtreet,
April 4, 1778.

S I R,

Read April 9, 1778.

A FRIEND of mine, the rev. Mr. WHISSON, of Trinity College, Cambridge, being acquainted with a gentleman in Lincolnſhire, who labours under an inability of diſtinguiſhing colours ſimilar to that of which an account is given in the laſt volume of the Philoſophical Tranſactions, did write to him for ſome particular information concerning this infirmity. The anſwer which Mr. WHISSON received I have now, by his permiſſion, the honour of tranſmitting to you, to be communicated to the Royal Society, if you ſhall think it worthy their attention.

I am, &c.

MICHAEL LORT.

TO THE REV. MR. WHISSON.

REV. SIR, Rafen, May 26, 1777.

I RECEIVED your favour in due time. I fhould have given you my anfwer fooner, but have been greatly afflicted with the gout. I am very willing to inform you (and take your inquiry as a favour) of my inability concerning colours, as far as I am able from my own common obfervation.

It is a family failing: my father has exactly the fame impediment: my mother and one of my fifters were perfect in all colours: my other fifter and myfelf alike imperfect: my laft mentioned fifter has two fons both imperfect; but fhe has a daughter who is very perfect: I have a fon and daughter, who both know all colours without exception; and fo did their mother: my mother's own brother had the like impediment with me, though my mother, as mentioned above, knew all colours very well.

Now I will inform you what colours I have the leaft knowledge of. I do not know any green in the world; a pink colour and a pale blue are alike, I do not know one from the other. A full red and a full green the fame,

ſame, I have often thought them a good match; but yellows (light, dark, and middle) and all degrees of blue, except thoſe very pale, commonly called ſky, I know perfectly well, and can diſcern a deficiency, in any of thoſe colours, to a particular nicety: a full purple and deep blue ſometimes baffle me. I married my daughter to a genteel, worthy man a few years ago; the day before the marriage he came to my houſe, dreſſed in a new ſuit of fine cloth cloaths. I was much diſpleaſed that he ſhould come (as I ſuppoſed) in black: ſaid, He ſhould go back to change his colour. But my daughter ſaid, No, no; the colour is very genteel; that it was my eyes that deceived me. He was a gentleman of the law, in a fine rich claret-coloured dreſs, which is as much a black to my eyes as any black that ever was dyed. She has been married ſeveral years; no child living, and my ſon is unmarried; ſo how this impediment may deſcend from me is unknown.

I have a general good ſatisfaction in the midſt of this my inability; can ſee objects at a diſtance when I am on travel with an acquaintance, and can diſtinguiſh the ſize, figure, or ſpace, equal to moſt, and I believe as quick, colour excepted.

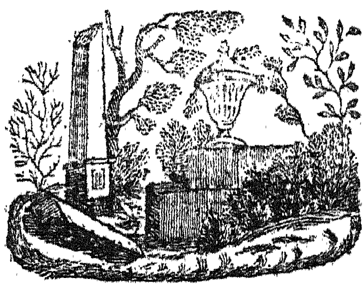
My

My buſineſs was behind a counter many years, where I had to do with variety of colours. I often, when alone, met with a difficulty; but I commonly had a ſervant in the way to attend me, who made up my deficiency. I have been now ſeven years from trade. My eyes, thank God, are very good at diſcerning men and things.

If your learned Society can ſearch out the cauſe of this very extraordinary infirmity, and find a method for an amendment, you will be ſo obliging to acquaint me.

I am, &c.

J. SCOTT.



XXVIII. *An Account of Baptiſms, Marriages, and Burials, during Forty Years, in the Pariſh of* Blandford Forum, Dorſet. *Communicated by* Richard Pulteney, *M. D. F. R. S.*

TO DR. WATSON, F. R. S.

SIR,

Read Mar. 19, 1778.

AS the ſubjects of the following tables have been much diſcuſſed of late, I take the liberty of putting theſe into your hands, encouraged thereto by your aſſurances, when I had the pleaſure of ſeeing you laſt in town, that they might not improperly be laid before the Royal Society. If upon inſpection you ſhould continue to think ſo, you will do me the honour of preſenting them.

Excluſive of general curioſity, I was alſo induced to examine the regiſter of this place, in order to determine how far the general opinion, which gives the credit of extraordinary ſalubrity to this country, was founded on fact, as far as that could be proved by this mode of examination. I found that the regiſter had been kept with ſufficient

sufficient accuracy in the usual method; that, since the recovery of this place from the dreadful fire of 1731, scarcely any new houses had been built, at least on any new site; that no fluctuation in the number of inhabitants from the introduction of any new manufactory, or from any other apparent cause, had taken place; and was therefore led to conclude, that the result might prove a tolerably fair one, and more especially upon finding the excess of the baptisms above the burials so small as it appears to be.

In the year 1773, when these tables were drawn up, I also procured the number of the people to be taken; by which it appeared, that there were 446 families, reckoning a workhouse of 44 persons, and three schools containing 92, as four families only, which gives nearly $4\frac{3}{4}$ to a family. The whole number of souls in the parish was found to be, males 1174, females 936, total 2110; but it must be remarked, that in this number were not included any of the inhabitants of the close adjoining villages of St. Mary, Blandford, Brianston, or Langton.

From an inspection of table the first it will be seen, that $55\frac{3}{4}$ is nearly the average of deaths for 40 years; and the average of the last ten years will appear to be 54: hence, taking 55 for the average number, which also will be seen to be the exact number of burials in the

year

year preceding that of the survey, it will be seen that about 1 in 38 or 39 dies yearly; and, as it can scarcely be doubted, that the errors of a survey must be on the side of omission, it may not be too much to allow 1 in 39 only.

From this result, therefore, it is manifest, that although the salubrity of this place does not equal what we read of some particular villages in the North of England, or of the district of Vaud, yet it turns out very much in favour of the general opinion, and very far beyond that of London and larger towns, where the number is found to be from 1 in 20 to 1 in 26 or 28.

I am, &c.

TABLE I. A table of the baptisms, marriages, and burials, during forty years, in the parish of Blandford Forum.

	Baptisms, Males.	Baptisms, Females.	Baptisms, Total.	Marriages.	Burials, Males.	Burials, Females.	Burials, Total.		Baptisms, Males.	Baptisms, Females.	Baptisms, Total.	Marriages.	Burials, Males.	Burials, Females.	Burials, Total.
1733	39	33	72	33	27	27	54	1753	22	24	46	24	54	42	96
1734	35	32	67	27	26	20	46	1754	36	18	54	18	24	38	62
1735	34	25	59	24	17	26	43	1755	30	21	51	8	35	39	74
1736	39	22	61	27	29	17	46	1756	35	23	58	23	46	40	86
1737	24	52	76	29	36	29	65	1757	21	11	32	17	29	25	54
1738	38	29	67	30	26	25	51	1758	24	29	53	23	15	23	38
1739	31	27	58	15	25	30	55	1759	20	29	49	13	22	32	54
1740	25	22	47	14	33	31	64	1760	24	25	49	19	35	15	50
1741	21	16	37	18	46	56	102	1761	35	20	55	24	28	15	43
1742	34	24	58	20	36	28	64	1762	17	26	43	18	32	26	58
1743	22	21	43	33	27	27	54	1763	30	16	46	26	15	20	44
1744	32	32	64	23	16	18	34	1764	38	32	70	24	24	26	41
1745	20	24	44	7	23	26	49	1765	38	30	68	18	37	28	65
1746	21	29	50	24	21	26	47	1766	22	25	47	17	50	54	104
1747	21	27	48	18	21	31	52	1767	32	25	57	20	12	21	33
1748	28	19	47	26	34	33	67	1768	23	30	53	15	26	33	59
1749	26	32	58	28	15	25	40	1769	39	24	63	25	23	23	46
1750	24	29	53	26	23	23	46	1770	23	31	54	20	19	28	47
1751	26	19	45	18	22	27	49	1771	28	25	53	20	27	19	47
1752	27	33	60	8	21	26	47	1772	29	31	60	12	21	34	55
	567	547	1114	448	524	551	1075		566	495	1061	384	575	581	1156

	Total baptiſms.	Marriages.		Total burials.
Males	1133	832	Males	1099
Females	1042		Females	1132
	2175			2231

The baptiſms among the Diſſenters are brought into this account only during the laſt ten years, males 39, females 54, total 93: if therefore the ſame proportion is taken for the firſt-three decennial periods, the total of the baptiſms will amount to 2454.

Small-pox years, of which diſtemper died as underneath:

In 1741	-	76
1753	-	40
1766	-	44
		160

In the year 1756, among the burials, 18 were of ſoldiers, who died during the time of the encampment.

This table does not include the cloſe adjoining villages of Langton, Brianſton, and Blandford St. Mary.

TABLE II. The annual average of baptiſms and burials in the four decennial periods of the foregoing table, taking among the baptiſms 93 in each period for the number among the Diſſenters, which appeared to be the exact number baptized in the fourth period.

Annual average of the baptiſms in the			Annual average of the burial in the		
firſt ten years,	$69\frac{1}{2}$	695	firſt ten years,	59	590
—— ſecond ten years,	$60\frac{1}{2}$	605	—— ſecond ten years,	$48\frac{1}{2}$	485
—— third ten years,	$58\frac{3}{10}$	583	—— third ten years,	$61\frac{1}{2}$	615
—— fourth ten years,	$57\frac{1}{10}$	571	—— fourth ten years,	54	541
In the 40 years,	$61\frac{1}{4}$	2454	In the 40 years,	$55\frac{3}{4}$	2231

The exceſs of baptiſms over the burials in forty years, 223.

TABLE III. Shewing the whole number of burials in each month, and each quarter of the year collectively throughout the whole of the above period, diſpoſed in two columns, in the latter of which, thoſe who died of the ſmall-pox are excluded, ſerving to illuſtrate the different ſalubrity of the ſeaſons.

Winter.			Summer.		
January	220	204	July	146	145
February	218	200	Auguſt	144	144
March	223	195	September	165	162
	661	599		455	451
Spring.			**Autumn.**		
April	193	170	October	172	171
May	218	184	November	174	170
June	190	166	December	168	160
	601	520		514	501

XXIX. *Part of a Letter from* Matthew Guthrie, *M. D. of* Peterſburgh, *to Dr.* Prieſtley, *F. R. S. on the Antiſeptic Regimen of the Natives of* Ruſſia.

Read April 30, 1778.

READING the other day the elegant oration of Sir JOHN PRINGLE, on the great merit of Captain COOK, for which old Rome would have loaded his ſhip with civic crowns, one part of the learned preſident's diſcourſe drew my attention in particular, as it regarded this country, and touched upon a ſubject which I have long paid attention to, *viz.* the antiſeptic regimen which nature has dictated to the peaſants of this empire. Nothing ſeems clearer to me than that, if nature had not taught theſe people habits, and given them a taſte which galloping travellers treat with contempt, they muſt undoubtedly have ſunk under the ſcurvy, as they are, for the greateſt part of the year, expoſed to the influence of thoſe pre-diſpoſing cauſes to putrid complaints that make the body of the Greenland ſeaman livid; yet under all theſe diſadvantages ſuch ſeems to be the efficacy of the regimen they obſerve, that putrid diſeaſes are ſtrangers to their huts, and the

Ruſſian boor enjoys a ſtate of health that aſtoniſhes an inhabitant of a country where the dreadful conſequences are ſo well known of bad air within, exceſſive cold without, joined to a want of freſh vegetables for a length of time. I think you will by no means have your reſpect diminiſhed for the late diſcovered antiſeptic agent, when I have given in detail the multitude of enemies it has to encounter, in preſerving from putrid attacks the bodies of the people I am treating of.

The Ruſſian boor lives in a wooden houſe, made with his own hatchet, his only inſtrument, in the uſe of which he is moſt dextrous: it is caulked with moſs, ſo as to be very ſnug and cloſe. It is furniſhed with an oven, which anſwers the triple purpoſe of heating the houſe, dreſſing the victuals, and ſupporting on its flat top the greaſy mattraſs on which he and his wife lie. From over the oven, which is on one ſide of the room, are laid ſome boards reaching to, and ſupported by, the oppoſite wall, raiſed a little above the ſtove, ſo as to receive its heated air. On thoſe ſleep the children and ſecondary perſonages of the hut; for the oven itſelf is a luxury reſerved for the firſt. Round the room runs a bench with a table in the middle, and in the corner is a ſort of cupboard for the reception of ſaints, before whom ſmall tapers frequently burn, or a lamp with hemp oil. During the long ſevere winter

ſeaſon,

ſeaſon, the cold prevents them from airing this habitation, ſo that you may eaſily conceive, that the air cannot be very pure, conſidering that four, five, or ſix people eat and ſleep in one room, and undergo, during the night, a moſt ſtewing proceſs from the heat and cloſeneſs of their ſituation; inſomuch that they have the appearance of being dipped in water, and raiſe a ſteam and ſmell in the room, not offenſive to themſelves, but ſcarcely ſupportable to the perſon whom curioſity may lead thither.

Now if it be conſidered, that this human effluvium muſt adhere to every thing in the room, eſpecially to the ſheep ſkins or mattraſs on which they ſleep, the moſs in the walls, &c. and that the apartment is never ventilated for ſix months at leaſt; at the ſame time that theſe people are living occaſionally upon ſalt fiſh or meat, and the whole time without freſh vegetables, expoſed likewiſe when out of doors to a ſevere cold atmoſphere, the ſcorbutic tendency of which is well known: I ſay, when all theſe circumſtances are taken into conſideration, if it be a fact that they are, in ſpite of all thoſe pre-diſpoſing cauſes, ſtrangers to putrid diſeaſe, it will ſufficiently juſtify my firſt aſſertion, that the regimen nature has dictated to theſe people is moſt highly antiſeptic, and it may be doing ſervice to mankind to deſcribe it minutely.

This I ſhall endeavour to do, and it will probably give pleaſure to thoſe gentlemen, who have preſcribed the new regimen to the Britiſh navy with ſo much ſucceſs, to have the evidence of ſome millions to prove, that they have actually hit upon the very ſecret by which nature defends her creatures, in thoſe countries where it is neceſſary, from the very diſeaſe which has been the ſcourge of the nobleſt naval eſtabliſhment that ever the world ſaw. Nay, one would think that the diet theſe people uſe had been dictated by modern philoſophy, or rather that your Preſident, your MACBRIDE'S, &c. had ſtudied at this ſchool; for almoſt every thing they uſe ſeems to be of that kind which the fortunate attention to the antiſeptic qualities of fixed air has recommended for medical uſe. Here the experimental philoſopher may be indulged in a triumph; and I really think your lords of the admiralty ought in gratitude to erect ſtatues to the induſtrious and ſucceſsful proſecutors of that noble and uſeful ſtudy.

The only part of the food of our Northern people, that does not come under the deſcription given, is ſalt meat and fiſh; the latter they eat during their faſts where freſh fiſh cannot be procured, at leaſt not upon terms that ſuit their circumſtances; and there are alſo ſome places where the ſcarceneſs of fodder during the

winter obliges them to live much upon ſalt meat; yet in all theſe caſes they manage to correct the action of this additional leaven of putridity by mixture with their prepared vegetables, in ſuch a manner as to elude its baneful effects, which furniſhes me with another corroborating proof of the powerful antiſeptic qualities of this mode of preparation, which I ſhall particularly deſcribe, and what in fact is the main purpoſe of this paper, in hopes thereby to throw ſome additional light upon the new antiſcorbutic ſyſtem which cannot be too well underſtood, and in hopes that ſome of the many diſhes I ſhall deſcribe of a ſimilar nature with your ſour cabbage now in uſe in the Britiſh navy, may be thought worth a place in your marine antiſcorbutic bill of fare; and if I am ſo happy as to contribute to the preſervation of the lives of the gallant corps of men that enables us to plant our cabbages in ſafety at home, I ſhall think my trouble well rewarded.

One of their principal articles of food, and what enters into the compoſition of moſt of the Ruſſian ſoups, is their ſour cabbage, which you are already ſo well acquainted with, both as to the preparation and qualities of it, that it becomes unneceſſary to do more than juſt give it the firſt place in detailing their antiſcorbutic diſhes, which it certainly merits.

The ſecond capital article is called quaſs, a liquor which not only ſerves them for drink, but alſo as ſauce to a number of diſhes, eſpecially to ſuch as have a tendency to bring on the diſeaſe which their ſituation threatens, and is the baſis of the favourite cold ſoup of the North, which is made by adding cold meat cut in pieces with cucumbers (prepared after a manner to be deſcribed in the ſequel) or with onions, or garlick, to a bowl of this ſub-acid liquor. This ſeems to be a good method of qualifying and eating ſalt meat to thoſe that are fond of the acid taſte, and ſhould make the proceſs in the ſtomach very different from what we muſt ſuppoſe is the caſe when ſalt beef is eaten off a biſcuit, accompanied with nothing but what ſerves for a plate, or the ſuet pudding of the navy, judging from ſome experiments I have made in the ſtile of Dr. MACBRIDE's alimentary mixtures.

The manner of preparing the common Ruſs quaſs.

They take a large potful of cold water, and put into it as much rye-flour as will make a thin dough: they then place it in an oven, moderately heated, for three hours, at which time they take it out, and throw it into a tub of cold water: this mixture they work until it

froths with a machine resembling the staff of a chocolate pot, but larger. To this liquor, thus prepared, is added a couple of slop-basons full of the grounds of old quass, leaven, or, if these are not to be procured, which can scarce happen in Russia, they use as a ferment a piece of their sour bread, and cover the tub with a cloth to keep out the dust, until the liquor has acquired a sourish taste, which marks its being ready for use. However, this depends upon the temperature of the weather, as it acquires the necessary acidity sooner or later, according to the season or degrees of artificial heat that is employed. This liquor the poorest of the people drink as they draw it from the tub or cask where it is kept for use; but there is a superior kind of quass, which the better sort of people make and bottle for their common use; indeed people of the highest rank love and use it constantly.

The better sort of Quass, or Keesla Stchee.

They take one pood (thirty-six pounds English) of rye, flour, or meal, and half that quantity of ground malt, and put them into a tub made for the purpose with a close cover, pouring a kettle-full of scalding water, stirring with a stick as they pour, and then cover it close up for an hour; at the expiration of which time they add

boiling

boiling water in the ſame manner as before, until it becomes as thin as ſmall-beer. The tub is then placed in a cool ſituation for ſome hours, the cover being kept half open with a ſtick; then the liquor is paſſed through a ſieve into a caſk, and two baſons full of old quaſs, or the ſubſtitutes mentioned in the laſt receipt, are added, and the veſſel placed in a cellar or cool ſituation for five or ſix days, until it acquires the ſub-acid taſte, when it is fit for bottling.

Here ſeems to be an elegant improvement of Dr. MACBRIDE's infuſion of malt, for the acidulous taſte makes it highly palatable and refreſhing, and probably there may be a virtue in this ſpecies of acidity, which is perhaps the only thing that the ſweet infuſion wants, to give it all the antiſcorbutic qualities of your ſour krout, &c. as it alſo abounds in the antiſeptic fluid fixed air which recommends the other for medical purpoſes, and particularly as an antiſcorbutic; at the ſame time that the fermentation is permitted to run on until it acquires the acid taſte which I obſerve every one of the efficacious vegetable preparations uſed in the North is poſſeſſed of, and what nearly ſeems to be the ſecret alone by which theſe people preſerve them for a length of time, and put them upon an equality with freſh vegetables, as one would be led to think by their ſalutary effects.

4 The

The very bread that our people make uſe of has alſo acquired this acidity before it is judged wholeſome, and adapted to their conſtitutions.

The manner of making the Ruſſian *rye bread.*

In the morning they mix as much rye flour with warm milk, water, and a baſon full of grounds of quaſs, or leaven, as will make a thin dough, and beat it up for half an hour with the chocolate ſtaff before deſcribed; this they ſet in a warm place till night, then they add more meal by degrees, working it up at the ſame time with the ſtaff, until the dough becomes ſtiff. They then return it to its warm ſituation until morning, at which time they throw in a proper quantity of ſalt, and work it with the hand into a proper conſiſtence for bread (they think the longer this laſt operation is continued the better) then they place it before the fire until it riſes, when it is cut into loaves, and returned once more into the warm place where it before ſtood, and kept there for an hour before the laſt part of the proceſs, the baking, which compleats it.

For ſea proviſion they cut the ſame ſour dough into biſcuits or ruſk, and dry them in the oven. This, I am told by very intelligent ſea officers, makes a moſt uſeful

and wholeſome article of food, ever at hand to qualify the ſeamen's ſalt proviſions, which they commonly eat in form of brothin the Ruſſian navy, with the addition of this bread, which is put in as we do the white bread in our ſoups of that name, or they take off the ſaltneſs of their ſea beef by making it into ſoup with their prepared vegetables; but never ſuffer their ſailors to eat it dry as they call it, being of opinion that it promotes the ſcurvy in the fleet.

This ruſk alſo not only anſwers the common purpoſe of bread, but when thrown into warm water produces their favourite liquor quaſs, with or without the addition of ground malt: and I am likewiſe told, that they put this laſt article into the ſour dough, with which they make a ſort of ruſk for the purpoſe of quaſs alone.

There are prepared cucumbers which are eaten with meat in this country, and the people are remarkably fond of them. They are called ſalted cucumbers, as ſalt is the principal ingredient uſed in the preparation; but they have the ſame ſouriſh taſte ſo often mentioned, and ſeem to have their ſhare alſo in the merit aſcribed to the regimen at large.

The manner of preparing the Ruſſian *ſalted cucumbers.*

They put any quantity of cucumbers into a caſk, and as much cold water as covers them, with four or five handfuls of ſalt, ſome oak and black currant leaves, ſome dill and garlick. They then ſet the caſk into a cool place for about forty-eight hours, until the liquor taſtes ſouriſh, when they pour it off from the cucumbers into a pan, and add to it four or five handfuls of ſalt, then boil it for about fifteen minutes, and when cold return it into the caſk to cover the cucumber, which they now bung up for uſe, and place in the cellar, where they become criſp and fit to be eaten in three or four days, and are counted a luxury by their admirers, amongſt which number I cannot reckon myſelf; however, this is a matter of palate.

To conclude this ſubject, there are ſtill a few other diſhes to be mentioned that ſeem to have the ſame tendency as theſe already deſcribed: *viz.* what is called ſooins in Scotland, and much uſed by the common people there. It is an infuſion of oat-meal bran in warm water, left to ferment until it acquire the ſouriſh taſte, and then ſtrained and boiled to a conſiſtence. Another of their diſhes is compoſed of rye-meal, ground malt,

and

and water, as thick as cream, which is placed all night in the oven, previouſly heated to a moderate degree, and in the morning a piece of ſour rye bread is added to effect their favourite end, and the meſs eaten when cold.

Horſe-radiſh they dry in the oven and keep all winter, which they powder, when wanted, and mix with vinegar to eat with ſalt fiſh.

Turnips they preſerve during the winter in dry ſand (as they likewiſe do the large white radiſh); theſe they put into an earthen pot with a cloſe cover, and ſtew them in the oven, with their own juice alone, till perfectly ſoft, and then eat them with quaſs. When ſugar is added inſtead of quaſs, they make an elegant diſh, and proper in coughs and pectoral diſorders.

Oats they prepare and grind in the manner of malt, and make a ſort of flummery of this meal, which they eat with quaſs, their favourite ſauce; and ſometimes milk ſupplies its place for theſe ſorts of diſhes.

I believe I have now made mention of the greateſt part of their food and its preparation; and I will take the liberty to ſay, that it is a regimen ſo conſiſtent and uniformly calculated to ward off the diſeaſe that their ſituation threatens (even when viewed by the teſt of modern opinion and experience) that the moſt enlightened phyſician of our day could not have preſcribed a better,

 and

and perhaps you may think with me, that there are ſome articles in it which, from their cheapneſs and antiſcorbutic qualities, might be permitted to accompany, for trial, their old Northern companion ſour cabbage, who has, I ſuppoſe, been met with ſtraggling in Germany, where he was ſingly able to make head againſt all the dangers that their climate threatened; although in our more frigid realms it requires his whole united phalanx to keep us in ſafety.

However, after ſaying every thing of and for the food made uſe of by the people inhabiting the Northern parts of this extended empire, I muſt not omit to give the ſhare of merit that I think is due to ſome cuſtoms that I hinted at in the beginning, and which probably have their ſhare in effecting the great end treated of in this letter. Theſe are their cloathing, baths, and manner of ſleeping.

In the firſt place, they go very warmly cloathed when out of doors, although they wear nothing but a ſhirt and a pair of linen drawers when within; the legs and feet in particular are remarkably guarded againſt the cold by many plies of coarſe flannel, with a pair of boots over all, at the ſame time that their bodies feel all the warmth of ſheep-ſkin coats, and nothing is left open to the action of the air but the face and neck, which

laſt

laſt although never covered, yet coughs and ſore throats are ſeldom heard of: nay, they are diſorders that we ſhould almoſt forget to treat, if foreigners did not keep us in uſe.

Their religion happily conſpires with the unavoidable bodily dirtineſs attached to their ſituation to ſend them to their vapour baths once or twice a week: here they waſh away with aqueous vapour, and afterwards with water in its condenſed ſtate, the dirt that by obſtructing the pores is ſo well known to promote putrid diſeaſes, at the ſame time that they moſt effectually open the cuticular emunctories, and throw off any obſtructed perſpiration that might have otherwiſe acted as a *fomes* to begin the ſeptic proceſs in the body; and laſtly, they undergo nightly, as I mentioned in the introduction, a degree of perſpiration that enables our coach-men, for example, to ſit the whole day and ſevere winter evening on the box, or at leaſt out of doors, without once dreaming of what we call catching cold, as they throw off every night what may have been retained in the day, and, to uſe a vulgar phraſe, may be ſaid to clear out as they go; but keep them from the nocturnal luxury of their oven, and you kill them in a week.

I muſt here obſerve, that, excepting the judicious ſeaman Captain COOK, I have not in my reading met with

any perſon that has paid the indiſpenſable attention to warm cloathing of ſailors in cold climates, which, we are taught by experience in thoſe countries, is a moſt neceſſary precaution to preſerve health: however, as to enter upon this ſubject at preſent would ſwell my letter to a ſtill more prodigious ſize, I will rather make it the ſubject of ſome future one, as the effects of our winter atmoſphere will merit particular attention, eſpecially when the opinionated obſtinacy of new arrived foreigners brave its fury in a more Southern dreſs, inſtead of taking a hint, like leſs ſyſtematic men, from the experience that a ſucceſſion of ages has taught the natives.

XXX. *Astronomical Observations made in the* Austrian Netherlands *in the Years* 1773, 1774, *and* 1775. *By* Nathaniel Pigott, *Esq. F. R. S. Foreign Member of the Academies of* Brussels *and* Caen, *and Correspondent of the Royal Academy of Sciences at* Paris.

TO THE REV. DR. MASKELYNE.

REV. SIR,

Read May 7, 1778.

THE inclosed observations are a sequel to those I communicated to you in 1775, and which are printed in the LXVIth vol. of Phil. Transf. for the year 1776.

Louvain being a very considerable and the only university in the Austrian Netherlands, and upon that account a town of very great note, I was particularly desirous of settling its longitude and latitude with all the accuracy my situation and time would admit of; and the more so, that I believe myself to be the first person who ever made any astronomical observations in that place, and

and the first, of course, who has determined its position with any degree of certainty: there is, however, reason to think, that the government there will, in a short time, provide an observatory, and supply the same with proper instruments; the first use of which will, no doubt, be to verify the inclosed observations. I spared no pains to render their results as accurate as possible. With this view I collected, by the help of my friends, all the observations of Jupiter's satellites I could, corresponding to each of ours; convinced by a mature consideration of this matter, that the most effectual method of obviating the unavoidable errors which arise from the different goodness and power of instruments from the different conformation of eyes, different states of the atmosphere and other circumstances, is to compare each observation to as great a number corresponding as possible, made in different places by known observers, whose longitudes from a given meridian are well determined, and then taking a mean arising from each such comparison. The amazing differences which not unfrequently are found in observations of this kind, even among astronomers of high note, confirm me still more in this opinion; and I own, I should prefer, in order to settle the longitude of any place by Jupiter's satellites, three or four immersions and as many emersions, made in favourable circumstances,

and

and compared with a great number of correfponding ones, made in different places, to a far greater number, which could but be compared with thofe of one or two aftronomers only. If this confideration be of the weight it appears to me, it has not been fufficiently attended to, and I mention it as it may be of ufe, particularly to young aftronomers.

The meridian zenith diftances of the Sun and ftars were taken with a quadrant one foot and a half radius, made by BIRD, exactly and very fteadily fixed in the plane of the meridian. The declinations of the fixed ftars, with the equations for aberration and nutation, were taken from the *Connoiffance des Tems*; the Sun's declination from the Nautical Almanac; its parallax and refractions, with the corrections for the barometer and thermometer, were computed from MAYER's tables publifhed by the Board of Longitude. It may not be improper to add, that, when I obferved both the upper and lower limbs of the Sun upon the meridian, I made the horizontal wire of the quadrant merely a tangent to the Sun's limb; for which reafon its diameter will come out by fuch obfervations confiderably greater than it really was.

For the obfervations of Jupiter's fatellites we ufed a reflector two feet and a half focal length, a telefcope by

SHORT,

SHORT, 18 inches focus, and sometimes an achromatic treble-object glass of the same length, and not much inferior to it, made by Mr. RAMSDEN; the times were got by a quadrant and a grid-iron pendulum.

It is necessary to mention the difference of meridians between Paris and the places where the observations were made to which we compared our own; the more so, as some are not generally known, and others differ a small matter, from what is commonly supposed and printed in the *Connoissance des Tems*.

M. MESSIER, in communicating a considerable number of observations, informed me in a letter of the longitudes of the following places:

	h	′	″
Corbeil East of R. Observatory at Paris, — —	0	0	38
Nolon, Mem. of Acad. of Sciences, 1764, —	0	3	45
Perinaldo, by nine corresponding observations of ♃'s first satellite,	0	21	33

By a letter from Mr. MALLET,

Geneva, — — — —	0	14	40

And by a letter on this occasion from M. BERNOULLI,

Berlin, East, — — — —	0	44	10
Milan, East of ditto, — — —	0	27	24
Petersburg, — — — —	1	52	0

By the Rev. Mr. HORNSBY'S letter,

Oxford, West of Greenwich, 4′ 59″; hence of Paris,	0	14	15

By Mr. DALBY'S letter,

Muswel-hill, 30″ West of Greenwich, and of Paris, —	0	9	46

The

The longitudes of the other obſervatories are aſſumed, as ſet down in the *Connoiſſance des Tems*, 1777, as being well ſettled.

Twenty ſeconds were added to, or ſubtracted from, the obſervations of the ſatellites made at Greenwich, with the ſix-feet reflector, on account of the great and ſuperior power of that inſtrument.

Although Jupiter's ſatellites obſerved at Bruſſels be emerſions only, nevertheleſs, as they agree with what the aſtronomers of the Academy of Sciences at Paris have done, as appears by the *Connoiſſance des Tems*, the reſult is well eſtabliſhed.

Being deſirous of affording every means of examining and forming a right judgement of theſe obſervations, I could not avoid ſaying thus much concerning them. It now remains for me to return you my thanks for the obliging communication of your obſervations.

I am, &c.

NATH. PIGOTT.

Latitude of the *Refuge de Vrowperg, Rue des Dominiquaines*, at Louvain.

			°	′	″
1774					
July	8.	By the Sun's lower limb — —	50	52	54,2
	9.	By the Sun's upper limb, — —	50	53	5,5
	11.	By ζ Ophiuci, — — —	50	53	6,8
	11.	By α Herculis, — — —	50	52	53,8
	11.	By α Ophiuci, — — —	50	52	54,9
	12.	By both limbs of the Sun observed, —	50	52	51,5
	14.	By the Sun's lower limb, — —	50	53	9,3
	14.	By α Herculis, — — —	50	52	54,1
	15.	By the Sun's lower limb — —	50	53	8,1
	15.	By α Ophiuci, — — —	50	52	58,1
	16.	By ζ Ophiuci — — —	50	53	4,2
	16.	By α Herculis, — — —	50	52	55,7
	16.	By α Ophiuci, — — —	50	53	3,2
	18.	By α Ophiuci, — — —	50	52	57,0
	19.	By observations of both limbs of the Sun, —	50	53	7,9
	20.	By α Herculis — — — —	50	52	55,3
	22.	By the Sun's upper limb, — —	50	53	6,7
	22.	By ζ Ophiuci, — — —	50	53	5,5
	22.	By α Herculis, — — —	50	52	55,8
	23.	By α Herculis, — — —	50	52	55,5
	24.	By the Sun's lower limb, — —	50	53	0,2
Sept.	30.	By both limbs of the Sun observed, —	50	52	51,1
1775					
Aug.	21.	By β Cygni, — — —	50	52	51,1
		The mean of the above, — —	50	52	59,4

Having

Having on the 21ft, 23d, 24th, 25th of April 1775, obferved the zenith diftances of ε and ζ urfæ majoris with the face of the quadrant to the Eaft and Weft, in order to determine the error of the line of collimation, which I found to be 25″,2, to be fubtracted from the obferved zenith diftances, I computed the latitude likewife from the fame ftars, and can depend the more upon the refult from them, as, being fo near the zenith, they were little affected by refraction, and not at all by the error of the line of collimation, becaufe obferved both on the quadrantal arc and arc of excefs.

	°	′	″
By ε urfæ majoris the latitude was found, —	50	53	1 +
By ζ urfæ majoris the latitude was, — —	50	53	9½
By a mean of the Sun and ftars as above, —	50	52	59,4
Now, if a mean of the whole be taken, the latitude North of my obfervatory at Louvain will be — —	50	53	3

Longitude of the *Refuge de Vrowperg*, *Rue des Dominiquaines*, at Louvain, deduced from observations of Jupiter's satellites.

Corresponding altitudes of Sun and stars.

				h	′	″
1774, Aug.	15.	Clock at noon, by six sets of the Sun,		11	39	10,5
	16.	Clock at noon, by seven ditto,	—	11	38	28,5
	22.	Clock at noon, by eight ditto,	—	11	34	45—
	23.	Clock at noon, by two ditto,	—	11	34	4+
	24.	Clock at noon, by six ditto,	—	11	33	23+
	25.	Clock at noon, by seven ditto,	—	11	32	24,5
	29.	α Aquilæ on meridian by the clock,	—	8	35	42+
	30.	Clock at noon, by four sets of the Sun,		11	29	22—
	31.	Clock at noon, by three ditto,	—	11	28	40+
1774, July	24.	Clock at noon, by four ditto,	—	0	2	9+
	29.	Clock at noon, by two ditto,	—	0	3	28—
Aug.	21.	Clock at noon, by four ditto,	—	11	59	51
	27.	Clock at noon, by three ditto,	—	11	59	36
Sept.	2.	Clock at noon, by four ditto,	—	11	58	59+
Oct.	5.	Clock at noon, by five ditto,	—	11	47	56+
	6.	Clock at noon, by three ditto,	—	11	47	42+
	14.	Clock at noon, by three ditto,	—	11	46	15
		α Aquilæ crossed a vertical by the clock,		6	27	24
	20.	α Aquilæ crossed the same vertical,		6	4	49
	21.	Clock at noon, by three sets of the Sun,		11	45	58,5
	22.	Clock at noon, by three ditto,	—	11	45	58—
1775, Feb.	10.	Clock at noon, by five ditto,	—	11	59	47+
	16.	Clock at noon, by five ditto,	—	11	59	54+
	18.	Clock at noon, by five ditto,	—	11	59	50+
	22.	Clock at noon, by ten ditto,	—	11	59	45+
	28.	Clock at noon, by four ditto,	—	11	58	58,5+

The altitudes are corrected by the equation for declination.

For

For the Longitude of Louvain.

1773, Auguſt 15, immerſion of ♃'s firſt ſatellite.

At 11h 28′ 31″ by the clock, ſelf.

11 28 54 ditto, my ſon,

		Apparent times. h
At Paris, M. MESSIER, $3\frac{1}{2}$ feet achrom. good,	—	11 41 5
At Geneva, M. MALLET, 10 feet achrom. fine ſky,	—	11 55 58
At Tyrnaw, F. WEISS,	— — — —	12 41 23
At Peterſburg,	— — — —	13 32 58
At Louvain,	— — — —	11 50 5

On a mean, Louvain Eaſt of Paris 9′ 7″+.

Auguſt 22, immerſion of ♃'s firſt ſatellite.

At 13h 18′ 52″ by the clock, hazy, ſelf.

13 19 24 ditto, by my ſon.

At Corbeil, M. MESSIER, 32 inches reflector, fine,	—	13 37 25
At Peterſburg,	— — — —	15 28 20
At Griefswalde,	— — — —	14 20 44
At Greenwich, ſix feet reflector,	— —	13 26 14
At Louvain,	— — — —	13 45 2

On a mean, Louvain Eaſt of Paris 9′ $4\frac{1}{2}$″.

For

For the Longitude of Louvain.

1773, August 24, immersion of ♃'s second satellite.

At 9h 56′ 27″ by the clock, self, good observation.

9 56 43 ditto, by my son.

		Apparent times. h ′ ″
At Corbeil, M. MESSIER, reflector as above, doubtful,	—	10 14 52
At Perinaldo, M. MARALDI, 3 feet achrom. fine	—	10 35 31
At Geneva, M. MALLET, 10 feet achrom.	— —	10 29 14
At Stockholm, M. WARGENTIN, 10 feet achrom. good	—	11 17 22
At Louvain,	— — — —	10 23 45

On a mean, Louvain East of Paris 9′ 25″½.

August 29, immersion of ♃'s first satellite.

At 15h 10′ 40″ by the clock, self.

15 11 0 ditto, by my son, good.

At Paris, M. MESSIER, achrom. as above, good,	—	15 32 41
At Geneva, M. MALLET, achrom. as above, fine,	—	15 47 40
At Perinaldo, M. MARALDI, achrom as above, fine,	—	15 53 49
At Muswel-hill Mr. DALBY,	— —	15 22 34
At Louvain,	— — — —	15 41 25

On a mean, Louvain East of Paris 8′ 51″.

For

For the Longitude of Louvain.

1773, Auguft 31, immerfion of ♃'s firft fatellite.

At 9h 39′ 1″ by the clock, felf, good, Moon-light.

9 38 50 ditto, my fon.

		Apparent times.		
		h	′	″
At Nolon, Cardinal DE LUYNES, achrom. 60 times,	—	10	4	45
At Tyrnaw, F. WEISS,	— — —	10	2	7
At Perinaldo, M. MARALDI, achrom. as before,	—	11	22	16
At Oxford, Mr. HORNSBY, achrom. 3½ feet, clear,	—	9	47	36
At Greenwich, 6 feet reflector,	— —	9	51	57
At Louvain,	— — — —	10	10	39

On a mean, Louvain Eaft of Paris 9′ 31″,2

August 31, immerfion of ♃'s fecond fatellite.

At 12h 30′ 49″ by the clock, felf, hazy and Moon-light.

At Perinaldo, M. MARALDI, achrom. doubtful	—	13	14	32
At Upfal,	— — — —	13	55	20
At Geneva, M. MALLET, achrom. as before, thin fog,		13	8	51
At Greenwich, fix feet reflector,	— —	12	45	1
At Louvain,	— — — —	13	2	31

On a mean, Louvain Eaft of Paris 8′ 42″.

For

For the Longitude of Louvain.

1774, July 24, immersion of ♃'s second satellite.

At 13h 5′ 0″ by the clock, self, doubtful.

13 4 30 by ditto, my son.

	Apparent times. h	′	″
At Paris, M. MESSIER, achrom. as before, a little doubtful,	12	53	25
At Geneva, M. TREMBLAY, 10 feet achrom. fine, —	13	7	55
At Tyrnaw, — — — —	13	54	21
At Greenwich, six feet reflector, — —	12	43	49
At Louvain, — — — —	13	2	42

On a mean, Louvain East of Paris 9′ 29″+.

July 26, immersion of ♃'s first satellite.

At 15h 21′ 7″ by the clock, self, good.

15 21 9 ditto, by my son.

At Geneva, M. MALLET, achrom. as before, fine, —	15	24	0
At Milan, telescope magnifying 200 times, — —	15	36	31
At Louvain, — — — —	15	18	15

On a mean, Louvain East of Paris 9′ 1″½.

For

For the Longitude of Louvain.

1774, Auguft 25, immerfion of ♃'s fecond fatellite.

At 12h 51′ 36″ by the clock, hazy, my fon.

	Apparent times. h ′ ″
At Perinaldo, M. MARALDI, achrom. as before, good	13 3 59
At Paris, M. MESSIER, — — —	12 42 40
At Geneva, M. MALLET, fine weather, — —	12 57 45
At Stockholm, — — — —	13 46 11
At Louvain, — — — —	12 51 55

On a mean, Louvain Eaft of Paris 9′ 2″.

Auguft 27, immerfion of ♃'s firft fatellite.

At 11h 54′ 45″ by the clock, my fon, good.

At Upfal, — — — —	12 46 32
At Milan, telefcope as before, — — —	12 13 7
At Louvain, — — — —	11 55 12

On a mean, Louvain Eaft of Paris 9′ 39″,5.

September 1, immerfion of ♃'s fecond fatellite.

At 15h 30′ 56″ by the clock, my fon, good.

At Geneva, M. MALLET, achrom. as before, fine —	15 37 27
At Tyrnaw, — — — —	16 23 5
At Louvain, — — — —	15 31 55

On a mean, Louvain Eaft of Paris 9′ 26″$\frac{1}{2}$.

For the Longitude of Louvain.

1774, October 1, immersion of ♃'s first satellite.

At 10h 21′ 46″ by the clock, my son, hazy.

	Apparent times. h ′ ″
At Greenwich, six feet reflector, — —	10 15 5
At Paris, M. MESSIER, reflector $2\frac{1}{2}$ feet, excellent, —	10 24 45
At Milan, telescope as before, — —	10 51 43
At Geneva, M. MALLET, achrom. 10 feet, fine, —	10 39 19
At Oxford, Mr. HORNSBY, $3\frac{1}{2}$ feet achrom. magnifying 100 times,	10 10 19
At Marseilles, telescope magnifying 100 times, —	10 36 22
At Tyrnaw, — — — —	11 25 9
At Stockholm, — — — —	11 26 59
At Petersburg, — — — —	12 16 5
At Louvain, — — — —	10 33 56

On a mean, Louvain East of Paris 9′ 36″

October 14, immersion of ♃'s second satellite.

At 7h 16′ 7″ by the clock, my son, good.

At Greenwich, six feet reflector, — —	7 11 23
At Paris, M. MESSIER, $3\frac{1}{2}$ feet achrom. good, —	7 20 42
At Tyrnaw, — — — —	8 21 13
At Upsal, — — — —	8 21 58
At Stockholm, — — — —	8 24 4
At Louvain, — — — —	7 29 53

On a mean, Louvain East of Paris 9′ 13″.

For

For the Longitude of Louvain.

1774, October 21, immerfion of ♃'s firft fatellite,

At 8h 39′ 27″ by the clock, my fon, Moon-light.

	Apparent times.		
	h	′	″
At Greenwich, fix feet reflector, — —	8	35	0
At Paris, M. MESSIER, achrom. as above, good, —	8	44	47
At Milan, — — — —	9	11	41
At Geneva, M. MALLET, achrom. as above, —	8	59	20
At Oxford, Mr. HORNSBY, 3½ feet achrom. thin fog, —	8	30	26
At Louvain, — — — —	8	53	29

On a mean, Louvain Eaft of Paris, 9″ 1′.

October 21, immerfion of ♃'s fecond fatellite.

At 9h 55′ 15″ by the clock, my fon, ☽ near ♃.

At Greenwich, fix feet reflector, — —	9	51	1
At Paris, M. MESSIER, achrom. 3½ feet, excellent, —	10	0	27
At Geneva, M. TREMBLAY, Moon-fhine, — —	10	14	43
At Milan, telefcope as before, — —	10	27	12
At Louvain, — — — —	10	9	17

On a mean, Louvain Eaft of Paris 9′ 13″.

For the Longitude of Louvain.

1775, February 15, emersion of ♃'s first satellite.

At 6h 12′ 29″ by the clock, self.
6 12 17 by ditto, my son, good.

		Apparent times.	
	h	′	″
At Greenwich, reflector, air clear, but twilight, reflector,	5	53	2
At Paris, M. MESSIER, achrom. as before, excellent, —	6	2	33
At Louvain, — — — —	6	12	22

On a mean, Louvain East of Paris 9′ 46″½.

February 19, emersion of ♃'s second satellite.

At 8h 36′ 46″ by the clock, self, good.
8 36 20 ditto, by my son.

At Greenwich, achrom. 3½ feet, very fine, —	8	17	18
At Tyrnaw, — — — —	9	27	37
At Louvain, — — — —	8	36	30

On a mean, Louvain East of Paris 9′ 52″.

February 22, emersion of ♃'s first satellite.

At 8h 8′ 33″ by the clock, good.
8 8 26 ditto, by my son.

At Lunden, — — — —	8	42	39
At Tyrnaw, — — — —	9	0	6
As Stockholm, — — — —	9	1	55
At Oxford, Mr. HORNSBY, achrom. emerged quick, —	7	44	32
At Greenwich, six feet reflector, — — —	7	49	37
At Louvain, — — — —	8	8	43

On a mean, Louvain East of Paris 9′ 39″

R E-

RESULT.

	′ ″		′ ″
By the immerſions	9 7 9 4½ 9 25½ 8 51 9 31 8 42 9 29 9 1½ 9 2 9 39½ 9 26½ 9 36 9 13 9 1 9 13	By the emerſions,	9 46½ 9 52 9 39
		The mean, —	9 46—
		By the immerſions,	9 13½
		By a mean of immerſions and emerſions,	9 30
The mean —	9 13½		

If the obſervations made at Louvain, excepting that of the ſecond ſatellite of Auguſt 31, 1773, in which I ſuſpect an error of a minute, be compared to thoſe made at Greenwich alone, with which there is a ſingular agreement, they ſtand as follows:

	′ ″		′ ″
Immerſions, —	9 52 9 46 9 57 9 55 9 34 9 33 9 20	Emerſions, —	9 44 9 56 9 30
		By a mean of emerſions,	9 43
		By ditto of immerſions,	9 42½
Mean —	9 42½		

If

If a mean be taken between 9′ 30″, the reſult of the whole of the compared obſervations as above, and 9′ 43″ the mean of the Louvain obſervations, compared with thoſe made at Greenwich only, the mean of theſe means will probably be very near the truth, and give the *Refuge de Vrowperg*, *Rue des Dominiquaines*, at Louvain, 9′ 37″ in time, or 2° 24′ 15″ of a great circle, Eaſt of the Royal Obſervatory at Paris.

Farther obſervations of Louvain.

1773, Auguſt 15.

At 3 h. P.M. in a SSE. room, out of the Sun, FAHRENHEIT'S thermometer ſtood at — — —	87°
At 4 h. P.M. in a North-eaſt room, — —	75
At 4 h. 30′ in a garden ſcreened from the Sun, — —	73

1774, October 5.

	Apparent times. h ′ ″
Immerſion of ♃'s third ſatellite, with a reflector 18 focal length, by SHORT, magnifying 130 times, by my ſon, —	9 57 8

1775, February 18.

Occultation of Saturn by the Moon.

Contact of Saturn's Weſt and ☽'s Eaſt limb, —	9	25	15
Total immerſion of Saturn's body, — —	9	25	39
Total immerſion of the ring, — — —	9	26	5

By

By my ſon, with the ſame reflector as above, magnifying 95 times on Saturn's emerging from a cloud, he appeared in contact with the limb of the Moon at 9h 25′ 15″. The ſuddenneſs of this obſervation may poſſibly make it uncertain for a few ſeconds. At the total immerſion of Saturn's body, the Eaſtern part of the ring became very faint by the brightneſs of the Moon. Vapours affected pretty ſtrongly both Saturn and the Moon. Clouds hindered from ſeeing the emerſion.

March 1.

	Apparent times. h ′ ″
Emerſion of ♃'s firſt ſatellite, very ſlow, ſelf, —	10 5 59

I have no correſponding obſervation.

For the Longitude of Bruſſels.

1773, Nov. 1, emerſion of ♃'s firſt ſatellite.

At 11h 23′ 23″ by the clock, ſelf, good.

11 23 32 ditto, by my ſon, good.

	Apparent times. h ′ ″
At Greenwich, ſix feet reflector, — —	12 2 10
At Paris, M. MESSIER, 3½ feet achrom. good, —	11 11 49
At Perinaldo, M. MARALDI, achrom. three feet fine, —	11 33 24
At Muſwel-hill, Mr. DALBY, — — —	11 1 16
At Bruſſels, by my obſervation, — —	11 19 34

On a mean, Bruſſels Eaſt of Paris 7′ 57″.

Nov. 10, emerſion of ♃'s firſt ſatellite.

At 7h 51′ 20″ by the clock, ſelf.

7 51 15 ditto, my ſon, good.

At Greenwich, by tables corrected, Phil. Tranſ. 1777, p. 183.	7 26 12
At Bruſſels, — — — —	7 43 35

On a mean, Bruſſels Eaſt of Paris 8′ 7″.

December 10, emerſion of ♃'s firſt ſatellite.

At 10h 14′ 14″ by the clock, my ſon.

At Greenwich, ſix feet reflector, — —	9 27 59
At Muſwel-hill, Mr. DALBY, — —	9 27 32
At Bruſſels, — — — —	9 45 34

On a mean, Bruſſels Eaſt of Paris 8′ 7″½.

For the Longitude at Bruſſels.

1775, December 19, emerſion of ♃'s firſt ſatellite.

At 6h 42′ 48″, by the clock, my ſon.

	Apparent times.		
	h	′	″
At Tyrnaw, F. WEISS, — — —	6	59	15
At Bruſſels, — — — —	6	6	26

On a mean, Bruſſels Eaſt of Paris 8′ 6″.

1774, January 11, emerſion of ♃'s firſt ſatellite.

At 7h 8′ 7″ by the clock, my ſon.

At Paris, M. MESSIER, 3½ feet achrom. good, —	6	4	24
At Stockholm, M. WARGENTIN, good, — —	7	7	2
At Bruſſels, — — — —	6	11	57

On a mean, Bruſſels Eaſt of Paris 7′ 39″.

February 3, emerſion of ♃'s firſt ſatellite.

At 7h 34′ 10″ by the clock, my ſon.

At Paris, M. MESSIER, achrom. 3½ feet, good, —	6	15	33
At Stockholm, — — — —	7	18	10
At Upſal, — — — — —	7	16	17
At Bruſſels, — — — —	6	23	40

On a mean, Bruſſels Eaſt of Paris 8′ 20″.

For the Longitude of Brussels.

1774, February 18, emersion of ♃'s second satellite.

At 7h 24′ 58″ by the clock, my son.

	Apparent times.		
	h	′	″
At Tyrnaw, F. WEISS, — — —	7	1	25
At Stockholm, M. WARGENTIN, — — —	7	3	36
At Brussels, — — — —	6	8	44

On a mean, Brussels East of Paris 8′ 6″.

R E S U L T.

The mean of these emersions give Brussels East of Paris 8′ in time; but if the observation of January 11, 1774, be rejected, as it ought, because differing considerably from the rest, the mean of the others will give the Court at Brussels 8′ 7″ in time, or 2° 1′ 45″ in parts of a great circle, East of the Royal Observatory at Paris.

Farther

Farther obſervations at Bruſſels.

1773, October 27.

At $3\frac{1}{2}$ h. P.M. height of quickſilver in FAHRENHEIT's thermometer in a SW. room, out of the Sun, — —	70°
At 7 h. P.M. in a NE. room, — — —	$65\frac{1}{2}$
At $9\frac{1}{2}$ h. P.M. in the ſame room, — — —	$65\frac{1}{2}$
At 11 h. P.M. in the ſame room, — — —	65

A little wind from the weſt; cloudy; I was obliged to open the windows till 9 h. P.M.

November 1.

Occultation of Aldebaran by the Moon, ſelf.

	Apparent times. h ′ ″
The ſtar ſeemed to touch the Moon's limb, — —	9 33 3
The ſtar, which ſeemed on the Moon's diſk, vaniſhed, —	9 33 11
Occultation, by my ſon, — — —	9 33 $12\frac{1}{2}$

Aldebaran entered $\frac{1}{6}$th nearly of the Moon's diameter, South of the ſpot Grimaldus. The night was very fine at the occultation, but clouds hindered ſeeing the emerſion.

November 12.

The quickſilver in the barometer, in a room one ſtory high, at 7 h. P.M. ſtood at 28,645 Engliſh inches; the preceding night was very ſtormy.

1774, February 18.

	Apparent times. h ′ ″
Immerſion of a teleſcopic ſtar into the dark part of the Moon; inſtantaneous, by my ſon, — — —	9 17 21

This ſtar is ſet down in FLAMSTEAD *Atlas Cœleſtis* as follows:

61° 57′ R. A.

15 20 Declination North.

April 14.

Emerſion of Aldebaran out of the enlightened part of the Moon, oppoſite Mare Criſium; inſtantaneous; weather very fine; ſelf,	7 39 8

XXXI. *Obſervations on the Scurvy.* *By* Charles De Mertans, *M. D.* *Dated* Vienna, Jan. 14, 1778.

Read May 7, 1778.

THE diſeaſes of a great multitude of people, who live in the ſame manner, and are obliged to live upon unwholeſome food, are to be corrected by a correction of the food itſelf, and not by any medicines properly ſo called.

Conſiſtently with this principle, I have always thought that the ſalt proviſions uſed by ſea-faring people being the principal cauſe of the ſcurvy which makes ſuch fatal havoc

Obſervations ſur la Scorbut. *Par M.* Charles de Mertans, *Docteur en Medecine.*

LORSQU'IL s'agit de la conſervation de la ſanté d'une multitude de gens vivans tous de la même maniere, et obligés de ſe nourrir principalement d'alimens qui l'altérent; c'eſt dans la correction de cette nourriture, et non dans les remédes donnés comme médecines, qu'il faut chercher les moyens de les préſerver des maux, auxquels l'experience démontre qu'ils ſont les plus ſujets.

D'après ce principe j'a toûjours cru que les proviſions ſalées, dont uſent les gens de mer, étant la principale cauſe du ſcorbut qui attaque les équipages dans les voyages de longue durée, et prive ſouvent les vaiſſeaux des bras néceſſaires pour

havoc amongft crews engaged on long navigations, it was neceffary to find out fome food of an oppofite nature to this, capable likewife of being preferved at fea.

Salt provifions are hard of digeftion; and we all know that all food, which our powers of digeftion cannot reduce to a good chyle, undergoes in the *primæ viæ* fuch alterations as are proper to the refpective fpecies of it in regard to heat and humidity; confequently, the chyle produced by falt provifions partakes altogether of an animal nature tending to putrefaction. When it mixes with the blood, it increafes this difpofition which our fluids have of themfelves; and thus, by degrees, introduces that flow putrid degeneration which we call fcurvy, of which, I am perfuaded, there is but one fort, different in its degrees. I am likewife perfuaded, that the fea and land fcurvy are the fame diforder, arifing from fimilar

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pour les conduire, il falloit chercher à y oppofer des alimens d'une nature contraire et qui fe confervaffent fur mer.

Les viandes falées font de difficile digeftion, et nous favons que les alimens que nos forces digeftives ne peuvent pas réduire en bon chyle, fubiffent dans les premieres voyes les altérations propres à leur efpece dans la chaleur et l'humidité; par conféquent le chyle produit de viandes falées feules, tient entierement de la nature animale tendante à la putrefaction. Lorfqu'il fe mêle au fang, il augmente cette difpofition, que nos fluides ont déja par eux mêmes, et par là peu à peu introduit cette dégénération putride lente, que nous appellons fcorbut, dont je fuis perfuadé qu'il n'y a qu'une feule efpece, qui a differens dégrés. Je fuis auffi convaincu que le fcorbut de mer et celui de terre font la même maladie, produite

fimilar caufes, that is, living upon falt meat or fifh, few or no vegetables, damp houfes, &c.

To prevent then or correct this alteration in the humours, we muft find out fome antifeptic aliments, which may keep a great while, and not be fubject to be damaged from the alteration of climate. Now, I ufed to think, that four-krout, or fermented cabbage, fo frequently ufed in Germany, had thefe qualties; that though it did not always pleafe thofe who eat it for the firft time, every body foon grew ufed to it, and found it good and wholefome food; that failors in particular were very fond of it, efpecially when they had no other greens. I had accordingly feveral converfations upon the fubject, twelve years ago, with Meffieurs PRESTON and LANGLEY, in

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produite par des caufes femblables : nourriture de viande ou poiffons falés, peu ou point de végétaux, habitations humides, &c.

Il s'agit donc, pour prévenir ou corriger cette altération des humeurs, de procurer des alimens d'une qualité antifeptique, qui puiffent fe conferver long tems, et que les changemens de climats ne gàtent point. J'ai cru que le fauer-kraut, ou choux fermentés, dont on fait fi grand ufage en Allemagne, avoit ces qualités; que fi elle ne plaifoit pas toujours a ceux qui en mangent pour la premiere fois, tout le monde s'y accoutumoit bientôt, et la trouvoit un mets bon et nourriffant; que les marins en feroient leurs délices, furtout lorfqu'ils manqueroient d'autres légumes. J'eus il y a une douxaine d'années plufieurs converfations à ce fujet avec Meff. LANGLOIS et PRESTON, attachés ici a l'ambaffade de mylord STORMONT, qui m'honnoroient de leur amitié. Je défirois qu'on fit des effais de tranfporter.

in which I expreſſed my wiſhes that ſour-krout might be carried out and made part of the ſhips proviſion.

For ſome years paſt I have ſeen, with great pleaſure, in the public papers, and the relations of travellers, that the trials I wiſhed for have been crowned with ſucceſs; and that the preſervation of the healths of many crews, which have gone round the world, has been owing to ſour-krout. The preſervation of ſea-faring people is an object ſo important to many nations, and whoever contributes towards it does ſo eſſential ſervice to mankind, that I will now communicate other methods, which, joined to the firſt, will ſerve to keep off the ſcurvy, as well as to cure it more readily and more ſurely. Theſe methods are likewiſe in the food, and they conſiſt of vegetables eaten in a ſtate of crudity, and ſuch as the earth affords

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tranſporter la ſauer-kraut ſur mer, pour en faire une partie de la nourriture des équipages.

Depuis quelques années je lis avec une vraye ſatisfaction dans les papiers publics et les relations des voyageurs que ces eſſais ont parfaitement réuſſi, et que c'eſt en grande partie a la ſauer-kraut que l'on doit la ſanté de pluſieurs équipages de vaiſſeaux qui ont fait le tour du monde. La conſervation des gens de mer eſt un article des plus importans pour pluſieurs nations, et en y travaillant on rend ſervice a une grande partie du genre humain. Dans cette vue je vais communiquer de nouveaux moyens, qui, joints aux premiers, ſerviront à préſerver du ſcorbut, a en arreter les progrès, et même a le guérir plus promptement et plus ſurement. Je les trouve de même dans la nourriture, ce ſont des végétaux de différentes eſpeces mangés dans l'état de crudité, et tels que la terre les donne.

Je

affords them. I am convinced, that all the greens uſed in our kitchens are much more antiſcorbutic when they are raw than after they have been boiled in water[(a)], or have gone through any other preparation by fire. I ground my opinion upon experience, the ſafeſt of all guides, and ſhall therefore begin with the facts which led me to it.

I was ſurprized to find, during an abode of many years at Moſcow, that many gentlemen merchants and ſtrangers were attacked by a ſlow ſcurvy, having their gums ſoft, ſwollen, and blueiſh, the breath ſtrong, and many ſcorbutic ſpots at the legs, whilſt it was rare to find among the lower people, either of town or country, a ſingle perſon with theſe marks. The nouriſhment of the

(a) Perhaps it is becauſe they loſe a great deal of fixed air by ebullition.

Je ſuis aſſuré que tous les légumes dont on ſe ſert dans nos cuiſines, ſont infiniment plus antiſcorbutiques lorſqu'ils ſont cruds, que quand ils ont été bouillis (a) dans de l'eau, ou qu'ils ont paſſé par toute autre préparation au feu. Je me fonde ſur le guide le plus ſur, l'expérience; c'eſt pourquoi je commencerai par rapporter des faits.

Pendant un ſéjour de pluſieurs années que je fis a Moſcou, je fus ſurpris de trouver beaucoup de gentils hommes, de marchands et d'étrangers, atteints d'un ſcorbut lent, ayant les gencives molles, gonflées, et bleuatres, l'haleine puante, et pluſieurs des taches ſcorbutiques aux jambes; tandis que parmi le peuple tant de la ville que de la campagne il eſt tres rare de trouver un ſeul homme qui ait la moindre de ces marques. La nourriture des premiers conſiſte ſurtout

(a) Peut être parce qu'ils per dent par l'ebullitione beaucoup de leur air fixe.

the former conſiſts of a great deal of meat, both ſalt and freſh, and likewiſe of fiſh; they ſeldom eat any greens, except now and then a ſoup made of ſour cabbage, exactly reſembling the German ſour-krout in every thing, ſave that this cabbage is chopped ſmall, whereas the ſour-krout is cut according to the length of the cabbage. Their common drink is very ſour ſmall beer, called *quas*, beſides which they drink wine, the beer of the country, Engliſh beer, and a ſmall glaſs of brandy at leaſt before every meal. They eat very little bread. The common people live all the year upon this ſour cabbage ſoup, in which they boil ſalt meat on common days, and ſalt or dried fiſh on meager days and during their four lents (which are more than a third of the year) when they add to it very ſtinking lin-ſeed oil inſtead of greaſe

ſurtout en beaucoup de viande, tant fraiche que ſalée, et de poiſſons de méme: ils mangent peu ou point de légumes, excepté de tems en tems d'une ſoupe aux choux aigres, qui reſſemblent entierement a la ſauer-kraut d'Allemagne, ſinon qu'ils ſont hachés menus, au lieu que cellecy eſt coupée ſoivant la largeur des choux. Leur boiſſon ordinaire eſt une petite bierre fort aigre qu'ils nomment *quas*; ils boivent en outre du vin, de la bierre du païs, de la bierre d'Angleterre, et un petit verre d'eau de vie au moins avant chaque repas; ils mangent fort peu de pain. Le peuple vit toute l'année de cette ſoupe aux choux aigres, dans laquelle on cuit de la viande ſalée les jours gras, et du poiſſon ſalé ou ſéché les jours maigres et pendant leurs quatre carémes, qui ſont plus d'un tiers de l'année, àlors ils y ajoutent de l'huile de lin fort puante au lieu de graiſſe ou de beurre.

greaſe or butter. In this ſoup, which is called *ſchſti*, both in the meager and other ſeaſons, they boil meal, principally that of Saracen wheat. They eat cucumbers like the others in ſummer, and ſalt them for the winter. They likewiſe feed very much upon oat bread. The common people live in ſmall wooden houſes, generally very low, in which they get together both night and day during three parts of the year, on account of the great cold. There is little air in the room, the windows of which are very ſmall. Here they ſtew together in humidity and naſtineſs; for, except the bath, which, as well as thoſe I have mentioned firſt, they uſe once a week, they are extremely naſty.

Here then are many reaſons, all of which (except the conſtant uſe of ſour cabbage and bread) ſhould make them

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Dans cette ſoupe, qu'ils appellent *ſchſti*, en maigre et en gras, ils font cuire des gruaux, ſurtout celui de bled Sarraſin. Ils mangent, de même que les premiers, en été des concombres, et les font ſaler pour l'hiver. Ils ſe nouriſſent auſſi beaucoup de pain de ſeigle. Les gens du peuple habitent de petites maiſons de bois, ordinairement baſſes, où il ſe raſſemblent nuit et jour en grand nombre les trois quarts de l'année, a cauſe du grand froid; il y a peu d'air dans les chambres, les fenêtres en ſont fort petites: ils y croupiſſent généralement dans la malpropeté et l'humidité. A l'exception du bain dont ils ſe ſervent, de même que la plus part de ceux que j'ai nommés en premier lieu, une fois la ſemaine au moins, ils ſont fort malpropres.

Voila bien des raiſons, excepté le plus grand uſage de choux aigres et de pain, qui

them more ſubject to the ſcurvy than the people of faſhion, or thoſe who live at their eaſe; a conſtant uſe of meat or fiſh that is ſalt (for they eſteem neither ſo much when they are freſh) much more brandy, filth and damp in their houſes, leſs change of cloaths or linen.

I was many years making theſe obſervations, and inquiring what it was that could preſerve them from the ſcurvy, which, on ſo many accounts, they ought to have been more ſubject to than the others. It appeared to me that, excluſive of the daily uſe of the ſour cabbage, which I conſider as the moſt powerful of all preſervatives, they were indebted for their ſafety to the great quantity of raw greens, ſuch as onions, leeks, raddiſhes, turnips, peas in the pod, and others, which they eat. The berries of Vaccinium, with others much reſembling them, called *kloukna*,

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qui devroient rendre ceux cy plus ſujets au ſcorbut que les nobles et les gens aiſés: preſque toûjours de la viande ou du poiſſon ſalé (ils ne font même pas tant de cas de l'une et de l'autre quand ils ſont frais) beaucoup plus d'eau de vie, la mal propreté et l'humidité de leurs maiſons, le changement plus rare de linge et d'habits.

Je fus quelques années à faire ces obſervations et a chercher ce qui pouvoit principalement les préſerver du ſcorbut, dont par tant de raiſons ils auroient du être attaqués préférablement aux autres: il me parut qu' outre l'uſage journalier des choux aigres que je regarde comme le plus puiſſant préſervatif du ſcorbut, ils en étoient redevables à ce qu'ils mangent quantité de légumes cruds; oignons, porreaux, radis, raves, navets, pois avec leurs gouſſes. Les bayes du Vaccinium,

kloukna, which are of the ſize of a ſmall cherry and very acid, are, together with apples, ſtrawberries, and raſberries, almoſt the only fruits of theſe countries.

In the Foundling Hoſpital, of which I was a phyſician, there were every winter ſeveral ſcorbutic patients. This hoſpital was built near the conflux of two rivers, in a place the ſoil of which has been raiſed at a great expence. As near back as the year 1770 there were ſtill ſtagnated waters to be ſeen in the place; but only a part of the children lived there, the remainder lived in a ſtone houſe, ſituated upon an eminence in the neighbourhood.

The uſual ſymptoms of the ſcurvy on theſe children were, the ſwelling of the gums, the nauſeous breath, a great languor and dejection; they uſed to become cachetic,

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nium, et d'autres preſque ſemblables qu'ils appellent *kloukna*, de la groſſeur d'une petite ceriſe et fort acides, font avec les pommes, les fraiſes et les framboiſes, preſque les ſeuls fruits de ces contrées.

J'avois chaque année en hiver et au printems, dans la maiſon des enfans trouvés, dont j'étois medecin, beaucoup de ſcorbutiques. On a bâti cette maiſon pres du confluent de deux rivieres, dans un lieu dont on a relevé le terrein a grands fraix. Juſqu'en 1770 on voyoit encore parci par là de l'eau croupiſſante dans cet endroit; mais il n'y avoit qu'une partie des enfans qui y demeuroit, les autres occupant une vielle maiſon de pierre ſituée ſur une éminence dans le voiſiuage.

Les ſymptoms ordinaires de ſcorbut chés ces enfans étoient le gonflement des gencives, la peſanteur de la bouche, une grande laſſitude et abattement; ils devenoient

tic, and of a leaden colour. In proceſs of time the ſwelling of the gums increaſed; they were uſed to aſſume a livid colour: puſtules were formed in the mouth, the infection of the breath grew moſt horrible, the gums and all the inſide of the mouth became gangrenous, the jaw bones were carious, the fall of the teeth followed, and the bones of the *alveoli* fell to pieces. The ſick could ſcarce ſtir, though they had as yet no fever, and had a very good appetite. The legs of ſome amongſt them were from the firſt covered with ſcorbutic ſpots, and dried cruſts, like ſcales; others only had theſe ſymptoms after the miſchief had made a great progreſs. Moſt of them had their legs ſwelled. In ſome, the flexor tendons of the legs grew ſhorter, and ſtiffened in ſuch a manner that they were forced to keep always in a lying poſture, with

venoient chachectiques, et d'une couleur plombée. Peu a peu le gouflement des gencives augmentoit, elles prenoient une couleur livide; il ſe formoit des puſtules a la bouche l'haleine repandoit une infection horrible; les gencives et tout le dedans de la bouche ſe gangrenoit; les os des mâchoiſes ſe carioient, la chute des dents ſuivoit, et les os des alvéolés tombaient par morceaux. Les malades pouvoient a peine ſe remver, quoique toûjours ſans ſiévre; l'apétit ne leur manquoit pas. Il y en avoit dont les jambes dès le commencement étoient couvertes de taches ſcorbutiques, et de croutes ſéches comme des écailles; a d'autres elles ne venoient que lorſque le mal étoit fort avancé, la plus part avoient les jambées enflées. Chez quelques uns les tendons fléchiſſeurs des jambes ſe raccourciſſoient, et ſe roidiſſoient de façon qu'ils étoient obligés de reſter continuellement couchés, ayant les pieds près des cuiſſes; j'ai vu une couple

with the legs near the thighs. In two cafes the fame thing has happend to the arms.

The gangrene of the gums and mouth, as well as the caries of the bones, ufed infenfibly to increafe to fuch a degree, that the bones of the *alveoli* and the fpongious part of thofe of the upper jaw ufed to fall out. The mifchief was ufed, however, to make a flow progrefs; there often elapfed a fortnight, and fometimes more, after the beginning of the gangrene of the mouth and caries of the bones; and many months between the firft fymptoms and the ftage of the diforder I have been defcribing. Even in this ftage, dreadful as it was, they ftill took nourifhment fufficient, and even much more than it would be thought poffible they fhould have taken in fuch a fituation. It was impoffible, however, they fhould live long in fuch a ftate, and death foon put an end to their

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couple de fois arriver la même chofe aux bras.

La gangréne des gencives, de la bouche et la carie des os augmentoient infenfiblement, au point que les os des alvéoles et la partie fpongieufe de ceux de la mâchoire fuperieure tomboient. Ce mal alloit lentement; il fe paffoit quelques fois quinze jours, et même plus, depuis le commencement de la gangrene de la bouche et de la carie des os, et plufieurs mois depuis l'apparition des premiers fymptomes de la maladie jufqu'au point que je viens de décrire. Malgré cela ils prenoient encore dans ce dernier période de la nourriture en quantité fuffifante, et infiniment plus qu'on n'auroit pu fe l'imaginer d'après leur état. Ils ne pouvoient cependant vivre longtems dans cette fituation, et la mort venoit enfin

their torments. I have often been furprized at not hearing any cries of anguifh come from them in fo lamentable a fituation; but they were ufed almoft continually to complain of their voice being feeble.

The mode of treatment which I commonly made ufe of in curing the greater part, provided the mifchief had not made a confiderable progrefs in the fpongious bones of the upper jaw, was this: the firft thing I did was to put them on a vegetable diet, and order them foups, with a great many greens dreffed in light broth, fuch as four cabbage, carrots, turnips, and onions, &c. to which I added ftewed onions and forrel. The drink of the bigger fort was quas or four fmall beer; the leffer ones (none of which were ever feized with the fcurvy under two years old) drank water.

During

enfin les délivrer de tant de maux. J'ai été fouvent étonné de ne leur entendre pouffer aucun cri de douleur dans un état aufli déplorable; mais ils fe plaignoient prefque continuellement d'une voix languiffante.

Le traitement que j'employois ordinairement guériffoit la plus part, pourvu que le mal n'eût pas fait des progrès dans les os fpongieux de la mâchoire fupérieure. D'abord je les mettois entièrement a la nourriture végétale, leur faifant donner des foupes avec beaucoup de légumes cuits dans un bouillon léger; comme choux aigres, carottes, panais, navets, oignons, &c. des épinards, de jeunes orties, de l'ofelles, étuvés: la boiffon des plus grands étoit le quas ou petite bierre aigrelette, les petits *(b)* buvoient de l'eau.

(b) Il n'en ai jamais vu au deffous de deux ans attaqués du fcorbut.

Au

During the ſpring, thoſe who had the ſcurvy took, in proportions ſuitable to their ages, a drink made of whey, in which were infuſed antiſcorbutic plants, ſuch as *cochlearia*, *naſturtium aquaticum*, *becca bunga*, *acetoſa*. This infuſion was ſweetened with plain ſyrup, or ſyrup of ſugar. Beſides this, in the courſe of the day, they uſed a gargle, made of an infuſion of herbs, rue, ſage, agrimonia in water, to which was added ſpirit of *cochlearia*, and honey of roſes. When the gangrene began to ſhew itſelf at the mouth, beſides the remedies I have mentioned, they uſed to take a ſtrong decoction of bark, part of which decoction I likewiſe added to the gargle. I likewiſe had the gangrened parts touched with honey of roſes, mixed with a ſmall quantity of ſpirit of ſea ſalt.

This

Au printems tous les ſcorbutiques prenoient chaque matin une certaine quantité, ſuivant leur age, de petit lait où l'on avoit infuſé des plantes antiſcorbutiques, comme *cochlearia*, *naſturtium aquaticum*, *becca bunga*, *acetoſa*; cette infuſion étoit édulcorée avec un ſirop ſimple ou du ſucre. En outre ils ſe ſervoient ſouvent pendant la journée d'un gargariſme fait d'une infuſion d'herbes de rhue, de ſauge, d'*agrimonia* dans de l'eau, à laquelle on ajoutoit de l'eſprit de *cochlearia*, et du miel roſat. Lorſque la gangréne ſe manifeſtoit à la bouche, outre les remédes que je viens de rapporter, ils prenoient une forte décoction de kinkina, j'ajoutois auſſi de cette décoction au gargariſme. Je faiſois toucher les parties gangrénées avec du miel roſat, au quel on avoit mêlé un peu d'eſprit de ſel marin.

This method of treatment had ſucceeded perfectly well the three firſt years; inſomuch, that the greater part of the ſick, as well adults as infants, were commonly cured in the ſpace of three weeks or a month, when the diſtemper was not far advanced. It was in ſpring and winter that the ſcurvy uſed to be moſt fatal.

In autumn 1770, the foundling children, who remained in town to the number of a thouſand[(c)], were lodged, contrary to my advice, in the wing of the houſe finiſhed but about a year ſince. In a climate where the ſummer is ſo ſhort, new walls made of bricks take a great time in drying, and this houſe was ſituated on a ſoil which had been a bog a few years before. Notwithſtanding all the poſſible precautions that could be taken, a

(c) The greater part of the ſucking children were at nurſe in the country.

damp

Ce traitment m'avoit réuſſi les trois premieres années, de ſorte que preſque tous ces malades, tant adultes qu' enfans, gueriſſoient ordinairement dans l'eſpece de trois ſemaines ou un mois lorſque le mal n'étoit pas fort avancé. C'étoit en hiver et au printems que le ſcorbut faiſoit le plus de ravages.

En automne 1770 on logea, contre mon avis, tous les enfans trouvés, qui étoient en ville au dela de mille (c), dans l'aile de la maiſon achevée depuis un an. Dans un climat où l'été eſt ſi court, les murailles neuves, faites de briques, ſéchent difficilement, et cette maiſon étoit ſituée dans un terrein qui avoit été un marais quelques années auparavant. On ſentit pendant tout l'hiver, malgre

(c) La plus parts des petits à la mamelle étoient en nourrice à la campagne.

tout

damp was felt in the room the whole winter. The ſcurvy ſhewed itſelf early in the ſpring, and I had many more children ill than I had had the preceding ſeaſons. The violent ſymptoms were likewiſe much more frequent. Many had gangrenous puſtules at the mouth, the jaw bones were carious in ſome; the limbs, particularly the legs of many, were drawn up and ſtiff.

I put all theſe ſick perſons in the wooden houſe, which had already ſerved many years as an hoſpital for the ſcurvy, and gave them the food and medicines above-mentioned; but the diſorder was more ſtubborn than ever it had been, and all I could do could hardly keep it down. In the middle of May, ſeeing that the remedies I had formerly tried were unſucceſsful, I began to think of other methods. The reflections communicated above, which

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tout ce qu'on put faire pour l'éviter, de l'humidité dans les chambres. Le ſcorbut commenca a ſe manifeſter de bonne heure, et j'eus beaucoup plus d'enfans ſcorbutiques que les années précédentes; les ſymptomes violents étoient auſſi plus frequents. Pluſieurs eurent des puſtules gangréneuſes dans la bouche; quelqueſuns les os des mâchoires cariés; d'autres les membres, ſurtout les jambes, retirés et roides.

Je mis tous ces malades dans la maiſon de bois, qui avoit déja ſervi pluſieurs années d'hopital aux ſcorbutiques; je leur fis donner la nourriture et les rémedes dont j'ai fait mention. Le mal étoit plus opiniatre, et tout ce que je pus faire ſervoit a peine a en rallentir les progrès. Vers le mois de May, voyant que les moyens employés les années précédentes, ne ſuffiſoient pas pour guérir cette maladie,

which I had made upon the diet of the lower people, determined me to give my patients thoſe vegetables raw which they had before been uſed to eat boiled. In conſequence thereof, I ordered them, every morning, radiſhes, ſweet turnips, carrots, and young onions, which they eat like apples. At dinner, beſides the ſoup and greens as uſual, they eat ſallad with a little vinegar and a very little oil; in the afternoon the ſame roots as in the morning, and at night, greens and ſallad. The remedies were continued as before. In a few days all the bad ſymptoms decreaſed: thoſe who were at the worſt, and had been ill for ſome time, began to grow better; thoſe who had been but ſlightly ſeized were ſoon well, ſo that at about a month's end there only remained a few of thoſe who had been the worſt, and they too were getting well

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ladie, qui étoit plus enracinée, je penſai à differens autres remédes. Les reflexions que j'ai communiquées cy deſſus au ſujet de la diéte du bas peuple, me determinerent à donner cruds a mes petits malades les vegetaux, qu'ils mangeoient cuits. Je leur fis donc donner chaque jours le matin des raves, des navets doux, des carottes, de jeunes oignons; ils les mangeoient comme des pommes: à diner outre la ſoupe et les légumes comme à l'ordinaire, ils avoient de la ſalade avec un peu de vinaigre et fort peu d'huile: l'aprés midi les mémes racines que le matin, et le ſoir un légume et de la ſalade. On continuoit les mêmes rémedes qu'ävant. Aubout de quelques jours tous les ſymptomes diminuerent; ceux qui étoient le plus fortement attaqués, et languiſſoient depuis pluſieurs mois, ſe trouverent mieux et commencerent a guérir; les moins malades ſe remirent en fort peu de tems, de ſorte qu'au bout d'un mois il ne me reſtoit plus dans

well at a great pace. This change for the better was apparent in all a very few days after they had begun to eat the raw greens. I had not at that time read the obſervations of the Engliſh phyſicians and ſurgeons on malt, or I ſhould certainly have made uſe of it. *Quas*, which I have mentioned above as the principal drink of the common people, comes near it, with this difference only, that it is not drunk in a ſtate of fermentation. It is a ſpecies of ſour ſmall beer, to which, inſtead of hops, they add the wild mint.

The ſame method of treatment was attended with ſucceſs in 1772 and 1773; in both which ſprings I had ſcorbutic patients with the ſame ſymptoms, but not in ſuch numbers as in 1771 (when there were near ſixty) becauſe the houſe, having now dried, was become very wholeſome,

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dans cette partie de l'hopital que quelqueſuns de ceux qui avoient été le plus mal, et qui ſe trouvoient alors en parfaite convaleſcence. Ce changement en mieux fut viſible chés tous dès qu'ils eurent mangé les légumes creds pendant quelques jours. Je n'avois pas encor alors lu les obſervations faites par les médecins et chirurgiens Anglois ſur la dreche, ſans quoi je n'aurois pas manqué d'en faire uſage. Le quas, dont j'ai parlé plus haut, qui fait la principale boiſſon du peuple Ruſſe, en approche, excepté qu'on ne leboit pas dans l'état de fermentation: c'eſt une petite bierre aigre, a laquelle, au lieu de houblon, on ajoute de l'herbe de menthe ſauvage.

Le même traitement me réuſſit au printems de 1772 et 1773, ou j'eus comme toutes les autres années des ſcorbutiques avec les mêmes ſymptomes, quoique pas en ſi grand nombre qu'en 1771 où il y en avoit pres de ſoixante, parce que la maiſon

wholefome, and becaufe the foil had been again confiderably raifed.

I fhall not propofe carrying out vegetables on a voyage for the whole crew, becaufe that, in order to preferve them, they muft be kept in dry fand, which (if not altother impracticable) would be extremely difficult in fuch large quantities, not to add that even then a great part would be fpoilt: but might it not be poffible to provide a certain quantity of carrots, turnips, &c. and ftow them in fand, in a part of the fhip where they might not be expofed to get damp or wet, whence they might be given in fuch cafes as the four-krout alone would be found infufficient to cure? for I am apt to think that thefe greens, joined to an infufion of malt, would foon get the better of the diforders.

But

maifon ayant féché entierement devint fort faine, et que l'emplacement fut encore rehauffé de beaucoup.

Je ne propoferai pas d'embarquer fur les vaiffeaux des vegetaux frais pour tout l'équipage, parce que pour les conferver il faut les tenir dans du fable fec, ce qui feroit, fi non impoffible, du moins tres difficile pour une fi grande quantité; et que même malgre ces précautions plufieurs fe gateroient. Mais ne pourroit on pas en fe fervant de la fauer-kraut pour la confervation de la fanté des marins, mettre auffi une certaine quantité, autant que la grandeur des bâtimens et les autres circonftances le permettroient, de radis, de carottes, de navets, et d'oignons frais dans du fable bien fec a l'endroit du navire ou l'eau et l'humidité ne pourroient pas pénétrer, afin de donner quelquefuns de ces légumes a ceux, qui malgré l'ufage de la fauer-kraut prendoient le fcorbut? Je crois que ces légumes et l'infufion de la dreche les rétabliroient bientôt.

Si

But if this cannot be ſo well done at ſea, it is obvious, that the cure of the ſcorbutic perſons will be much accelerated, if raw greens are given them as ſoon as they come on ſhore; a mode which will have the additional advantage of ſhortening the ſtations ſhips are obliged to make, for the recruiting their ſick. Nature will of herſelf diſpoſe the ſick to make uſe of this remedy, eſpecially as I have obſerved that the ſtomach is never affected by it.

In Auſtria, as well as ſeveral other parts of Germany, the people eat ſour turnips, which are prepared in the ſame manner as the ſour-krout; that is, after having been chopped thin, ſalt is put to them, and they are left to ferment. They are put in tubs, and keep from one year to another. I propoſe this vegetable as a valuable addition

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Si cela n'eſt pas pratiquable quant aux vaiſſeaux en mer, il eſt aiſé d'en conclure, que, lorſqu'on met les ſcorbutiques à terre, on accelerera leur guériſon en leur donnant a manger cruds les légumes qu'on trouvera; ce qui, outre l'avantage de guerir cette maladie plus ſurement, abrégera les ſtations que les navires ſont ſouvent obligés de tenir pour remettre leurs ſcorbutiques. Il ne ſera pas difficile de perſuader à ces malades de manger les légumes cruds; la nature, notre meilleur guide en tout ce qui concerne notre conſervation, les y porte, et j'ai obſervé que leur eſtomac ne s'en trouve pas affecté.

En Autriche, et dans pluſieurs autres parties de l'Allemagne, le peuple mange des navets aigres; on les prépare de la même façon que la ſauer-kraut; les ayant hachés menus on y met du ſel, et on les laiſſe fermenter. Ils ſe conſervent tout l'hiver, et même d'une année à l'autre, dans des tonneaux. Ce légume eſt

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tion to the antiſcorbutic regimen of ſea-faring people. It has nearly the ſame taſte as ſour-krout, and will, I believe, be found to have the ſame virtues: and if ſo, though it ſhould have no other advantage, it will at leaſt vary the diet, which is itſelf no inconſiderable advantage on a long voyage.

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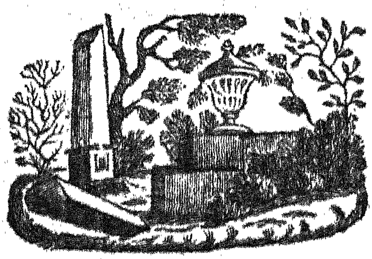
eſt une addition que je propoſe à la diéte antiſcorbutique des gens de mer; il a preſque le même gout que la ſauer-kraut, et je crois qu'il aura les mêmes vertus. Si cela eſt, comme je le penſe, ce ſera au moins pour changer de tems en tems de mets, ce qui n'eſt pas un petit avantage dans un voyage de longue durée.



XXXII. *Comparison between Sir* George Shuckburgh *and Colonel* Roy's *Rules for the Measurement of Heights with the Barometer; in a Letter to Col.* Roy, *F. R. S. from Sir* George Shuckburgh, *Bart. F. R. S.*

TO COLONEL ROY.

Welbeck Street, April 20, 1778.

SIR,

Read May 7, 1778.

SINCE the printing of your ingenious memoir on the subject of measuring heights with the barometer, I have been naturally led to a comparison of your rules and observations with my own(a); and herein am not more pleased than surprized at the general correspondency of our results, which carries with it the appearance of one and the same series of experiments, rather than of distinct observations made with different instruments, in different countries, and by different persons. That the standard temperature or zero on the scale of the thermometer should be found by each of us to fall in the same point to within one-third of a degree is, I think, truly surprizing; and I doubt not will evince to Mr. DE LUC the strong probability there is of

(a) Vide Phil. Transf. vol. LXVII.

the neceſſity of correcting his rules. But although in this eſſential and fundamental part of the inquiry we agree, there are, nevertheleſs, ſome little circumſtances wherein we differ; it is the ſubject of this letter, SIR, to point out to you the degree of our differences, a compariſon that I had the pleaſure ſlightly to exhibit to you a few days ago, and which I truſt will not be found unimportant to thoſe who may be engaged in theſe purſuits: if, therefore, you judge theſe remarks of ſufficient moment, I will beg the favour of you to lay them before the Royal Society, as the beſt means of communicating them to the public.

The two chief cauſes of our difference are, the expanſion of quickſilver and the expanſion of air. I ſhall begin with the equation for quickſilver.

The mean temperature of ordinary barometrical obſervations, I apprehend, will generally be found to lie between 40° and 70° on FAHRENHEIT'S thermometer; now the mean expanſion in this range, according to your obſervation, is ,0323 inch on a column of 30 inches for 10° of heat; by my table it is only ,0304 inch, the difference ,0019 inch is equal to about 20 inches in the reſult of the height, when the temperature of the two barometers differs 10°, and this may reaſonably be expected only in a height of 3000 or 4000 feet. In an obſer-

observation on Mount Ætna, one of the greatest accessible heights in Europe, the difference of temperature at the top and bottom might amount to 30°, and this would occasion a difference of about five feet, which, I apprehend, may be reckoned inconsiderable in a height of 11,000 feet. In fact, in an observation on this mountain by Mr. DESSAUSSURE it amounted to only $3\frac{1}{2}$ feet. I may add, that your equation makes the computed height less than mine.

I proceed to the expansion of air. Your equation is various according to the circumstances, the difference therefore of our results will, according to the circumstances, be various. The following table will give the quantity of this difference, *viz.* it shews how much your result is + or − mine upon one thousand feet, according to different pressures of the atmosphere and different temperatures. The first column to the left hand contains the mean heat of the column of air between the two barometers; the figures in the horizontal line at top are the mean height of the two barometers, or mean pressure of the atmosphere; the common point of meeting in the different columns gives the difference of our result in feet, according to the respective circumstances.

Mean heat.	Mean height of the two barometers in inches.										
o	30	29	28	27	26	25	24	23	22	21	20
32	o	o	o	o	o	o	o	o	o	o	o
42	+1	o	−1	−2	−3	−4	−5	−6	−7	−8	−9
52	+2	o	−2	−4	−6	−8	−10	−12	−14	−16	−17
62	+5	+2	−1	−4	−7	−10	−13	−16	−19	−22	−25
72	+8	+4	o	−4	−10	−12	−17	−21	−25	−29	−33

Thus, if the mean height of the barometer were 27 inches, and the temperature 52°, the difference of the refults would be four feet in a thoufand; how far, therefore, this is of moment is left to the judgement of the obferver. I conclude laftly, SIR, with a comparifon of your actual obfervations made in Great Britain computed after my tables, deduced from a feries of obfervations made in Savoy. I have indeed only collected fixteen of your obfervations; but as I have chofen fuch as prefented themfelves as moft proper, either on account of their heights or temperatures, I imagine thefe will be very fatisfactory.

Place

Place of observation.	True height in feet geometrically.	By my tables, Computed height.	Error in feet.	Error on 1000 feet.	By Col. Roy's table Computed height.	Error in feet.	Error on 1000 feet.	Mean temp. of the obs.
Station at Weem and top of Bolfrack's Cairn,	1076.5	1074.2	− 2.3	− 2.1	1075.5	− 1.0	− 0.9	58½
Ditto station and top of Dull-craig,	1244.5	1240.6	− 3.9	− 3.1	1244.4	− 0.1	− 0.0	56
Ditto station and South obs. on Schehallien,	2098.0	2096.0	− 2.0	− 0.9	2091.3	− 6.7	− 3.2	52½
Ditto station and West summit of Schehallien,	3281.0	3292.5	+ 11.5	+ 3.5	3279.0	− 2.	− 0.6	50½
	——	3261.3	− 19.7	− 6.1	3252.5	− 29.5	− 8.9	46⅛
Carmichael well and top of Tinto,	1642.5	1653.6	+ 11.1	+ 6.7	1646.9	+ 4.4	+ 2.7	54½
	——	1645.0	+ 3.5	+ 2.1	1642.7	+ 0.2	+ 0.1	48
Level of Hawk-hill and small rock on Arthur's Seat,	702.4	704.2	+ 1.8	+ 2.5	703.	+ 1.3	+ 1.9	20½
Base of Hawkhill Observatory and bottom of the small rock,	684.0	684.1	+ 0.1	+ 0.1	686.7	+ 2.7	+ 3.9	17
	——	685.4	+ 1.4	+ 2.0	687.0	+ 3.0	+ 4.4	68½
Hawkhill garden-door and bottom of the rock on Arthur's Seat,	730.8	723.0	− 7.8	− 10.7	721.8	− 9.0	− 11.3	29½
	——	745.8	+ 15.0	+ 20.6	748.4	+ 17.6	+ 24.2	71
Linhouse and East Cairn hill,	1176.6	1182.5	+ 5.9	+ 5.0	1180.0	+ 3.4	+ 2.9	0½
Carnarvon Quay and Moel Eilio,	2371.	2397.4	+ 26.4	+ 11.2	2393.3	+ 22.3	+ 9.4	62⅜
Carnarvon Quay and Peak of Snowdon,	3555.	3563.1	+ 8.1	+ 2.3	3551.3	− 3.7	− 1.0	56¼
	——	3551.5	− 3.5	− 1.0	3548.4	− 6.6	− 1.9	53
			Mean error	+ 2.0		Mean error	+ 1.4	

Thus

Thus it ſeems, that the error of my tables, from a mean of all theſe obſervations, amounts to $+\frac{20}{10000}$; of yours, to $+\frac{14}{10000}$: but it muſt be remarked, that the ſtandard temperature, from whence I compute, is $31°,24$ of FAHRENHEIT, whereas in your computations it is aſſumed at $32°,0$; this difference of $0°,76$ is equal to $\frac{18}{10000}$ in the correction for the expanſion of the air: if then we were to ſet out from the ſame zero, *viz.* 32° (which I have propoſed, ſee p. 569. of my memoir, Phil. Tranſ. vol. LXVII.) the error of my tables, according to your obſervations, would become only $\frac{2}{10000}$, that of yours remaining $\frac{14}{10000}$. I would by no means from hence conclude, that any preference is to be given to the former, but would ſay, that in moſt practical obſervations, in theſe countries at leaſt, it is indifferent which method is uſed. Theſe ſame compariſons alſo afford us another piece of information, *viz.* that under ſimilar conditions the denſity of the atmoſphere is the ſame, whether under the parallel of 46 or 56 degrees of latitude. Till, therefore, more accurate obſervations than thoſe of Mr. BOUGUER [(a)] can be obtained in the neighbourhood of the

(*a*) Mr. BOUGUER's obſervations I hold inadequate in ſuch an inquiry, not only on account of their incorrectneſs (for they are related only to the ¼ of a French line), but particularly inconcluſive, allow them all the preciſion you pleaſe, as they were not ſynchronous; inſomuch, that we are uncertain whether thoſe obſervations, which ſhould be correſponding in point of time, were made within ſix months of each other: and with regard to the temperature of ſome of them

the equator, I ſhould be extremely cautious how I admitted a latitudinal equation; nor do I think the ſingle obſervation, related in *The Voyage towards the North Pole*, of ſufficient authority itſelf to eſtabliſh ſuch a theory upon (b).

them we are ſtill more at a loſs, having a range of no leſs than 38° to aſſume it in (vide Voyage au Perou, p. 29.). The mean, however, of his five obſervations, according to my computation, would bring the zero of the ſcale to about 40° of FAHRENHEIT: but till it can be proved, that in this uncertainty of 38° we have, in fixing upon the mean, got the true temperature to within 8°; and alſo, that during many months the barometer in the ſame place had never altered a ſingle line: till then, I ſay, no fair concluſion, in point of theory, can be drawn from theſe experiments, for with ſuch ſuppoſed errors or variations the Peruvian obſervations will agree exactly with thoſe in this climate.

(b) It ſeems extraordinary, that the heights of the quickſilver, obſerved on the ſea-ſhore with one of Mr. RAMSDEN's barometers, ſhould differ a quarter of an inch from the height of the ſame obſerved the ſame day, and almoſt at the ſame hour, with Mr. NAIRNE's marine barometer on board a ſhip (vide p. 135 and 148. of *The Voyage towards the North Pole)*. This difference, therefore, I think remains to be explained, if the experiment is to be made uſe of in this inquiry; for this ſame marine barometer, compared a few days ago at Mr. IBBETSON's, ſecretary to the board of longitude, with one of my own, that I uſed in Savoy, agreed with it to within 0,04 inch. It may alſo be remarked, that Lord MULGRAVE's obſervation in lat. 79° 44′ brings the zero to about 64°; ſo that between the lat. 56° and 79° (equal 23°) the zero of the ſcale moves through a ſpace of no leſs than 32°; whereas, between the lat. 46° and 56° it is perfectly ſtationary, at leaſt as to ſenſe, having altered only one-third of a degree: which great want of proportion, I think, is of itſelf ſome argument againſt the exiſtence of ſuch a latitudinal equation. Moreover, Mr. BOUGUER's obſervations brings the zero of the ſcale on the ſame ſide of 32° with Lord MULGRAVE's, *viz.* to about 40°, ſo that, if any deduction is to be drawn from theſe obſervations, it is, that the air, both at the equator and at the pole is heavier than in middle latitudes, which is no very probable conjecture, and, I apprehend, more than is intended to be proved. I ſhould have aſked pardon for the freedom with which I have diſcuſſed this inquiry, were I not aſſured that the acquaintance and friendſhip of my Lord MULGRAVE, which I have experienced for ſome years, will lead him to attribute it to its proper motive.

I ſhall

I ſhall now beg leave to conclude with what I flatter myſelf will not here appear improper, a new rule for reducing the obſervations, and which I hope will be found particularly commodious, as it requires no logarithms, nor any other than the following ſhort table, which may be engraven upon the ſcale of a thermometer, and therefore, always accompanying the inſtrument, will ſerve for computing the obſervations upon the ſpot, (if the height ſhould not exceed 4000 or 5000 feet) which, I apprehend, will frequently be found very ſatisfactory.

Ther.	Feet.	The value of one-tenth of an inch of quickſilver on the barometer, expreſſed in feet in the atmoſphere when the barometer ſtands at 30 inches, according to the different temperatures.
32°	85.86	
35	87.49	
40	88.54	
45	89.60	
50	90.66	
55	91.72	
60	92.77	
65	93.82	
70	94.88	
75	95 93	
80	96.99	

The adjoined table gives the value of $\frac{1}{10}$th of an inch on the barometer in feet in the atmoſphere, when the quickſilver ſtands at 30 inches, for every five degrees of temperature from 32° to 80°; and for any other height of the barometer it will be in the inverſe ratio of that height to 30 inches. Thus, let A be the mean height of the two barometers in inches; α the difference of the two barometers in 10ths of an inch; β the number taken out of the adjoined table; x the height in feet; we have then the following expreſſion, $\frac{30\alpha\beta}{A}=x$, the height required.

I have the honour to be, &c.

XXXIII. *An Account of the Calculations made from the Survey and Meafures taken at* Schehallien, *in order to afcertain the mean Denfity of the Earth. By* Charles Hutton, *Efq. F. R. S.*

Read May 21, 1778.

THE furvey from which thefe calculations have been made was taken at and about the hill Schehallien in Perthfhire, in the years 1774, 1775, and 1776, by the direction, and partly under the infpection, of the Rev. NEVIL MASKELYNE, D. D. F. R. S. and Aftronomer Royal, by whom the manner of making the furvey has already been fully explained in the Philofophical Tranfactions for 1775.

I have therefore only to give an account of the meafures of the lines and angles, and of the calculations which I have raifed from them with all poffible care and faithfulnefs, for the purpofe of determining the meafure of the ratio of the mean denfity of the earth to that of water or any other known matter.

Thefe calculations were naturally and unavoidably long and tedious; and the more fo as the bufinefs was in a manner quite new, which laid me under the neceffity

 of

of inventing and deſcribing ſuch modes of computation as ſhould be proper to be applied in ſo important and delicate a buſineſs. Having, at length, with cloſe and unwearied application for a conſiderable time compleated all the calculations; I have, in the following ſheets, drawn up an account of thoſe operations, with the reſults ariſing from them; and have accompanied them with ſuch drawings as are neceſſary to illuſtrate the deſcriptions. I have alſo inſerted a ſynopſis of the meaſures which were taken of the lines and angles, from which any perſon may at any time ſatisfy himſelf of the truth of the computations that have been made and are herein deſcribed. Theſe meaſures I have here immediately ſubjoined, before I proceed to deſcribe the computations made from them.

A ſynopſis of the horizontal and vertical angles that were obſerved at the principal points in making the ſurvey about Schehallien.

In the firſt column are contained the names of the horizontal angles, the meaſure of which, in degrees and minutes, are in the ſecond column, and the vertical angles are in the third column, in which it is to be obſerved, that the letter denoting the object is placed before the degrees and minutes, and E or D after them, to ſhew that they are in elevation or depreſſion reſpectively. The mark * placed to the meaſure of any angle denotes that it is the mean of the two obſervations made with the inſtrument turned different ways; namely, after the firſt obſervation, reverſing it to make the ſecond. Alſo the mean height of the theodolite is put down to each ſtation. In the vertical angles, the bottom of the object is underſtood, unleſs where the top is mentioned below, and ſometimes the height of the pole is added in feet and inches.

At A	Theodolite = 4 ft. 10 in.	
	° ′	° ′
DAB	31 15	
DAN	77 30	N 12 22E
DAO	102 36½	O 0 58E
DAR	134 31½	
NAC	83 13½	N 12 20E
NAO	25 4	
OAS	27 24	S 5 25D
OAR	31 55	R 6 31D
At B		
DBN	81 8	
DBO	101 41	
DBA	139 59	
At C	Theodolite = 4 ft. 7½ in.	
ACD	126 6	
ACF	93 34	
ACG	92 15	
ACH	85 31½	
ACN	49 10	N 10 4E
ACB	8 11	
At D	Theodolite = 4 ft. 8½ in.	
ADC	48 7	
ADB	8 45	
ADN	49 14	N 10 40E
ADW	83 56½	N 10 39E
ADH	88 56	A 1 15E
ADG	95 5	G 3 17D
ADF	96 3	F 3 34D
At F	Theodolite = 4 ft. 8½ in.	
	° ′	° ′
GFW	17 27	W 0 58 D
GFK	76 7¼	K 9 50 E
GFN	103 57½	N 17 42½E
GFD	172 54	D 3 37 E
HFG	10 9	G 1 36 D
HFP	60 41	P 5 9 E
HFK	65 58	K 9 41 E
HFN	93 49	N 17 35½E
At G	Theodolite = 4 ft. 6½ in.	
DGF	6 8	
NGD	59 41	
NGF	65 49	
NGH	101 9	
HGF	166 57	
HGD	160 49	
HGX	71 11½	
HGW	4 32	
PGF	95 56	F 1 15 E
PGN	30 6½	N 16 54 E top of Cairn.
PGK	0 9¼	K 10 15 E
PGL	62 57	L 0 32 D
PGW	63 20	W 0 54 D
PGH	71 2	H 3 0 D
		P 5 46 E top of ball.

At

At H	Theodolite = 4 ft. 5½ in.	
	° ′	° ′
GHF	2 54	G 2 59 E
GHD	13 2	
NHG	49 44	N 14 42 E
NHK	29 17	
NHW	107 43	N 14 39 E top
NHW	107 41	
KHD	65 59	K 12 6 E top
KHG	79 1	
KHW	78 25	W 3 2 E
WHP	96 14	P 7 39 E top
WH*p*	149 2	*p* 0 22 E
WHG	157 25½	G 2 55 E
At W	Theodolite = 4 ft. 5½ in.	
LWK	107 27½	L 0 25½ E
LWP	130 40	P 5 43 E top
LWP	130 41	
LWN	133 48½	N 12 36½ E
LWF	175 2	K 11 30 E
LWQ	4 41	
LWG	178 21	
LWH	193 13	H 3 14 D
At L	Theodolite = 4 ft. 9 in.	
GLP	37 1	P 4 24½ E
GLY	139 32½ *	Y 2 35½ D
WLG	1 18	W 0 * 40½ D
	1 17¾ *	G 0 * 31 E
WLP	38 12	P 4 * 22½ E
	38 12½ *	

	° ′	° ′
WLN	38 58	N 10 * 56½ E
	38 58½ *	
WLK	60 33	K 10 * 24½ E
	60 34 *	
WLT	115 38 *	T 0 * 30 D
WLV	126 58½ *	V 0 * 35½ D
WLY	140 50½ *	Y 2 * 35 D
WLF	3 58½ *	F 0 * 38½ E
WLE	62 12 *	E 10 * 16½ E
WL*t*	38 4½ *	*t* 4 * 34½ E
At Y	Theodolite = 4 ft. 8 in.	
TYK	69 23¼ *	K 10 * 29 E
TYN	76 56½ *	N 9 * 37¼ E
TYL	124 47 *	L 2 * 35 E
TYG	103 24¾ *	G 1 * 36½ E
TYZ	19 21¾ *	Z 3 * 3½ E
TYV	13 29½ *	V 3 * 26½ E
VYT	13 28 *	T 2 * 4½ E
VYZ	32 52 *	Z 3 * 3½ E
VYE	81 49½ *	E 10 * 33 E
VYK	82 53 *	K 10 * 29 E
VYN	90 24 *	N 9 * 36½ E
VYG	116 52 *	G 1 * 35¼ E
VYH	126 57 *	H 1 * 0 E
VYW	130 33 *	W 2 * 2 E
VYL	138 14 *	L 2 * 33½ E

At V	Theodolite = 4 ft. 11 in.	
TVZ	17° 25¼′ *	Z 2° * 28½′ E
	17 25 *	2 * 26
TVU	34 24½ *	U 2 * 47½ E
TVE	70 43 *	E 9 * 10 E
TVK	71 23½ *	K 9 * 5½ E
	71 24 *	9 * 5½
TVW	112 32½ *	W 0 * 25½ E
TVL	119 57 *	L 0 * 34 E
TVY	147 51½ *	Y 3 * 31½ D
	147 50 *	3 * 28½ D
		T 0 6 E

At T	Theodolite = 4 ft. 10 in.	
YTV	18 41½ *	V 0 * 8¾ D
YTL	30 1 *	L 0 * 27¼ E
YTU	116 18¾ *	U 3 * 31 E
YTZ	133 31½ *	Z 3 * 49½ E
		Y 2 10 D
VTY	18 41 *	Y 2 * 6 D
VTL	48 43 *	L 0 * 30 E
VTE	94 2 *	E 9 * 42 E
VTU	135 1½ *	U 3 * 31½ E
VTZ	152 14½ *	Z 3 * 50 E
		V 0 7 D

At Z	Theodolite = 4 ft. 6½ in.	
UZR	10 53½ *	R 0 * 50 D
UZA	16 5 *	A 1 * 58¾ E
UZX	16 18 *	X 1 * 52 D
UZO	33 18¾ *	O 3 * 58 E
UZY	117° 21′ *	Y 3° * 8¼′ D
UZV	134 4½ *	V 2 * 31½ D
UZT	144 27½ *	T 3 * 56¼ D
		U 2 * 45¼ E

At U	Theodolite = 4 ft. 10 in.	
ZUT	18 20¾ *	T 3 * 32¼ D
ZUV	28 54½ *	V 2 * 51 D
ZUO	124 42 *	O 4 * 1½ E
ZUN	125 9½ *	N 10 * 13½ E
ZUX	128 2¾ *	X 10 * 55½ D
ZUA	157 55 *	A 1 * 36¾ E
ZUR	160 37½ *	R 3 * 38 D
		Z 2 * 51 D

At X	Theodolite = 4 ft. 7 in.	
OXA	40 59½ *	A 4 * 15 E
OXS	53 20 *	S 0 * 44¼ E
OXZ	139 22½ *	U 10 * 49¼ E
OXU	174 59 *	Z 1 * 48¼ E
		O 11 * 13½ E

At S	Theodolite = 4 ft. 9 in.	
RSA	18 27	A 5 24 E
RSA	18 28	A 5 20½ E
RSN	72 51	N 18 40 E
RSO	90 58	O 13 19½ E
RSX	171 1	X 0 55½ D
		R 0 24½ E

At R the E. end of the S. baſe.	Theodolite = 4 ft. 6 in.	
	° ′	° ′
KRN	41 6½	
B″RA	20 36¼ *	A 6 * 27½ E
B″RN	84 32¾ *	N 19 * 17½ E
B″RO	111 46¼ *	O 11 * 57¼ E
B″Rα′	100 14½ *	α′ 20 * 2 E
B″Rβ′	121 27¼ *	β′ 15 * 11¼ E
B″RS	177 39	
B″RZ	186 53½ *	Z 0 * 46 E
its ſup.	173 6½ *	O 12 29 E

At B″ the W. end of the S. baſe.	Theodolite = 4 ft. 10 in.	
RB″S	0 55½	R 0 37 D
RB″β′	36 8¼ *	β′ 10 * 24 E
RB″O	37 27 *	O 7 * 35¼ E
RB″α′	50 49½ *	α′ 15 * 48 E
RB″N	65 46¾ *	N 17 * 33 E
RB″A	139 2¼ *	A 11 * 22¼ E

At O the S. obſervatory.	Theodolite = 4 ft. 7 in.	
XOS	35 39 *	S 13 * 25½ D
XOM	42 12¾ *	M 13 * 12½ D
XOR	58 49¼ *	R 12 * 4 D
XOB″	89 35 *	B″ 7 * 39½ D
XOA	115 43½ *	A 0 * 59 D
XOU	1 39 *	U 4 * 4½ D
	° ′	° ′
XOZ	23 38½ *	Z 4 * 0 D
		X 11 * 18 D
MOR	16 36½ *	R 12 * 3½ D
MOB″	47 23 *	B″ 7 * 39½ D
MOA	73 32½ *	A 1 * 0½ D
MOB	81 22 *	B 1 * 1½ D top
MON	134 39	M 13 16
NOK	95 1	
MOα′	156 34 *	α′ 33 * 47½ E top
MOβ′	125 45 *	β′ 21 * 29½ E
MOZ	65 51½ *	Z 4 * 0 D
MOU	43 51½ *	U 4 * 3½ D
MOX	42 13½ *	X 11 * 19 D
MOS	6 34½ *	S 13 * 22½ D

M is in the meridian paſſing through O, and is a little South of the interſection of that meridian and the line RS.

At P the N. obſervatory.	Theodolite = 4 ft. 10 in.	
NPK	77 18¾	
GPK	179 35¼ *	G 5 39¾ D
GPH	47 45¼ *	H 7 * 32½ D
GPW	69 0¾ *	W 5 * 37 D
GPL	80 9¼ *	L 4 * 27¾ D
GPF′	20 54 *	F′ 5 * 6 D
GP*m*	38 18¼ *	*m* 12 * 20 D
γ′Pδ′	45 39	} by reduction
γ′PK	2 55¾	}

γ′ from P bears 30° 41′¾ E. of South.
δ′ from P bears 14 57 W. of South.
m is Mr. MASON's mark.

At

At N the Western cairn.	Theodolite = 4 ft. 11 in.	
	° ′	° ′
KNO	40 5¼	by reduction
KNR	74 48⅗	by reduction
KNP	50 47⅘	
KNδ'	9 37½	
KNγ'	2 11⅗	
KNA	133 53 *	A 12 * 22½ D
KNB	146 15½ *	B 11 * 17 D top
KNC	178 30½ *	C 10 * 4 D
KND	172 51 *	D 10 * 39 D top
KNn	144 37½ *	n 4 * 30½ D
KNG	98 23 *	G 16 * 55 D top Pole 4 ft. 4 in.
KNH	69 13⅓	H 14 38 D top Pole 6 ft. 5 in.
KNm	64 27 *	m 18 * 55 D
KNW	56 22 *	W 12 * 35 D top Pole 7 ft.
KNL	49 7 *	L 10 * 55 D top Pole 6 ft. 8 in.
KNY	18 50 *	Y 9 * 37½ D top
KNγ'	2 15 *	γ' 7 * 30 D
KNE	1 15½ *	E 6 * 38½ D
KNδ'	9 37½ *	δ' 6 * 25½ D
KNU	41 4½ *	U 10 * 12½ D top
KNS	60 44 *	S 18 * 41 D
KNR	74 48½ *	R 19 * 20 D
KNB$''$	104 30 *	B$''$ 17 * 35½ D
		K 6 * 43½ D top

At K the Eastern cairn.	Theodolite = 4 ft. 8 in.	
	° ′	° ′
NKO	44 51¾	by reduction
NKR	64 5⅓	by reduction
NKP	51 54⅓	N 6 * 41 E
NKn	0 45 *	n 6 * 36 E top
NKF	43 32½ *	F 9 * 46 D top Pole 4 ft. 2 in.
NKG	51 40 *	G 10 * 16 D top Pole 4 ft. 4 in.
		G 10 * 14½ D
NKα	55 12	α 10 3 D
NKH	81 28	H 12 7 D top Pole 6 ft. 5 in.
NKH	81 27½ *	H 12 * 5½ D
NKW	97 18 *	W 11 * 28 D top Pole 7 ft.
NKL	109 19¾ *	L 10 * 25 D top Pole 6 ft. 8 in.
NKL	109 18½ *	L 10 * 23½ D
NKY	153 40 *	Y 10 * 30 D top
NKV	174 20 *	V 9 * 5½ D top
NKE	156 18	E 3 34 D top
NKα'	11 36 *	α' 6 * 31¾ E
NKM$'$	7 8 *	M$'$ 4 * 51¾ E
NKδ'	6 2⅗ *	δ' 6 * 53 E
nKα	54 26⅕ *	α 10 3¼ D
nKα	54 25½ *	α 10 * 3½ D
nKβ	39 16½ *	β 8 * 58½ D
α'Kβ'	37 39	α' 6 32 E

At α the E. end of the N. base.	Theodolite = 4 ft. 4 in. ° ′	° ′
γαK	106 0 *	K 10* 1½ E
γαG	101 3 *	G 9*37½ E
γαN	89 41 *	N 13*45½ E
γαn	89 25 *	n 13*49½ E
γαF′	47 38 *	F′ 8* 6½ E
		β 0 19 D
		γ 0 0 top Pole 3 ft. 2 in.

At β between α and γ the ends of the N. base.	Theodolite = 4 ft. 8 in.	
αβk	174 51	k 0 41½ E
αβt′	147 37½	t′ 1*25½ E
αβF′	71 49 *	F′ 10*22¾ E
αβD	108 1¼ *	D 9*17½ E
αβn	70 58½ *	n 13* 5¾ E
γβD	71 57¾ *	D 9*17 7/10 E
γβF′	108 9½ *	F′ 10*22¾ E
		α 0* 6 D

At γ the Western end of the North base.	Theodolite = 4 ft. 8½ in.	
αγF′	50 6¼ *	F′ 8*23½ E
αγn	63 53½ *	n 12*27¼ E
αγD	97 5¾ *	D 9*41 E
		α 0* 2 D top Pole 3 ft. 2 in.

At n the new W. cairn.	Theodolite = 4 ft. 9 in. ° ′	° ′
Knα	108 59½ *	K 6*40½ D
Knβ	128 35¼ *	β 13* 7½ D
KnF′	128 55½ *	F′ 14*10½ D
Knγ	135 40½ *	γ 12*29 D
KnD	173 55	D 10*45 D
KnN	38 28½ *	N 1 42 D
KnB″	102 54¼ *	B″17 31 D
DnA	53 49 *	A 12*21½ D
DnG	75 24 *	G 17* 3½ D
DnP″	78 44¼	P″20* 2½ D P″ is a pole in a line with P and K
DnH	104 35	H 14*42½ D
DnL	124 34	L 10 56 D
DnK	173 55	α 13*51⅜ D
NnG	132 59	α 13*54 D Nn was = 93½ feet by the tape measure.

At E the new E. cairn.	Theodolite = 4 ft. 9 in.	
NEA	10 5½ *	A 6*22½ E
NEM′	4 58½ *	M′ 4*45½ E
NED	4 51½ *	D 6*41½ E
NEK	22 14½ *	K 2*11½ E
NEH	78 40½ *	H 11*47½ D top Pole 6 ft. 5 in.
NEW	94 17 *	W 11*16½ D top Pole 7 ft.
NEL	106 21½ *	L 10*17 D top Pole 6 ft. 8 in.

	° ′	° ′
NEY	151 16 *	Y 10 * 37 D
NEV	172 20 *	V 9 * 15 D
NET	172 22½ *	T 9 * 41½ D top
NEZ	144 46 *	Z 9 * 18½ D
NEU	114 20½ *	U 10 * 4½ D
*a′*E*α′*	68 35½ *	*α′* 6 * 21 E
*a′*E*♉′*	73 50½ *	*♉′* 6 * 37½ E
*a′*EN	78 39½ *	N 6 * 33 E
*a′*E*b′*	97 17 *	*b′* 9 * 5½ D
		a′ 8 * 27 D

At M′ the meridian mark on the top of the hill South of P.	Theodolite = 3 ft. 10½ in.	
KM′E	3 28½ *	E 4 * 56½ D
KM′*γ′*	6 37½ *	*γ′* 6 * 3½ D top
KM′L	53 27½ *	L 10 * 19 D top Pole 6 ft. 8 in.
KM′W	63 12½ *	W 11 * 41 D top Pole 7 ft.
KM′H	78 4½ *	H 12 * 56½ D top Pole 6 ft. 5 in.
KM′*m*	86 33½ *	*m* 22 * 12 D
KM′*p*	87 54 *	*p* 22 * 10 D
KM′G	108 49 *	G 12 * 33 D top Pole 4 ft. 4 in.
KM′F	118 3½ *	F 12 * 25 D top Pole 4 ft. 2 in.
KM′*♉′*	176 1½ *	*♉′* 12 * 8 E
		K 5 5 D

At *t* the center of the transit instrument near the N. observ.	Theodolite = 4 ft. 8½ in.	
	° ′	° ′
M″*t*P′	32 24 *	P′ 9 * 18½ D
M″*t*G	32 57½ *	G 5 * 58½ D
M″*tm*	5 14 *	*m* 12 * 32½ D
M″*t*H	14 28 *	H 7 * 44½ D
M″*t*W	35 23½ *	W 5 * 47½ D
M″*t*L	46 22 *	L 4 * 35½ D
mtp	68 13 *	*p* 12 * 50½ E
*mt*M′	68 15 *	M′ 22 * 5 E
*mt*G	144 17 *	G 5 * 57½ D
*mt*H	96 52 *	H 7 * 43½ D
*mt*W	75 56 *	W 5 * 47½ D
*mt*L	64 58 *	L 4 * 37½ D
*mt*P	54 4½ *	P 17 * 42½ D top
		m 4 * 3 E

M″ bears 1′ 8″ W. of North.
M′ bears South.
P′ in a line with K and P.
p, a pole immediately above or South of the transit instrument.

At *p*	Theodolite = 4 ft. 5 in.	
G*p*M′	147 5 *	M′ 22 * 8½ E
G*p♉′*	133 6½ *	*♉′* 24 * 18 E
G*p*F	26 43	F 5 26 D
G*p*P′	0 34½ *	P′ 9 * 23½ D
G*p*M″	32 47½ *	M″ 13 * 25½ D
G*pt*	32 48	*t* 19 17 D
G*pm*	38 0½ *	*m* 12 * 34½ D
G*p*H	47 11 *	H 7 * 49½ D

	° ′	° ′
G*p*W	68 3½ *	W 5 * 51½ D
G*p*P	76 55½ *	P 13 * 49½ D
G*p*L	79 0½ *	L 4 * 39½ D
G*p*m′	121 24 *	m′ 4 * 50¼ D
G*p*E	179 37 *	E 17 * 39½ E
m′ is a pole a little above or South of P.		
At *m′*		
G*m′*M′	145 4½ *	M′ 22 * 8½ E
G*m′*δ′	131 11 *	δ′ 24 * 12½ E
G*m′p*	57 53½ *	*p* 2 * 47 D
G*m′t*	34 43	*t* 10 15 D
G*m′*F	13 1 *	F 5 * 22½ D
G*m′*P′	0 29½ *	P′ 19 * 19 D
G*m′*M″	32 29½ *	M″ 13 * 23¼ D
G*m′*P	37 6 *	P 18 * 16½ D
G*m′m*	37 49½ *	*m* 12 * 36½ D
G*m′*H	47 10 *	H 7 * 49 D
G*m′*W	68 11 *	W 5 * 49½ D
G*m′*L	79 17½ *	L 4 * 40½ D
G*m′*E	177 53 *	E 17 * 54 E
At *m*		
P*m*K	15 57	K 15 0 E
P*ma*	37 11	P 12 27 E
At α′ the South-west pole.	Theodolite = 4 ft. 11 in.	
Kα′N	149 3 *	K 6 * 43½ D
Kα′B′	131 3 *	B′ 15 * 51¼ D
Kα′R	102 7¼ *	R 20 * 9 D

	° ′	° ′
Kα′O	73 39 *	O 33 * 52 D
Kα′β′	28 4¼ *	β′ 15 * 56½ D top
Kα′γ′	4 53⅝ *	γ′ 7 * 35⅛ top
Kα′δ′	65 24 *	δ′ 3 41½ top Pole 17 ft. 4 in.
Eα′d′	13 48 *	d′ 12 * 54½ D
Eα′a′	8 36½ *	a′ 8 * 3½ D
Eα′K	2 49½ *	E 6 30 D
At β′ the South-east pole.	Theodolite = 4 ft. 9 in.	
α′β′O	56 47 *	O 21 * 45½ D
α′β′B′	62 21½ *	B′ 10 * 28½ D
α′β′R	84 46 *	R 15 * 17 D
α′β′M	96 41 *	M 17 * 3½ D
α′β′K	114 8½ *	α′ 15 * 42½ E
At γ′ the North-east pole.	Theodolite = 4 ft. 4 in.	
Nγ′P	56 55¾ *	P 18 * 3⅝ D
Nγ′*t*	55 22½	*t* 17 49½ D
Nγ′*n*	0 50⅜ *	*n* 7 15 E
Nγ′δ′	8 17¼ *	δ′ 7 * 59¼ E
Nγ′α′	14 17 *	α′ 7 * 31 E
Nγ′K	151 54½ *	K 1 * 34½ E
		N 7 * 23½ E

At δ' the North-west pole	Theodolite = 4 ft. 4 in.	
	° '	° '
Kδ'M'	2 53 *	M' 12 * 36$\frac{1}{2}$ D
Kδ'γ'	4 29 *	γ' 8 * 8$\frac{1}{2}$ D
Kδ'*m*'	73 20$\frac{1}{2}$ *	*m*' 24 * 9$\frac{1}{2}$ D top
Kδ'*p*	74 35$\frac{1}{2}$ *	*p* 24 * 16$\frac{1}{2}$ D top
Kδ'α	108 48$\frac{1}{2}$ *	α 12 * 8$\frac{1}{2}$ D top Pole 4 ft. 4 in.
Kδ'N	164 19$\frac{1}{4}$ *	N 6 * 8$\frac{1}{2}$ D top
Kδ'α'	108 56 *	K 7 * 3 D
Kδ'E	2 30$\frac{1}{4}$ *	E 6 * 49 D
Eδ'*a*	9 5$\frac{1}{2}$ *	*a* 8 * 50$\frac{1}{2}$ D
γ'δα	113 20$\frac{1}{2}$ *	
γ'δ'*t*	70 14$\frac{1}{2}$ *	*t* 24 20$\frac{1}{2}$ D

At F'	Theodolite = 4 ft. 7$\frac{1}{2}$ in.	
DF'*n*	56 24$\frac{7}{10}$ *	
DF'P	77 37$\frac{1}{2}$ *	
DF'α	174 40$\frac{1}{4}$ *	α 8 * 12$\frac{4}{5}$ D
DE'β	124 47 *	β 10 * 30$\frac{1}{3}$ D
DF'γ	103 4 *	γ 8 * 28$\frac{3}{5}$ D
DF'*t*'	77 42	D 7 * 11 E

At *t*'	Theodolite = 4 ft. 10 in.	
β*t*'F'	57 7	β 1 * 37$\frac{1}{2}$ D
β*t*'*k*	115 32	*k* 0 41 D
F'*t*'*k*	172 38	F' 7 * 50$\frac{1}{2}$ E

At *k*	Theodolite = 4 ft. 10$\frac{1}{2}$ in.	
	° '	° '
t'*k*F'	4 42	F 5 11 E
t'*k*α	34 47	α 0 27 D
t'*k*β	37 13	β 0 52 D
		t' 0 26 E

At *a*	Theodolite = 4 ft. 8$\frac{1}{2}$ in.	
*ba*K	113 57$\frac{1}{2}$	K 14 45 E Top of the cairn.
*ba*N	155 24	
*ba*W	80 40	
*ba*L	59 48	

At *b*	Theodolite = 4 ft. 6 in.	
	43 35	K 13 32 E
*ab*K	18 49	N 11 26
*ab*N		*a* 5 39

At *d*	Theodolite = 4 ft. 9 in.	
*cd*N	14 11	N 11 33 E
*cd*G	34 53	
*cd*H	63 27	H 6 36 D
*cd*L	111 12	L 6 42 D

At *c*	Theodolite = 4 ft. 8 in.	
*dc*L	49 34 *	L 7 * 21$\frac{1}{2}$ D
*dc*H	97 9 *	H 9 * 17$\frac{1}{2}$ D
*dc*G	136 9$\frac{1}{2}$ *	G 4 * 58 D
*dc*K	118 55$\frac{1}{2}$ *	K 14 * 35$\frac{1}{2}$ E
*dc*F	144 20	F 4 24 D

At

At a′	Theodolite = 4 ft. 10 in.	
	° ′	° ′
E a′α′	102 48 *	α′ 7 * 56 E
E a′δ′	97 5½ *	δ′ 8 * 10 E
E a′b′	65 58½ *	b′ 6 * 4½ D
E a′c′	88 6 *	c′ 11 * 20½ D
E a′d′	108 21½ *	d′ 2 * 34½ E
		E 7 19 E

At b′	Theodolite = 4 ft. 9½ in.	
c′b′a′	36 41½ *	a′ 5 * 44½ E
c′b′E	53 26½ *	E 8 * 41½ E
		c′ 4 41 D

At c′	Theodolite = 4 ft. 8½ in.	
	° ′	° ′
a′c′b′	121 12½ *	b′ 6 * 1 E
		a′ 9 * 6 E

At d′	Theodolite = 4 ft. 5 in.	
a′d′α′	169 14½ *	α′ 12 * 35½ E
		a′ 2 48 D

Several other angles and bearing of objects were taken, which, being of no uſe in computing the attraction of the hill, are here omitted.

The foregoing tables, containing all the angles collected together which were obſerved at the ſame point, include all the horizontal angles that were, at different times taken for aſcertaining the relative places of the principal points and objects on a horizontal plane. The numerous other angles uſed, in finding the ſections of the ground, are given hereafter, with their computed reſults annexed to them.

We now proceed to ſpeak of the two principal baſes which were accurately meaſured, as foundations on which every thing elſe muſt depend; and firſt,

Of the measure of the base RB″ *in* Glenmore, *the valley on the South of* Schehallien, *taken the* 16*th*, &c. *of* Sept. 1774.

Here A and B are the names of the two measuring rods, which were laid down alternately in the order as expressed in the following table of measures. The lengths of these rods, by the brass standard, when the thermometer was at $62\frac{3}{4}$, were thus, *viz.*

$$\left.\begin{cases} A = 20 \text{ feet } 1{\cdot}255 \text{ inch.} = 20{\cdot}10458 \\ B = 20 \text{ feet } 1{\cdot}323 \text{ inch.} = 20{\cdot}11025 \end{cases}\right\} \text{feet.}$$

The numbers following each rod, with the sign + interposed, are inches and decimal parts; and they denote the distance beyond the end of each rod to the beginning of the next following rod; and, therefore, the sum of all these numbers must be added to the sum of the lengths of the rods themselves for the total of the measures. Also, as the first rod began at 2 feet 8 inches from the point R, this number is to be added to the total last mentioned, to give the measure of the whole base from R to B″.

A +8·29	B +3·45	B +7·22	A +3·34	A +4·47	A +3·69
B +2·53	A +3·80	A +1·91	B +3·65	B +3·75	B +4·07
A +6·11	B +6·64	B +4·46	A +6·96	A +4·74	A +2·75
B +6·66	A +7·76	A +1·95	B +3·07	B +3·06	B +4·65
A +2·79	B +3·28	B +2·26	A +3·55	A +2·58	A +4·07
B +1·20	A +4·87	A +4·54	B +2·93	B +4·15	B +3·23
A +2·07	B +6·18	B +4·48	A +5·53	A +3·16	A +4·16
B +4·80	A +8·70	A +3·14	B +5·33	B +4·64	B +5·73
A +0·00	A +7·87	B +3·38	A +4·38	A +3·26	A +4·12
B +1·78	B +4·75	A +5·00	B +3·67	B +4·18	B +4·91
A +3·29	A +6·56	B +4·85	A +5·12	A +4·04	A +3·18
B +2·85	B +5·24	A +6·12	B +1·06	B +2·92	B +3·91
A +6·39	A +7·90	B +3·44	A +5·96	A +3·10	A +5·28
B +4·86	B +6·32	A +6·18	B +2·47	B +5·11	B +2·90
A +6·08	A +6·92	B +4·19	A +3·84	A +4·61	A +4·39*
B +8·58	B +7·28	A +4·51	B +5·57	B +3·34	B +4·37
A +9·07	A +6·34	B +3·04	A +2·63	A +2·57	A +3·29
B +1·53	B +8·93	A +4·37	B +7·41	B +5·80	B +2·12
A +2·28	A +5·39	B +2·96	A +3·11	A +3·37	A +2·95
B +7·47	B +5·20	A +2·47	B +1·74	B +2·58	B +3·30
A +2·40	A +3·54	B +3·90	A +2·07	A +2·24	A +2·82
B +5·42	B +1·26	A +5·78	B +4·33	B +3·48	B +3·97
A +7·42	A +3·20	B +3·97	A +5·93	A +2·95	A +1·37
B +8·14	B +5·34	A +4·87	B +6·36	B +2·88	B +0·00
A +8·77	A +3·74	B +4·83			

The ſum of all theſe is 74A+73B+669·28 inches, or 74A+73B+55·773 feet, including the 2 feet 8 inches at the beginning of the meaſurement.

Now 74A is = 1487·73892
73B is = 1468·048825
The odd parts 55·773

Sum 3011·56017 = the base unreduced.

But a reduction of this must be here made according to the state of the thermometer, and for the wearing of the brass 5 feet standard (see Phil. Transf. vol. LVIII. for the year 1768, p. 313, &c.). Now the difference between 62° and $62\frac{3}{4}$ being $\frac{3}{4}$, therefore $3011·56 \times \frac{232}{1800000 \times 12} \times \frac{3}{4} = 3011·56 \times \frac{29}{2700000} \times \frac{3}{4} = 0·024$ feet, is the small correction on account of the thermometer, and which being added makes the number become 3011·584 for the length of the base as reduced to the state of 62° of FAHRENHEIT's thermometer. But the brass rod had been $\frac{1}{1000}$th of an inch shortened by wearing, and it was originally $\frac{1}{1000}$th of an inch shorter than the Royal Society's brass standard yard, so that it is now $\frac{1}{500}$dth inch shorter than that standard in the length of 3 feet, or $\frac{1}{18000}$th part of the whole; therefore subtracting the $\frac{1}{18000}$th part, or ·167 from the above quantity, there remains 3011·417 feet for the corrected measure of this base, or the true length of the line RB″.

The above measures, as far as to that marked * inclusively, together with 10 feet $10\frac{1}{2}$ inches more, reach

to a place to which they had before meaſured with the tape line, and by it found to be 2844·8 feet; while the meaſure of the ſame by the rods is found to be 2839·3 feet. The difference is $5\frac{1}{2}$ feet, a ſmall part of which might be owing to the unſtable ſtate of the wooden ſtands uſed in the firſt quarter of the baſe; but the greater part of this difference is more likely to be owing to the uncertain way of meaſuring with a tape, which, to ſay nothing of the ground not being quite level, is liable to be ſtretched more or leſs in length with different degrees of tenſion, and to be variouſly warped in length by moiſture.

Of the meaſurement of the baſe $\alpha\beta\gamma$ *in* Rannoch, *to the North-weſt of the hill of* Schehallien.

1. One part of this baſe was meaſured twice over in different ways. The part $\alpha\beta$ was carefully meaſured on the 8th of October 1774 with a chain, and found to be 63 chains and $40\frac{1}{2}$ links, or 63·405 chains in length.

Now on the 24th of the ſame month the chain was meaſured by means of the five-feet braſs ſtandard, when the thermometer was at $38°\frac{1}{2}$, and the length found to be 65·94542 feet. Hence then $65{\cdot}94542 \times 63{\cdot}405 = 4181{\cdot}269$ is the length of all the chains, to which, add-

 ing

ing 1·764 the breadth of the 63 iron pins, the sum is 4183·033 for the length of $\alpha\beta$ uncorrected.

But $62-38\frac{1}{2}=23\frac{1}{2}$, therefore $-23\frac{1}{2}\times\frac{29}{2700000}\times 4183 = -1{\cdot}056$ is the reduction on account of the state of the thermometer, which being applied with its proper sign, there results 4181·977; and from this last number deducting again $\frac{1}{18000}$th part or ·232, on account of the wearing of the brass standard, there then remains 4181·745 feet for the length of the part $\alpha\beta$ of the base in Rannoch, as measured by the chain.

But as the chain was measured not at the same time with the base, but between two and three weeks later, when the air was probably cooler, the reduction above made for the state of the thermometer is perhaps something too great, and we may safely conclude $\alpha\beta$ to be equal 4182 feet as measured by the chain.

2. The whole base $\alpha\beta\gamma$ was next, on the 10th, 11th, and 12th of October, very carefully measured by the twenty-feet measuring rods. The rods at that time measured thus, $\left\{\begin{array}{l} \text{A} = 20 \text{ ft.} + 1{\cdot}306 \text{ inch.} = 20{\cdot}108\frac{5}{6} \\ \text{B} = 20 \text{ ft.} + 1{\cdot}354 \text{ inch.} = 20{\cdot}112\frac{5}{6} \end{array}\right\}$ feet; the thermometer being then at 40°. The number of rods and the additional parts were as follows.

A	+ 4·49	A	+ 3·99	B	+ 1·73	B	+ 3·22	B	+ 2·43	B	+ 1·87
B	+ 3·29	B	+ 3·13	A	+ 2·41	A	+ 3·16	A	+ 1·97	A	+ 2·38
A	+ 6·57	A	+ 3·55	B	+ 2·99	B	+ 2·07	B	+ 1·74	B	+ 2·67
B	+ 3·62	B	+ 4·19	A	+ 2·62	A	+ 2·22	A	+ 2·84	A	+ 2·07
A	+ 3·84	A	+ 4·06	B	+ 2·18	B	+ 4·83	B	+ 3·65	B	+ 1·84
B	+ 3·52	B	+ 3·94	A	+ 2·72	A	+ 2·39	A	+ 1·62	A	+ 4·03
A	+ 4·50	A	+ 3·64	B	+ 3·02	B	+ 2·68	B	+ 1·27	B	+ 1·72
B	+ 3·62	B	+ 3·23	A	+ 2·46	A	+ 2·09	A	+ 2·38	A	+ 2·14
A	+ 4·88	A	+ 3·76	B	+ 3·68	B	+ 1·97	B	+ 2·36	B	+ 1·80
B	+ 2·74	B	+ 2·56	A	+ 2·62	A	+ 1·48	A	+ 2·57	A	+ 1·27
A	+ 3·24	A	+ 3·38	B	+ 2·72	B	+ 3·20	B	+ 2·07	B	+ 1·88
B	+ 4·30	B	+ 3·29	A	+ 3·33	A	+ 2·62	A	+ 2·48	A	+ 2·36
A	+ 3·50	A	+ 3·65	B	+ 2·93	B	+ 2·37	B	+ 2·31	B	+ 2·04
B	+ 3·26	B	+ 3·51	A	+ 3·14	A	+ 2·47	A	+ 2·28	A	+ 2·37
A	+ 2·96	A	+ 3·88	B	+ 2·93	B	+ 3·48	B	+ 3·96	B	+ 1·77
B	+ 3·32	B	+ 2·29	A	+ 2·40	A	+ 3·16	A	+ 4·87	A	+ 1·66
A	+ 5·93	A	+ 3·13	B	+ 2·06	B	+ 3·50	B	+ 2·61	B	+ 2·26
B	+ 5·00	B	+ 3·71	A	+ 3·13	A	+ 2·33	A	+ 2·22	A	+ 1·97
A	+ 3·43	A	+ 3·13	B	+ 2·57	B	+ 2·37	B	+ 1·63	B	+ 1·77
B	+ 3·87	B	+ 3·13	A	+ 2·85	A	+ 2·68	A	+ 1·87	A	+ 2·63
A	+ 6·37	A	+ 5·43	B	+ 2·87	B	+ 2·68	B	+ 2·41	B	+ 3·18
B	+ 3·48	B	+ 3·08	A	+ 4·12	A	+ 2·70	A	+ 2·62	A	+ 2·09
A	+ 4·86	A	+ 3·57	B	+ 2·20	B	+ 2·68	B	+ 2·09	B	+ 1·74
B	+ 4·87	B	+ 5·68	A	+ 2·68	A	+ 2·05	A	+ 2·27	A	+ 2·74
A	+ 3·08	A	+ 3·89	B	+ 2·17	B	+ 3·09	B	+ 3·02	B	+ 4·49
B	+ 3·67	B	+ 2·78	A	+ 2·13	A	+ 2·50	A	+ 2·41	A	+ 2·60
A	+ 3·28	A	+ 3·27	B	+ 2·17	B	+ 2·52	B	+ 2·53	B	+ 2·45
B	+ 3·43	B	+ 1·84	A	+ 3·12	A	+ 2·62	A	+ 1·84	A	+ 1·41
A	+ 4·82	A	+ 0·00	B	+ 2·70	B	+ 2·43	B	+ 2·68	B	+ 2·67
B	+ 4·46	A	+ 3·14	A	+ 3·17	A	+ 3·01	A	+ 2·37	A	+ 1·98

B +2·96	A +1·66	B +2·77	B +2·37	B +1·86	B +2·49
A +2·27	B +1·53	A +2·09	A +2·86	A +2·33	A +2·11
B +2·60	A +1·80	B +2·14	B +2·42	B +2·65	B +3·03
A +3·08	B +3·53	A +2·45	A +2·16	A +2·14	A +2·67
B +2·23	A +2·45	B +2·98	B +2·21	B +2·35	B +2·43
A +4·70	B +0·00*	A +2·40	A +2·75	A +2·43	A +1·93
B +2·28	A +2·36	B +2·50	B +2·03	B +2·59	B +2·75
A +1·79	B +2·75	A +2·82	A +2·73	A +2·48	A +1·99
B +1·83	A +1·77	B +2·37	B +2·14	B +2·48	B +2·17
A +2·74	B +1·55	A +2·76	A +1·57	A +2·91	A +1·83
B +2·48	A +1·97	B +2·91	B +1·77	B +2·47	B +1·93
A +2·23	B +2·04	A +2·58	A +2·21	A +1·80	A +1·65
B +1·66	A +2·56	B +2·34	B +2·11	B +1·99	B +1·04
A +3·08	B +2·15	A +2·86	A +2·99	A +2·81	A +1·96
B +2·31	A +2·26	B +2·67	B +2·06	B +2·44	B +2·17
A +2·20	B +2·37	A +2·19	A +2·36	A +2·07	A +2·25
B +3·06	A +1·94	B +2·37	B +1·77	B +2·44	B +1·79
A +2·36	B +1·97	A +2·93	A +1·66	A +2·11	A +0·00
B +2·24	A +1·94				

Of the foregoing measures, the sum of all from the beginning to that marked * inclusively, together with 13 feet 2 inches more, brings us to the point β before measured to by the chain. Now to this place, by adding together the measures, there are found to be 103 A and 102 B, and the sum of the parts is 586·71 inches.

Then

$$
\begin{aligned}
\text{Then } 103\,\text{A} &= 103 \times 20{\cdot}108\tfrac{5}{6} = 2071{\cdot}210 \\
102\,\text{B} &= 102 \times 20{\cdot}112\tfrac{5}{6} = 2051{\cdot}509 \\
586{\cdot}71 \text{ inches} &= 48{\cdot}893 \\
13 \text{ ft. } 2 \text{ inch.} &= 13{\cdot}167 \\
\text{Hence } \alpha\beta \text{ (unreduced) is } &\quad 4184{\cdot}779
\end{aligned}
$$

But ſince $62 - 40 = 22$, therefore the reduction for the ſtate of the air is $- 22 \times \frac{29}{27000000} \times 4185 = -{\cdot}989$, which being applied to the above ſum, there remains 4183·79 as correſponding to the ſtate of 62° of the thermometer. From this laſt number deduct its $\frac{1}{18000}$th part, *viz.* ·232, and there reſults 4183·558 for the correct length of the part $\alpha\beta$ as determined by this very accurate method; which is but about a foot and a half more than what it was found to be by the leſs accurate meaſure by the chain, which is a nearer approach to an equality than could well be expected.

To determine now the whole length of the baſe $\alpha\gamma$; by taking the whole ſums there are found to be 146 A with 144 B and 779·78 inches of the odd parts.

$$
\begin{aligned}
\text{Then } 146\,\text{A} &= 146 \times 20{\cdot}108\tfrac{5}{6} = 2935{\cdot}890 \\
144\,\text{B} &= 144 \times 20{\cdot}112\tfrac{5}{6} = 2896{\cdot}248 \\
779{\cdot}78 \text{ inches} &= 64{\cdot}982 \\
\text{The ſum or } \alpha\gamma \text{ (unreduced) is } &\quad 5897{\cdot}120
\end{aligned}
$$

The correction for the thermometer is $- 22 \times \frac{29}{2700000} \times 5897 = - 1{\cdot}394$, which being applied to the number above, there results 5895.726; and this again being diminished by its $\frac{1}{18000}$th part, or ·327, there remains 5895·399 feet, for the correct measure of the base $\alpha\gamma$ in the vale of Rannoch.

There is no occasion here to explain the manner of measuring these two bases by the twenty-foot rods, as that has been very circumstantially done in vol. LXV. of the Phil. Transf. for the year 1775, by the rev. Dr. MASKELYNE, the learned and accurate conductor of this very important experiment.

The following shorter lines were also measured as they happened to be wanted in different parts of the survey.

	Feet.	Inch.	
$\alpha'\delta'$ =	269	4	nearly horizontal.
Nn =	93	6	
Ke =	94	10	
KE =	240	10	
ac =	9	9	
an =	7	10	
cn =	1	11	
ma =	70	11	not horizontal.
mt =	68	3	
mp =	63	4	
pt =	27	2	

The

The other meaſures that were taken for determining the ſections will be delivered afterwards, when the reſults or computed altitudes have been obtained, in order to be placed oppoſite to their correſpondent angles.

Having now obtained, to a great degree of accuracy, the meaſured lengths of two lines which were to ſerve as baſes for all the future calculations, the next conſideration was how to make the propereſt uſe of them. Every other line or diſtance, drawn or conceived to be drawn, muſt be calculated from them by the help of the angles obſerved either at their extremities, or at all the other points and ſtations in the ſurvey and plan. As theſe two baſes are ſituated in the low parts of the country, from whence but a very few of the other principal ſtations are viſible, one method evidently is to compute immediately from theſe baſes ſuch of the great lines in the ſurvey whoſe extremities are viſible from them; and then from theſe calculated lines to compute others next to them, and ſo on quite around and within the whole figure. In this manner ſeveral values of each line will ariſe, both from the double computations by the two meaſured baſes, and from the various ſets of triangles which can be formed from the very numerous horizontal angles which were obſerved at the ſeveral ſtations. But in this mode of computation, after

great labour and pains, I had frequently the mortification to find that the several values of the same lines would differ so greatly one from another, that I was often very doubtful whether I could rely on any of them, or even on the mean among them all. These differences arose from the small errors in the observed angles, which in some degree are unavoidable; and indeed they were so small, that the sum of the angles of the several triangles which were used in the calculation seldom differed by more than a minute or two from 180°. But in a long connected chain of triangles, dependant on one another, the effects of such small errors at length become too great to be tolerated in a computation requiring much accuracy. Another method is, first to compute from both bases the length of the line KN extended along the ridge of the hill from East to West, and from it, as a secondary base, compute all the other lines in the plan. This method admits of much more accuracy than the former, supposing this secondary base to be truly assigned; because that, from the elevated and central situation of this line, all or most of the other points in the survey are visible from one or both of its extremities, by which it happens that the other lines are mostly determinable from it alone, without so close a connection with one another as in the other method of computation. By

both

both of theſe methods then, and by all the triangles furniſhed by each of them, I computed all the principal lines in the plan, and either took a mean among the ſeveral values of each, or elſe ſelected out of them ſuch one as from various circumſtances I judged it ſafeſt to rely upon, as neareſt the truth. The trigonometrical computations were always accurately made, and generally repeated by logarithms, and the reſult of every proportion determined to two or three places of decimals. I ſhall here abſtract the mean or corrected values of ſome of the principal lines or horizontal diſtances ſo computed, as well as the ſecondary baſe KN from the Eaſtern to the Weſtern cairn.

The mean among a great number of ways of computation from the South baſe gives the horizontal diſtance from K to N = 4052·2, and the mean of all the reſults from the North baſe $\alpha\beta\gamma$ gives KN = 4058·9, and the mean between theſe two gives 4055·5 for the mean diſtance of K and N. And this value of KN was uſed in computing moſt of the other lines, whoſe mean reſults are as here follows.

$\alpha\gamma$ = 5895·4 the Northern base in Rannoch.
RB″ = 3011·4 the Southern base in Glenmore.
NK = 4055·5 the distance of the two cairns.

RA = 5670	NR = 5545	KR = 5952	OR = 3582
AB = 1489	NB″ = 6053	KF = 8227	OB″ = 5466
BC = 4506	NA = 5941	KG = 8036	OA = 6769
CD = 775	NB = 6573	KH = 7748	OS = 3271
DF = 7388	NC = 7797	KW = 7603	OX = 4079
FG = 1166	ND = 7657	KL = 8335	OU = 6061
GH = 4068	NF = 5980	KY = 10008	OZ = 9073
HW = 2118	NG = 6370	KV = 10215	OM = 3317
WL = 1816	NH = 8195	KO = 2615	
LY = 7085	NW = 9059	KP = 3221	
YV = 3636	NL = 10405	Kα = 13710	MS = 381
VT = 2645	NY = 13752	Kβ = 15404	Te = 1335
TZ = 4393	NS = 5795	KM′ = 1817	Ze = 3719
ZU = 4132	NO = 2875	Kδ' = 2528	F′D = 6430
UX = 1984	NP = 3271	Ka = 3326	F′F = 3934
XS = 2378	Nα = 11876	Kb = 4409	F′t' = 4098
SR = 1410	Na = 5899		$t'k$ = 2327
	Nb = 7614		M′b = 1172
	Nb = 3381	PG = 4815	ab = 1843
	Nδ' = 1585	PH = 5196	cd = 1750

From the three first lines, or bases, and the horizontal angles observed at the several stations, a very large and accurate plan of the whole survey was constructed, forming a map of four feet long by four feet broad, which was verified in every part by the measures of the computed

puted lines, both thoſe above-written and others, and they were generally found to agree very exactly, according to the ſcale by which the plan was conſtructed. The uſe of this large map was to receive and admit of the diſtinct and accurate exhibition of the figures in their true places, expreſſing the number of feet in elevation or depreſſion with reſpect to each obſervatory of every point and ſection of the ground whoſe elevation or depreſſion might be obſerved. But before I proceed to the computation and conſtruction of the points in the ſections, I ſhall here abſtract the numbers which expreſs the relative elevation of the principal original points in the ſurvey, being the extremes of the lines whoſe lengths are above abſtracted. Theſe few numbers are the reſults of the calculation of ſeveral hundreds of triangles conceived in a vertical poſition, their baſes being either the horizontal lines above-written, or other lines drawn as diagonals between many diſtant points in the ſurvey, according to the number of vertical angles which had been obſerved; and of theſe baſes, whether real or imaginary, each generally afforded two vertical triangles, as the angles of elevation and depreſſion were taken alternately at both ends of the lines. It is ſcarcely neceſſary to remark, that all theſe triangles are right-angled, the common baſe being one of the ſides about the right angle, and the other the difference in altitude between the two

 given

given points or extremes of the baſe; and this difference in altitude is found from the application of this proportion, as radius is to the tangent of the angle of elevation or depreſſion, ſo is the given baſe to the altitudinal difference between the two given points, excluſive of the height of the theodolite or other inſtrument, which was afterwards allowed for. From the reſolution of all theſe triangles, and taking the means of the many correſponding reſults, were obtained the following numbers, which ſhew how many feet the points denoted by the letters ſtanding againſt them are below the level of the point N or the Weſtern cairn. They are all referred to this point N at the Weſtern extremity of the ridge of the hill, becáuſe it is the moſt elevated point in the whole ſurvey.

O	1184	γ	2898	H	2143	U	1613	e	2145
P	1457	A	1303	W	2024	X	1996	M	1958
K	480	B	1313	L	2006	S	1964	M′	322
R	1948	C	1384	Y	2335	a	1012	F′	2246
B″	1920	D	1445	V	2119	b	823	t'	2815
α	2898	F	1904	T	2114	c	1364	k	2835
β	2901	G	1935	Z	1815	d	1539	δ'	172

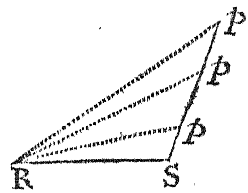
Theſe depreſſions, and thoſe of ſeveral other principal points, were firſt carefully computed by means of various different baſes, as ſo many places from whence the ſections were to commence.

Theſe ſections are very numerous, made in all directions from the primitive points before mentioned, and many of them extended to great diſtances far beyond the bounds of the plan hereunto annexed, ſo as to include the neareſt hills and valleys of the ſurrounding country. They are moſtly made in vertical planes in the manner deſcribed in the article of the Phil. Tranſ. before referred to, excepting ſome few of them which are level ſections in planes parallel to the horizon, and ſome indeed irregular as being neither vertical nor horizontal. To compute the relative altitude of each point in theſe ſections, it is evident, requires the reſolution of two different triangles, *viz.* a horizontal triangle by which its place in the plan is aſcertained, and a vertical triangle of which one ſide is the elevation or depreſſion of the point. Of theſe ſections there are above 70, containing near 1000 points, whoſe places in the plan and relative altitudes have been computed: ſo that the number of triangles, whoſe numerical reſolutions have been performed in the courſe of this buſineſs, amounts to ſeveral thouſands.

Before the abſtract of the computation of the ſections, I ſhall here put down at large the calculation of one of them, to ſhew the manner in which they have been computed in the readieſt and eaſieſt way that occurred to me; preſerving at the ſame time the proper degree of accu-

racy.

racy. I ſhall for this purpoſe ſelect the third ſection as not containing ſo many poles as ſome of the others. This ſection commences at S, and is carried up the hill in a vertical plane, making an angle of 105° with the line RS. The direction of this plane is here repreſented by the line Sppp making with RS the angle RSp = 105°. The points ppp &c. mark the places of the poles, whoſe angles of elevation or depreſſion were taken at S with a proper inſtrument, and they are written in the ſecond column of the table in this example. At R were obſerved the ſeveral horizontal angles, which lines ſuppoſed to be drawn from thence made with RS, and theſe are placed in the third column. And ſince in every triangle RSp, the angle S is conſtant, and the ſum of R and p is equal to the conſtant quantity 75°: therefore each of the angles R, or the numbers in the third column, being ſubtracted from 75°, there remains the correſponding angle p: and theſe remainders are placed in the fourth column. Then, ſince the method of ſolution is this, as ſ.p : ſ.R :: RS : S$p = \frac{\text{ſ.R}}{\text{ſ.}p} \times$ RS; and again, as radius (1) : tang. elev. :: Sp : alt. of p above S = S$p \times$ tang. elev. $= \frac{\text{ſ.R}}{\text{ſ.}p} \times$ RS $\times$ tang. elev. Or in logarithms



From	—	180°.
Take $\angle$S =		105
Leaves $\angle$R + $\angle p$ =		75

ſ.R−ſ.p+RS+tang. elev.=log. of the altitude of the point. Wherefore having taken, from a table, the ſines of R and p, and placed them in the fifth and ſixth columns, ſubtract the latter from the former, and write the remainders in the next or ſeventh column; to theſe add the conſtant logarithm of RS, and write the ſums in the eighth column; take out then the tangents of the angles in the ſecond column, and having placed them in the ninth column, add together the adjacent numbers of the eighth and ninth columns, placing the ſums in the tenth column, which being the logarithms of the altitudes or depreſſions of the points p, take the correſponding numbers from a table of logarithms, and write them in the eleventh or laſt column, for thoſe altitudes or depreſſions with reſpect to the point S, with the height of the theodolite included, and which is afterwards allowed for, its height being generally about $4\frac{1}{2}$ or $4\frac{3}{4}$ feet. In the ſecond column D denotes depreſſion and E elevation; in the laſt column D denotes depreſſion and A altitude.

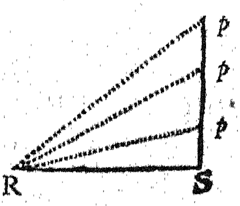
N° of Poles.	Angles of Dep. and Elev. at S.	Horiz. Ang. at R.	75° − ∠'s R, or angles p.	Sines of R	Sines of p.	Sin. R − sin. p.	Sin. R − sin. p + RS, or Sp.	Tang. of Dep. and Elev.	Sum of 8 and 9, or Log. of Dep. and Alt.	Dep. and [illegible] above S
	° ′	° ′	° ′							
1	3 27D	19 11	55 49	9·51666	9·91763	9·59903	2·74843	8·78022	1·52865	34D
2	2 36E	30 24	44 36	9·70418	9·84643	9·85775	3·00715	8·65715	1·66430	46A
3	4 33	38 28	36 32	9·79383	9·77473	0·01910	3·16850	8·90080	2·06930	117
4	6 12	44 10	30 50	9·84308	9·70973	0·13335	3·28275	9·03597	2·31872	208
5	7 51	47 55	27 5	9·87050	9·61828	0·25222	3·40162	9·13948	2·54110	348
6	10 38	51 22	23 38	9·89574	9·60302	0·29272	3·44212	9·27357	2·71569	520
7	12 20	53 9	21 51	9·90320	9·57075	0·33245	3·48185	9·33974	2·82159	663
8	13 46	54 43	20 17	9·91185	9·53991	0·37194	3·52134	9·38918	2·91052	814
9	15 43	56 21	18 39	9·92035	9·50486	0·41549	3·56489	9·44933	3·01422	1033
10	17 35	57 47	17 13	9·92739	9·47127	0·45612	3·60552	9·50092	3·10644	1278
11	18 0	58 58	16 2	9·93291	9·44122	0·49169	3·64109	9·51178	3·15287	1422
1	2	3	4	5	6	7	8	9	10	11

Thus

Thus then every line in the table contains the ſolutions of the two triangles, the one horizontal and the other vertical, uſed in finding the altitude of each point or pole in the ſection. The addition of the conſtant logarithm of the baſe RS to the logarithms in the ſeventh column, is moſt eaſily performed by writing it on the bottom of a little ſlip of paper, and ſo ſliding it down ſucceſſively over each of thoſe numbers, and in that poſition adding them together, and placing the ſums immediately oppoſite in the next column.

And in this manner were computed the relative altitudes of the points in the other vertical ſections; excepting two or three caſes, in which the conſtant angle formed by the ſection and the baſe was a right angle; and one caſe in which the vertical angles were not taken at the beginning of the ſection line, but at the other end of the baſe line where the horizontal angles were alſo obſerved. It may be neceſſary, therefore, to inſert and explain an example of each of theſe caſes, and the more ſo as they point out the propereſt means of meaſuring theſe ſections ſo as to ſave moſt part of the labour in the computation, in which the trouble chiefly conſiſts.

Of the caſe of the right angle, the firſt ſection is an inſtance, where alſo RS is the baſe as before, and the angle RS*p* being = 90°.



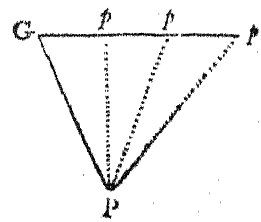
Poles.	Dep. and Elev. at s.	Horiz. Ang. at R.	Tang. of vert. ∠'s at s.	Tang. of ∠'s R.	Sum of Columns 4 and 5.	6th+RS =Log. Alt.	Depth and Alt.
	° ′	° ′					
1	5 [illegible]½ D	10 0	8·96533	9·24632	8·21165	1·36105	23D
2	0 30 E	31 35	7·94086	9·78874	7·72960	0·87899	7½ A
3	4 15	41 56	8·87106	9·95342	8·82448	1·97388	94
4	6 14¼	49 25	9·03861	10·06722	9·10583	2·25523	180
5	8 16	55 51½	9·16224	10·16870	9·33094	2·48033	302
6	10 13	59 57½	9·25582	10·23783	9·49365	2·64305	440
7	11 37	62 56½	9·31297	10·29174	9·60471	2·75411	568
8	12 25	65 3¾	9·34276	10·33257	9·67533	2·82472	668
9	13 21	66 41½	9·37532	10·36568	9·74100	2·89040	777
10	14 10	67 36¾	9·40212	10·38519	9·78731	2·93671	864
11	15 17	68 42½	9·43657	10·40925	9·84582	2·99522	989
12	17 46	70 58	9.50572	10.46221	9·96793	3·11733	1310
13	19 33	72 48	9·55035	10·50927	10·05962	3·20902	1618
14	20 6	74 30	9·56342	10·55701	10·12043	3·26983	1861
1	2	3	4	5	6	7	8

In this form there are three columns less than in the former, by which it happens, that about one-third of the labour is saved. The method of solution is thus; as 1 (radius) : tang. R :: RS : Sp = RS × t. R; and again, as 1 : tang. S (vertical angle) :: Sp : Sp × t. S = RS × t. R. × t. S. Or, in logarithms, log. RS + t. R + t. S = log. of the vertical perpendicular: and by this theorem, it is evident, the columns of this table are constructed.

But

But nearly the ſame ſaving in the great labour of computation would be made if the vertical and horizontal angles had both been taken at the end of the baſe fartheſt from the beginning of the ſection. And this method would alſo be much the eaſieſt in making the ſurvey on the ground, as there would then need only one obſerver with an inſtrument to meaſure both horizontal and vertical angles; and any perſon, without an inſtrument, could direct in a line the perſon who moves and places the poles, or he may even direct himſelf after his firſt pole has been placed, by means of a back object, as is commonly done in land ſurveying.

Of this kind there happens to have been one ſection taken, proceeding from G, and making with GP an angle of 85°, P being the Northern obſervatory, and where both the bearings and depreſſions of the points p in the ſection line were obſerved.



$$\begin{array}{rr} & 180° \\ \angle G = & 85 \\ \hline \angle P + \angle p = & 95 \end{array}$$

	Log.	
PG	3·68262	which is a conſtant number from which the ſines of p in the fifth column are to be deducted.
ſ.G	9·99834	
Sum	3·68096	

Poles.	Vertic. Angles at P.	Horizont. Ang. at P.	$95° - \angle$ P $= \angle p$	Sines of $\angle$'s at p.	PG + s.G − $s.p$ = Pp	Tang. of Depr.	Sum of Col. 6 and 7. = Pp.	Dep. below P.
	° ′	° ′	° ′					
1	8 45D	9 15	85 45	9·99880	3·68216	9·18728	2·86944	740
2	8 46	16 25	78 35	9·99132	3 68964	9·18812	2·87776	755
3	9 58	27 16	67 44	9·96634	3·71462	9 24484	2·95946	911
4	8 38	30 52	64 8	9·95415	3 72681	9·18136	2·90817	809
5	7 6	34 30	60 30	9·93970	3·74126	9·09537	2·83663	686
6	5 23	37 55	57 5	9·92400	3·75696	8·97421	2·73117	538
1	2	3	4	5	6	7	8	9

Here it is evident is a saving of two of the most laborious columns in the table. This happens because that in every triangle PGp there are now constant those two parts which are used in the proportion made use of in the calculation, *viz.* PG and the angle G. For then it is, as f.p : f.G :: PG : Pp, or log. Pp=log. PG+f.G−f.p; so that the sum of the logarithms of PG and sine of $\angle$ G is a constant number, from which the numbers in the fifth column are to be subtracted, to find those in the sixth column. The rest of the work is the same as in the first example.

As to the irregular sections, the computation of them differs so little in manner from that of the usual vertical sections,

ſections, that an example of it is unneceſſary: and the few horizontal ſections need no computation, but only an allowance for the height of the theodolite.

In the following abſtract of the reſults of the computation of the ſections, the firſt column contains the number of the pole, the ſecond and third the vertical and horizontal angles, and the laſt the difference of altitude in feet, between the foot of each pole and the point from whence the vertical angles were obſerved, after making the allowance for the height of the theodolite above the ground. At the end of this abſtract is a plate of the figures referring to the number of the ſection, ſhewing the direction in which it was carried, with the degrees and minutes in the angle formed by it and the baſe line.

E. C.

SECTION 1.

Pole.	Vert. ∠'s at s.	Bearings at R.	Diff. of Alt.
	° ′	° ′	
1	5 $16\frac{1}{2}$ D	10 0	18 D
2	0 30 E	31 35	12 A
3	4 15	41 56	99
4	6 $14\frac{1}{4}$	49 25	185
5	8 16	55 $51\frac{1}{2}$	307
6	10 13	59 $57\frac{1}{2}$	444
7	11 37	62 $56\frac{1}{2}$	572
8	12 25	65 $3\frac{3}{4}$	673
9	13 21	66 $41\frac{1}{2}$	782
10	14 10	67 $36\frac{3}{4}$	869
11	15 17	68 $42\frac{1}{2}$	994
12	17 46	70 58	1315
13	19 33	72 48	1623
14	20 6	74 30	1866

SECTION 2.

Pole.	Vert. ∠'s at s.	Bearings at R.	Diff. of Alt.
	° ′	° ′	
1	3 40 D	20 46	30 D
2	1 26 E	32 42	28 A
3	4 20	42 8	103
4	6 21	49 30	192
5	9 $58\frac{1}{2}$	59 2	429
6	11 $50\frac{1}{2}$	62 30	591
7	12 52	65 2	721
8	13 22	65 51	780
9	13 $36\frac{1}{2}$	66 9	806
	° ′	° ′	
10	15 $14\frac{1}{2}$	67 36	972
11	16 24	68 45	1118
12	17 45	70 0	1302
13	18 45	71 3	1467
14	19 35	71 $57\frac{1}{2}$	1625
15	19 59	72 34	1726

SECTION 3.

Pole.	Vert. ∠'s at s.	Bearings at R.	Diff. of Alt.
	° ′	° ′	
1	3 27 D	19 11	29 D
2	2 36 E	30 24	51 A
3	4 33	38 28	122
4	6 12	44 10	213
5	7 51	47 55	352
6	10 38	51 22	524
7	12 20	53 9	668
8	13 46	54 43	818
9	15 43	56 21	1038
10	17 35	57 47	1283
11	18 0	58 58	427

SECTION 4.

Pole.	Vert. ∠'s at R.	Bearings at s.	Diff. of Alt.
	° ′	° ′	
1	7 42 D	13 8	39 D
2	1 2 E	31 35	19 A
3	4 4	40 24	80
4	5 36	48 36	137
5	6 55	56 56	215

	° ′	° ′	
6	9 10	62 36	337
7	9 23	68 12	415
8	10 17	70 33	495
9	11 18	74 0	623
10	13 2	76 3	785
11	13 37	76 47	848
12	14 30	77 45	946
13	15 3	78 58	1042
14	16 15	80 11	1200
15	17 24	81 13	1362
16	18 32	82 5	1528
17	19 29	82 59	1699
18	20 7	83 30	1812

SECTION 5.

Pole.	Vert. ∠'s at s.	Bearings at x.	Diff. of Alt.
	° ′	° ′	
1	3 46 D	15 0	38 D
2	0 14 E	24 23	9 A
3	2 16	36 14	60
4	3 29	46 5	112
5	4 21	54 24	163
6	5 48	63 55	258
7	7 2	71 30	361
8	9 3	76 30	513
9	11 25	81 18	720
10	12 55	84 16	874
11	13 54	87 22	1017
12	14 51	90 30	1182
13	15 9	93 0	1295

SECTION 6.

Pole.	Vert. ∠'s at x.	Bearings at s.	Diff. of Alt.
	° ′	° ′	
1	9 16 D	11 43	80 D
2	4 46 D	16 51	60 D
3	1 10 E	23 50	28 A
4	4 51	30 2	137
5	6 6	33 14	196
6	7 32	37 25	287
7	9 30	40 24	409
8	11 15	43 12	546
9	12 22	45 18	656
10	13 35	47 9	782
11	15 19	48 54	955
12	16 29	50 12	1093
13	17 8	51 1	1171
14	17 27	51 48	1248

SECTION 7.

Pole.	Vert. ∠'s at x.	Bearings at s.	Diff. of Alt.
	° ′	° ′	
1	7 30 D	9 30	60 D
2	1 49	15 52	24
3	2 5 E	20 49	52 A
4	4 30	24 48	135
5	5 41	28 23	208
6	7 13	30 27	297
7	8 14	32 29	380
8	9 24	33 41	465
9	10 26	35 0	558

	° ′	° ′	
10	11 41	36 21	678
11	12 36	37 15	773
12	13 31	38 17	885
13	13 46	39 30	973
14	13 56	40 17	1036

SECTION 8.

Pole.	Vert. ∠'s at A.	Bearings at B.	Diff. of Alt.
	° ′	° ′	
1	17 56½ D	37 12	516 D
2	10 12½	41 50	359
3	5 41	44 48	229
4	2 25	47 2	107
5	0 29	49 6	20
6	1 12 E	51 0	74 A
7	3 0	52 35½	198
8	5 0	54 44½	378
9	6 18	55 56½	520
10	7 2	56 47½	620
11	8 52	57 24	821
12	10 10	57 55½	986
13	11 24	58 28	1160
14	12 20½	58 58	1316

N.B. The place of this last pole would seem to be the same as N the Western cairn, as the section was directed through it. But then the last number 1316 is too great; as, from all the other measures, the diff. in alt. between A and N is only 1303 feet. This diff. of 13 feet seems to be caused by the last bearing being about 7′ too great, for in other places this angle is only 58° 51′. And indeed many other angles taken at the same time with the above seem to be much wrong, as they greatly differ from corresponding ones taken at other times.

Such differences among corresponding angles I often met with in the measures contained in the books of the survey, and it required much care to detect them, and trouble to reconcile them.

SECTION 9.

Pole.	Bearings at A.	Bearings at B.
	° ′	° ′
1	132 21½	36 45½
2	130 27	37 34½
3	127 26	39 8
4	124 25½	40 18
5	119 6½	43 19
6	111 37	48 7
7	103 45½	52 58
8	95 25	58 50
9	85 55	67 54
10	78 2½	75 58½
11	73 14	82 30
12	71 41½	86 21
13	70 38½	89 33
14	69 39	92 23

This is a horizontal or level section through A, and therefore each point is 4½ feet (the height of the theodolite) above that point.

SECTION 10.

Pole.	Vert. ∠'s at A.	Bearings at B.	Diff. of Alt.
	° ′	° ′	
1	19 41 D	51 12	531 D
2	13 52	57 22½	426
3	8 7½	62 56	295
4	2 30	68 47	106
5	0 59	73 55½	47
6	0 22½ E	76 52	27 A
7	0 59	79 14½	70
8	1 39	81 7	124
9	2 25	82 42	194
10	3 5	84 12	266
11	3 36	85 41½	338
12	3 48	86 27	373

SECTION 11.

Pole.	Vert. ∠'s at D.	Bearings at C.	Diff. of Alt.
	° ′	° ′	
1	12 56 D	48 18	231 D
2	9 50	62 4	334
3	5 15	67 9	242
4	2 25	69 51½	135
5	0 59 E	71 20	70 A
6	3 0	72 13	220
7	4 31	73 9	363
8	6 7	73 53	533
9	6 59	74 27	652
10	7 53	74 52	777
11	8 49	75 9½	904
12	9 27	75 42	1048
13	9 44	76 12½	1168
14	9 53	76 34	1258
15	10 38	76 55½	1443

SECTION 12.

Pole.	Bearings at D.	Bearings at C.
	° ′	° ′
1	68 14½	102 6
2	71 16	98 23
3	73 55	94 56
4	79 0	89 33
5	85 28	82 49
6	91 19	76 48
7	97 5	71 8
8	102 21	66 20
9	108 18	60 36
10	113 33	56 15
11	117 12	53 17
12	119 16	52 4
13	119 53	52 1

Each of these poles is five feet above D, the section being horizontal and taken at that point.

SECTION 13.

Pole.	Vert. ∠'s at G.	Bearings at F.	Diff. of Alt.
	° ′	° ′	
1	3 7 D	75 2	92 D
2	0 12	79 46	2
3	4 1 E	91 55	221 A
4	6 8	95 0	385
5	7 49	96 34	530
6	9 50	99 27	789
7	10 52	100 10	915

	° ′	° ′	
8	11 52	100 44	1040
9	12 52	101 15	1172
10	13 56	101 49	1327
11	14 49	102 20	1472
12	16 10	103 3	1710
13	16 45	103 27	1837
14	16 55	104 20	2013

SECTION 14.

Pole.	Vert. ∠'s at G.	Bearings at F.	Diff. of Alt.
	° ′	°	
1	2 3 E	97 5	73 A
2	2 35	99 14	95
3	3 52	109 39	182
4	5 58	117 53	384
5	6 25	119 8	440
6	8 31	120 34	633
7	10 14	121 51	827
8	11 29	122 41	985
9	12 30	123 34	1149
10	13 4	124 15	1272
11	13 14	124 41	1339

SECTION 15.

Pole.	Vert. ∠'s at G.	Bearings at H.	Diff. of Alt.
	° ′	° ′	
1	9 46 D	14 14	172 D
2	3 32	23 9	102
3	0 45	31 7	27
4	1 43 E	36 15	94 A
	° ′	° ′	
5	6 27	45 0	465
6	8 10	47 32	642
7	9 10	49 47	781
8	10 39	51 37	970
9	12 0	53 36	1177
10	13 24	55 38	1422
11	14 0	57 18	1584
12	14 18	58 37	1704
13	14 21	58 58	1734

SECTION 16.

Pole.	Vert. ∠'s at H.	Bearings at G.	Diff. of Alt.
	° ′	° ′	
1	7 22 D	21 37	189 D
2	6 11	34 32	248
3	4 1	43 23	201
4	0 9	48 20	4
5	1 43 E	54 38	119 A
6	3 35	60 8	275
7	5 14	65 8	446
8	6 49	70 13	653
9	8 1	73 19	828
10	8 41	76 39	977
11	9 18	78 38	1104
12	10 0	80 22	1246
13	10 44	82 0	1403
14	11 31	83 24	1572
15	13 8	84 43	1874

There seems to be some general error in this section, as the depressions and altitudes are utterly incompatible with those of all the other neighbouring points in the plan.

SECTION 17.

Pole.	Vert. ∠'s at H.	Bearings at G.	Diff. of Alt.
	° ′	° ′	
1	12 15 D	16 13	242 D
2	7 11	25 6	216
3	4 19	33 3	171
4	1 49	38 20	82
5	1 59 E	45 24	119 A
6	3 38	50 3	243
7	5 10	53 33	377
8	7 10	57 3	572
9	8 17	60 46	731
10	8 45	62 58	820
11	9 53	66 56	1038
12	10 24	69 0	1161
13	10 45	70 35	1260
14	11 31	72 15	1424
15	12 10	73 55	1590
16	12 31	75 7	1703

SECTION 18.

Pole.	Vert. ∠'s at H.	Bearings at W.	Diff. of Alt.
	° ′	° ′	
1	13 35 D	21 40	186 D
2	9 17½	43 28	263
3	5 21	55 3	203
4	2 4	63 8	95
5	1 23 E	69 50	84 A
6	3 37	73 24	238
7	5 25	76 0	386
	° ′	° ′	
8	6 55	78 7	531
9	8 33	81 13	737
10	9 28	88 4	1108
11	9 38	89 10	1201
12	9 53	89 58	1290
13	10 24	90 33	1407
14	11 2	91 10	1552

SECTION 19.

Pole	Vert. ∠'s at W.	Bearings at L.	Diff. of Alt.
	° ′	° ′	
1	18 8 D	14 51	168 D
2	16 28	31 38	388
3	11 53	39 42	394
4	7 47	42 48	292
5	4 11	46 12	180
6	2 10	48 6	100
7	0 31	50 16	23
8	2 24 E	52 45	148 A
9	5 55	54 48	402
10	8 48	56 30	657

SECTION 20.

Pole.	Vert. ∠'s at W.	Bearings at L.	Diff. of Alt.
	° ′	° ′	
1	22 45 D	7 37	98 D
2	19 0	16 20	179
3	18 41	31 8	367
4	14 20	41 54	412
5	9 41	48 4	341

	° ′	° ′	
6	3 38	54 6	155
7	1 15	56 50	57
8	0 55 E	60 7	55 A
9	4 4	62 58	257
10	6 45	66 2	487

SECTION 21.

Pole.	Vert. ∠'s at L.	Bearings at W.	Diff. of Alt.
	° ′	° ′	
1	23 59 D	21 18	295 D
2	19 25	33 12	377
3	14 37	48 26	418
4	10 5	56 7	383
5	5 9	62 51	237
6	1 2	69 24	56
7	2 2 E	71 36	133 A
8	4 22	73 2	297
9	6 6	75 22	455

SECTION 22.

Pole.	Vert. ∠'s at *a*.	Bearings at *b*.	Diff. of Alt.
	° ′	° ′	
1	7 16 D	5 2	20 D
2	2 49 E	10 20	25 A
3	3 33	15 13	44
4	4 18	29 34	120
5	7 0	32 15	222
6	9 1	35 3	331
7	10 26	38 52	471
8	12 19	41 46	657
	° ′	° ′	
9	14 40	43 34	874
10	14 45 top of E. cairn.	43 35	874

SECTION 23.

Pole.	Vert. ∠'s at *a*.	Bearings at *b*.	Diff. of Alt.
	° ′	° ′	
1	15 2 D	4 33	34 D
2	0 51	15 15	4
3	0 10	35 37	0
4	3 7 E	41 53	97 A
5	6 43	42 34	209
6	7 36	43 30	244
7	9 50	45 48	342
8	11 35	59 5	664

SECTION 24.

Pole.	Vert. ∠'s at *b*.	Bearings at *a*.	Diff. of Alt.
	° ′		
1	3 27 D	10 29	29 D
2	0 32 E	21 9	12 A
3	2 58	32 57	64
4	4 40	58 53	133
5	5 14	76 22	173
6	6 24	121 5	340
7	7 50	125 12	448
8	8 27	130 23	546
9	8 55	134 57	664
10	9 6	136 15	712

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SECTION 25.

Pole.	Vert. ∠'s at *b.*	Bearings at *a.*	Diff. of Alt.
	° ′	° ′	
1	15 39 D	8 14	74 D
2	1 47 E	52 24	54 A
3	3 9	63 13	115
4	5 51	70 37	248
5	9 11	81 35	505
6	11 21	85 10	691
7	12 44	87 45	802

SECTION 26.

Pole.	Vert. ∠'s at *b.*	Bearings at *a.*	Diff. of Alt.
	° ′	° ′	
1	17 53 D	6 35	64 D
2	0 26	48 5	9
3	2 1 A	60 8	93 A
4	3 4	62 45	151
5	4 40	66 15	256
6	6 20	68 29	374
7	8 10	71 57	550

SECTION 27.

Pole.	Vert. ∠'s at *b.*	Bearings at *a.*	Diff. of Alt.
	° ′	° ′	
1	4 30 E	59 25	280 A
2	3 33	57 12	204
3	1 47	54 11	92
4	0 10 D	50 15	3 D
5	1 22	46 54	47
	° ′	° ′	
6	2 20	42 15	69
7	4 50	37 30	122
8	18 52	6 12	65

SECTION 28.

Pole.	Vert. ∠'s at T.	Bearings at V.	Diff. of Alt.
	° ′	° ′	
1	10 23 D	16 36	142 D
2	9 26	23 56	195
3	5 23	28 45	136
4	4 14	32 24	120
5	2 42	37 46	96
6	1 34	41 20	62
7	0 35½	45 24	24
8	1 30 E	48 52	89 A
9	3 30	51 4	220
10	4 20	54 22	307
11	4 49	56 11	367
12	5 15	58 41	443
13	6 0	60 4	537
14	6 47	62 4	664
15	7 24	63 33	778
16	8 16	64 46	924
17	8 46	65 44	1030

SECTION 29.

Pole.	Vert. ∠'s at T.	Bearings at V.	Diff. of Alt.
	° ′	° ′	
1	8 49 D	26 9	182 D
2	6 51	30 7	163

	° ′	° ′	
3	4 27	38 45	140
4	2 55	44 0	106
5	1 30	49 34	62
6	0 10	52 56	3
7	0 42 E	55 55	42 A
8	2 12	58 28	132
9	3 3	60 58	195
10	3 36	63 32	247
11	4 4	66 16	303
12	4 8	68 28	331
13	4 27	70 9	377
14	5 0	71 54	450
15	5 7	74 23	506

SECTION 30.

Pole.	Vert. ∠'s at T.	Bearings at V.	Diff. of Alt.
	° ′	° ′	
1	3 26 E	81 0	353 A
2	3 10	79 53	313
3	2 55	77 7	263
4	2 46	75 2	233
5	2 35	72 45	204
6	2 26	70 16	179
7	1 20	66 28	91
8	0 19	62 32	23
9	0 49 D	57 58	37 D
10	2 33	51 54	107
11	4 23	43 37	151
12	6 21	35 48	176
	° ′	° ′	
13	8 3	29 13	143
14	10 2	21 54	170

SECTION 31.

Pole.	Vert. ∠'s at Y.	Bearings at V.	Diff. of Alt.
	° ′	° ′	
1	11 18 D	10 40	132 D
2	5 29	15 43	93
3	2 56	19 13	60
4	1 42	22 49	40
5	0 42	28 23	20
6	0 16	31 43	5
7	0 30 E	36 10	28 A
8	1 33	40 41	90
9	2 13	45 0	146
10	2 41	47 18	190
11	2 54	50 47	231
12	3 13	53 12	278
13	3 39	56 0	349
14	4 53	58 51	519
15	5 42	60 38	650
16	6 0	62 8	728
17	6 27	63 23	825
18	6 41	64 21	892
19	6 55	65 50	988

SECTION 32.

Pole.	Vert. ∠'s at Y.	Bearings at V.	Diff. of Alt.
	° ′	° ′	
1	7 9 E	51 46	812 A
2	6 55	50 50	751
3	6 7	49 45	631
4	5 0	46 51	452
5	4 25	45 30	377
6	3 29	43 37	275
7	3 5	40 32	214
8	2 17	38 26	146
9	1 21	35 57	80
10	0 52	33 0	47
11	0 3	29 47	7
12	1 3 D	26 24	33 D
13	2 3	23 40	60
14	3 21	18 30	73
15	7 54	13 4	121

SECTION 33.

Pole.	Vert. ∠'s at T.	Bearings at Z.	Diff. of Alt.
	° ′	° ′	
1	13 6 D	4 6	108 D
2	9 15	8 4	139
3	7 32	14 53	187
4	5 10	21 28	167
5	3 55	26 54	148
6	2 36	36 19	119
7	1 30	45 32	78
8	0 40	56 5	37
	° ′	° ′	
9	0 28 E	65 38	38 A
10	1 40	71 15	132
11	2 22	77 3	197
12	2 25	87 12	226
13	2 32	93 33	257

SECTION 34.

Pole.	Vert. ∠'s at T.	Bearings at Z.	Diff. of Alt.
	° ′	° ′	
1	4 43 E	84 3	471 A
2	4 8	79 45	386
3	3 31	76 0	311
4	2 45	71 30	229
5	2 0	66 22	156
6	1 8	60 49	84
7	0 19	53 20	25
8	0 55 D	46 47	46 D
9	1 29	40 15	69
10	1 59	34 52	83
11	3 41	26 15	127
12	5 27	21 55	165
13	6 0	17 16	150
14	9 5	9 56	143
15	14 6	4 32	109

SECTION 35.

Pole.	Vert. ∠'s at T.	Bearings at Z.	Diff. of Alt.
	° ′	° ′	
1	15 16 D	1 58	86 D
2	9 36	5 3	126

3	8° 21′	8° 5′	162
4	7 34	12 54	208
5	5 38	17 50	191
6	4 21	22 48	170
7	3 12	32 56	153
8	2 31	41 54	135
9	1 42	54 12	103
10	1 10	64 26	76
11	0 52	75 48	61
12	0 30	86 54	36
13	0 14	94 17	15

SECTION 36.

Pole.	Vert. ∠'s at U.	Bearings at Z.	Diff. of Alt.
1	17° 42′ D	2° 45′	60 D
2	15 55	22 26	465
3	12 39	25 13	404
4	8 51	29 37	333
5	7 23	33 36	318
6	3 51	40 33	205
7	1 22	44 45	80
8	1 3 E	48 26	77 A
9	2 54	50 52	218
10	4 22	52 45	347
11	5 37	54 40	471
12	6 30	55 30	559

SECTION 37.

Pole.	Vert. ∠'s at U.	Bearings at Z.	Diff. of Alt.
1	3° 53′ E	63° 50′	345 A
2	3 27	60 54	286
3	1 55	57 45	150
4	0 16	54 3	23
5	1 14 D	50 40	74 D
6	2 53	47 28	166
7	4 45	43 0	247
8	6 25	38 49	300
9	8 40	33 14	348
10	12 6	28 25	420
11	16 36	23 43	419

SECTION 38.

Pole.	Vert. ∠'s at U.	Bearings at Z.	Diff. of Alt.
1	15° 9′ D	20° 37′	405 D
2	11 30	31 39	438
3	9 23	36 7	398
4	6 46	40 30	314
5	5 27	46 10	283
6	3 32	52 5	204
7	2 49	55 9	171
8	1 49	61 25	122

SECTION 39.

Pole.	Vert. ∠'s at U.	Bearings at Z.	Diff. of Alt.
	° ′	° ′	
1	4 28 D	69 7	312 D
2	5 35	59 8	345
3	6 30	52 3	366
4	8 9	45 25	418
5	10 32	38 30	485
6	12 47	34 3	544
7	13 47	29 27	531

SECTION 40.

Pole.	Vert. ∠'s at P.	Bearings at P.	Diff. of Alt.
	° ′	° ′	
1	8 45 D	9 15	735 D
2	8 46	16 25	750
3	9 58	27 16	906
4	8 38	30 52	804
5	7 6	34 30	681
6	5 23	37 55	533

SECTION 41.

Pole.	Vert. ∠'s at O.	Bearings at A.	Diff. of Alt.
	° ′	° ′	
1	10 41 D	23 33	510 D
2	11 31	32 36	769
3	8 52	37 23	682
4	5 38	44 24	524
5	4 52	46 39	480
6	4 6	51 8	455
7	2 8	56 26	269
8	1 21	58 30	178
9	0 57	59 53	129
10	0 43	62 21	103
11	0 40	64 35	102
12	0 5	68 41	10
13	0 10 E	69 43	36 A
14	0 32	71 20	109

SECTION 42.

Pole.	Vert. ∠'s at M.	Bearings at S.	Diff. of Alt.
	° ′	° ′	
1	0 17 D	73 48	0
2	3 25 E	80 57	72 A
3	4 52	85 4	126
4	6 52	88 53	232
5	8 33	90 25	332
6	10 2	91 34	439
7	11 44	93 0	610
8	13 7	94 0	787
9	15 10	94 34	1002
10	17 21	95 13	1292
11	18 38	95 37	1506
12	19 36	96 2	1736
13	19 38	96 0	1727

SECTION 43.

Pole.	Vert. ∠'s at O.	Bearings at A.	Diff. of Alt.
	° ′	° ′	
1	12 3 D	31 52	759 D
2	10 54	36 42	776

	° ′	° ′	
3	8 58	41 8	705
4	7 2	46 9	614
5	5 35	49 34	521
6	3 33	58 6	389
7	2 52	62 38	341
8	2 14	67 20	290
9	2 12	85 34	421

SECTION 44.

Pole.	Vert. ∠'s at R.	Bearings at S.	Diff. of Alt.
	° ′	° ′	
1	3 41 D	6 37	32 D
2	1 18	10 10	21
3	0 51 E	11 21	25 A
4	0 52	12 20	30
5	1 4	16 25	70
6	2 3	16 52	143
7	3 6	17 27	243
8	4 38	18 0	413
9	5 14	18 6	478
10	5 41	18 11	530
11	6 28	18 27	647

SECTION 45.

Pole.	Vert. ∠'s at A.	Bearings at R.	Diff. of Alt.
	° ′	° ′	
1	2 15 D	30 14	122 D
2	2 0	31 57	116
3	1 22	38 35	100
	° ′	° ′	
4	0 8 E	42 19	16 A
5	4 38	49 48	517
6	5 12	50 45	598
7	5 58	52 13	721
8	6 37	54 58	878
9	7 18	56 7	1008
10	7 22	56 39	1037

SECTION 46.

Pole.	Vert. ∠'s at D.	Bearings at C.	Diff. of Alt.
	° ′	° ′	
1	8 50 D	90 20	201 D
2	7 24	111 4	260
3	6 36	114 46	272
4	6 9	116 22	275
5	5 24	119 44	298
6	5 4	121 17	315
7	4 16	122 24	292
8	3 27	123 13	255
9	2 23	124 17	197
10	1 17	124 55	113
11	0 44 E	125 50	82 A
12	1 17	126 8	146

SECTION 47.

Pole.	Vert. ∠'s at T.	Bearings at V.	Diff. of Alt.
	° ′	° ′	
1	10 42 D	17 45	152 D
2	6 6	28 48	145
3	3 19	38 30	111

	° ′	° ′	
4	2 4	43 5	79
5	0 44	47 48	30
6	0 30 E	49 46	30 A
7	2 28	52 45	143
8	3 24	54 21	205
9	4 39	59 0	335
10	4 54	62 16	387
11	5 57	64 50	517
12	6 16	65 38	562
13	6 50	67 39	656
14	7 11	69 40	764
15	8 54	80 54	1425
16	9 6	81 53	1641

SECTION 48.

Pole.	Vert. ∠'s at V.	Bearings at Y.	Diff. of Alt.
	° ′	° ′	
1	9 0 D	17 3	164 D
2	7 0	30 56	236
3	3 22	44 36	170
4	1 16	47 23	66
5	0 5	49 52	0
6	2 46 E	58 6	214 A
7	4 57	64 34	457
8	5 55	67 2	585
9	6 21	70 8	696
10	6 30	72 34	772
11	6 34	75 13	857
12	7 41	77 32	1097
13	8 39	79 6	1316
14	8 46	79 34	1361

SECTION 49.

Pole.	Vert. ∠'s at T.	Bearings at C.	Diff. of Alt.
	° ′	° ′	
1	11 56 D	16 58	83 D
2	9 12	35 25	120
3	8 14	64 20	190
4	8 12	80 0	250
5	5 55	92 52	234
6	4 14	98 36	193
7	1 58	103 12	101
8	0 34 E	106 12	39
9	1 20	108 26	92
10	2 15	109 6	157
11	3 7	110 35	230
12	3 49½	111 48	300

SECTION 50.

Pole.	Vert. ∠'s at L.	Bearings at W.	Diff. of Alt.
	° ′	° ′	
1	17 46 D	10 52	226 D
2	17 2	13 33	296
3	14 51	16 2	333
4	10 47	20 13	363
5	9 26	23 0	418
6	8 54	24 37	466
7	8 7	26 16	508

	° ′	° ′	
8	7 8	27 23	507
9	6 17	28 27	507
10	4 55	29 28	451
11	4 30	30 12	490
12	3 51	30 55	433
13	3 2	31 15	357
14	2 35	31 29	315

SECTION 51.

Pole.	Vert. ∠'s at F.	Bearings at F′.	Diff. of Alt.
	° ′	° ′	
1	14 37 D	3 57	73 D
2	2 56	44 8	140
3	1 43	48 47	90
4	1 3	55 5	61
5	1 0	60 58	65
6	0 41	67 26	50
7	0 23	74 28	31
8	0 25 E	82 0	52 A
9	1 53	83 46	225
10	3 17	87 0	423
11	3 32	87 28	461

SECTION 52.

Pole.	Vert. ∠'s at F′.	Bearings at F.	Diff. of Alt.
	° ′	° ′	
1	2 48 E	68 41	187 A
2	3 50	83 0	282
3	6 45	102 0	630
	° ′	° ′	
4	7 6	108 57	728
5	8 4	111 38	863
6	8 37	112 58	943
7	9 19	115 56	1075
8	10 8	118 30	1230
9	10 57	120 40	1393
10	11 30	122 30	1528
11	12 22	124 50	1748
12	13 40	127 12	2075
13	14 3	128 13	2205
14	14 7	128 32	2239

SECTION 53.

Pole.	Vert. ∠'s at F′.	Bearings at F.	Diff. of Alt.
	° ′	° ′	
1	3 11 E	64 3	208 A
2	5 9	71 24	366
3	5 58	75 50	448
4	6 14	84 12	522
5	8 51	91 23	821
6	9 0	97 47	924
7	10 7	99 40	1074
8	11 2	102 33	1236
9	11 45	104 44	1375
10	12 6	106 28	1469
11	12 43	108 24	1612
12	12 55	109 18	1673

SECTION 54.

Pole.	Vert. ∠'s at t'.	Bearings at F'.	Diff. of Alt.
	° ′	° ′	
1	0 22 E	14 7	12 A
2	1 27	20 5	44
3	5 13	39 2	241
4	5 46	45 18	299
5	6 9	55 40	379
6	6 38	62 0	451
7	7 11	70 8	553
8	7 30	78 56	662
9	7 48	84 30	755
10	7 50	94 8	908
11	7 54	98 26	1006
12	7 58	100 58	1077

SECTION 55.

Pole.	Vert. ∠'s at t'.	Bearings at F'.	Diff. of Alt.
	° ′	° ′	
1	2 0 E	11 46	35 A
2	2 3	22 45	62
3	4 3	25 32	130
4	7 32	34 49	324
5	9 15	42 55	497
6	9 52	48 8	604
7	10 30	53 40	736
8	10 33	62 34	917
9	11 7	66 12	1060
10	11 57	69 15	1235
11	11 52	73 19	1370

SECTION 56.

Pole.	Vert. ∠'s at t'.	Bearings at F'.	Diff. of Alt.
	° ′	° ′	
1	3 11 E	9 4	41 A
2	4 27	16 21	97
3	7 15	21 33	205
4	9 12	27 0	331
5	10 30	29 15	414
6	11 18	31 14	480
7	11 26	35 39	569
8	12 21	38 50	686
9	12 42	41 44	777
10	13 18	44 21	887
11	13 32	46 36	971

SECTION 57.

Pole.	Vert. ∠'s at k.	Bearings at t'.	Diff. of Alt.
	° ′	° ′	
1	7 51 E	29 43	176 A
2	11 24	34 13	299
3	13 22	37 54	397
4	14 52	42 20	509
5	16 40	47 44	675
6	16 46	50 15	833

SECTION 58.

Pole.	Vert. ∠'s at k.	Bearings at t'.	Diff. of Alt.
	° ′	° ′	
1	0 3 E	5 28	5 A
2	6 10	8 5	44

3	10° 3′	13° 52′	121
4	10 20	29 37	313
5	14 20	32 18	497
6	15 0	33 18	517

SECTION 59.

Pole.	Vert. ∠'s at M′.	Bearings at h.	Diff. of Alt.
1	28° 16′ D	Not seen.	
2	25 20	66 56	912 D
3	22 43	74 52	1056
4	22 12	76 20	1114
5	21 40	79 10	1232
6	21° 20′	83° 31′	1521
7	20 14	85 50	1656
8	19 12	87 30	1758
9	18 45	89 17	1958
10	17 30	91 20	2016
11	16 8	91 5	1990
12	14 42	92 27	2093
13	13 35	92 54	2035
14	12 46	93 14	1991
15	11 30	93 45	1915
16	10 50	94 7	1845

The following are the irregular sections. In the first column is the number of poles; in the second the vertical angles; in the third and fourth the two bearings or horizontal angles at each end of the base; and in the fifth the computed result, being the difference of altitude between the foot of each pole and the point mentioned in the second column where the vertical angles were taken.

SECTION 60.

Pole.	Vert. ∠'s at H.	Bear. at H.	Bear. at G.	Diff. of Alt.
1	8° 30′ E	70° 55′	66° 39′	832 A
2	8 30	66 46	70 33	850
3	8 30	62 56	75 9	884
4	8 30	58 20	79 20	892
5	8 30	54 27	82 42	891

SECTION 61.

Pole.	Vert. ∠'s at H.	Bear. at H.	Bear. at G.	Diff. of Alt.
1	9° 31′ E	68° 51′	71° 0′	1004 A
2	9 31	64 19	77 50	1091
3	9 31	60 19	80 38	1072
4	9 31	55 40	84 34	1066

SECTION 62.

Pole.	Vert. ∠'s at H.	Bear. at H.	Bear. at G.	Diff. of Alt.
1	11° 0′E	72° 45′	72° 40′	1334A
2	11 0	69 57	76 21	1389
3	11 0	62 40	82 29	1376
4	11 0	59 7	84 22	1327

SECTION 63.

Pole.	Vert. ∠'s at H.	Bear. at H.	Bear. at G.	Diff. of Alt.
1	12° 15′E	74° 9′	74° 0′	1613A
2	12 15	70 14	78 7	1652
3	12 15	67 32	80 52	1669
4	12 15	64 2	84 1	1664

SECTION 64.

Pole.	Vert. ∠'s at W.	Bear. at W.	Bear. at I.	Diff. of Alt.
1	5° 57′E	74° 37′	81° 12′	382A
2	6 57	78 12	76 36	333
3	7 11	81 12	73 19	329
4	6 19	86 33	68 34	394
5	7 5	92 16	64 4	334
6	7 48	96 43	60 49	271
7	8 40	100 10	58 40	184
8	8 52	102 42	56 41	167
9	8 41	106 0	54 23	168
10	8 30	108 53	52 16	176

SECTION 65.

Pole.	Vert. ∠'s at b.	Bear. at b.	Bear. at a.	Diff. of Alt.
1	2° 1′D	47° 47′	19° 56′	20D
2	1 15E	32 1	26 45	26A
3	2 59	27 0	32 56	65
4	4 35	23 40	45 18	118
5	5 0	19 2	63 47	150
6	5 39	15 26	98 12	202
7	6 1	13 3	121 47	238
8	5 44	9 29	143 12	247

SECTION 66.

Pole.	Vert. ∠'s at d.	Bear. at d.	Bear. at c.	Diff. of Alt.
1	14° 25′D	162° 37′	11° 30′	870D
2	15 55	158 15	13 36	823
3	16 57	152 52	16 35	827
4	18 12	149 43	18 0	831
5	20 57	147 3	18 41	866
6	23 5	136 18	23 37	865
7	23 40	129 0	27 23	876
8	23 3	122 16	31 32	877
9	23 46	112 59	36 25	894
10	23 23	104 22	41 34	891
11	23 35	91 13	48 53	892
12	21 21	82 49	57 38	902
13	20 11	77 33	63 0	897
14	18 47	69 33	71 24	890
15	17 31	65 26	77 38	893

	° '	° '	° '	
16	16 1	61 51	83 57	884
17	14 33	59 1	89 55	875
18	12 48	56 49	96 15	868
19	11 38	54 45	100 45	850
22	10 0	32 45	65 51	841
23	9 22	29 27	71 0	819

SECTION 67.

Pole.	Vert. ∠'s at P.	Bear. at P.	Bear. at H.	Diff. of Alt.
	° '	° '	° '	
1	5 42D	47 54	61 12	4761D
2	7 42	54 10	53 56	592
3	7 13	55 23	52 22	542
4	7 12	58 40	50 36	532
5	8 1	63 52	46 43	564
6	6 47	68 29	44 48	469
7	5 27	71 25	43 18	369
8	5 31	79 14	40 16	368
9	4 15	81 17	39 11	278
10	3 51	87 37	36 34	247
11	3 48	90 58	35 16	242
12	1 5	96 47	32 56	64
13	1 27	96 53	30 46	80
14	3 42	91 16	32 0	208
15	6 0	83 54	34 35	348
16	7 21	69 39	38 26	435
17	9 5	64 1	41 18	564
18	9 34	55 56	44 56	625
19	10 1	48 45	49 59	706
20	9 24	41 45	54 41	703
21	9 52	37 19	59 16	777

SECTION 68.

Pole.	Vert. ∠'s at P.	Bear. at P.	Bear. at G.	Diff. of Alt.
	° '	° '	° '	
1	6 23D	21 51		
2	6 20	22 44	139 42	1140D
3	6 20	24 2	136 17	1092
4	6 14	25 20	132 35	1025
5	5 54	27 20	127 52	932
6	5 41	29 23	121 57	843
7	5 21	32 27	115 58	769
8	5 29	35 59	107 50	740
9	5 45	41 31	87 8	615
10	5 30	45 10	82 44	578
11	5 42	49 50	77 53	589
12	5 56	53 57	74 41	613
13	6 3	58 6	70 2	605
14	5 4	64 0	66 29	510
15	5 8	67 27	65 5	527
16	4 11	73 46	65 27	486
17	4 20	76 52	64 11	518
18	4 21	80 26	62 58	542

SECTION 69.

Pole.	Vert. ∠'s at H.	Bear. at H.	Bear. at W.	Diff. of Alt.
	° '	° '	° '	
1	12 47E	116 58	51 34	1809A
2	14 36	107 42	59 25	2135

	° ′	° ′	° ′	
3	13 56	101 11	65 9	2023
4	13 43	96 35	69 7	1960
5	13 9	92 53	72 30	1875
6	12 38	88 41	76 6	1760
7	12 28	82 53	81 18	1703
8	12 3	78 24	85 45	1656
9	11 47	76 54	87 30	1646
10	11 26	74 56	89 25	1592
11	10 6	68 32	96 23	145[illegible]

The three following ſections were taken in a manner different from all the reſt. They were made by meaſuring in a ſtreight ſloping line (or nearly ſtreight) from certain points towards K and N, and at the beginning of the line taking the angle of elevation or depreſſion of ſeveral places or points in it, whoſe diſtance from the beginning were meaſured. In theſe caſes each diſtance is the hypothenuſe of a right-angled triangle, and the manner of operation is this, as radius is to the hypothenuſe or meaſured ſlope diſtance, ſo is the ſine of the elevation or depreſſion to the difference of altitude, and ſo is the coſine of the ſame vertical angle to the horizontal diſtance.

SECTION 70, from M′ to K.

Pole.	Slope Diſt.	Vert. ∠'s at M′.	Horiz. Diſt.	Diff. of Alt.
1	463	7° 50½′ D	459	60
2	794	6 54½	788	92
3	992	6 50	985	114
4	1257	5° 28½′	1251	116
5	1455	5 25½	1449	134
6	1824 Ends at K.	5 4	1817	157

SECTION 71, from δ′ to K.					SECTION 72, from δ′ to N.				
Pole.	Slope Dift.	Vert. ∠'s at δ.	Horiz. Dift.	Diff. of Alt.	Pole.	Slope Dift.	Vert. ∠'s at δ′.	Horiz. Dift.	Diff. of Alt.
		° ′					° ′		
1	727	13 39½	706	167	1	528	3 10½	527	38
2	1455	9 44½	1434	242	2	1188	4 30	1185	95
3	1720	9 4½	1699	267	3	1594 Ends at N.	6 8	1585	179
4	1984	7 52	1965	267					
5	2547 Ends at K.	7 3	2528	308					

Befides thefe fections there were many more fingle points, whofe places and relative altitudes were obferved and computed, but it is not neceffary to abftract them all here.

The following plate (Tab. VIII.) has 72 figures anfwering to thefe 72 fections, each to each, according to the numbers. In thefe figures, the line having the letters p, p, p, &c. annexed is the fection line, the letters p, p, &c. denoting the poles; the other line, forming the angle with the fection, is the bafe line; and between them are the degrees and minutes contained in the angle formed by them; at the angular point was obferved the elevation or depreffion of each point p, and the bearings or horizontal angles were obferved at the other end of the bafe, from whence faint lines are drawn to fome of the points p forming with the bafe line thofe horizontal angles.

angles. The baſe and ſection lines in each figure are alſo drawn nearly in the ſame direction as they are in the plan or on the ground, ſuppoſing the top of the paper to be the North, towards which a perſon looks when viewing the ground from the South.

Having finiſhed the computation of the relative altitudes of all the points, the next conſideration is how they are to be applied in determining the attraction of the hill. In whatever manner this laſt mentioned operation may be performed, it is evident, that all the points and ſections with their altitudes muſt be entered in the plan. Wherefore, having accurately conſtructed a large plan of the ground, as before mentioned, containing all the principal lines or baſes, at the extremities of which either vertical or horizontal angles were taken, from them I then determined in this plan the places of all the other points in the ſections, whether vertical, horizontal, or irregular. Theſe places or points were determined by drawing lines from each extremity of the baſe ſo as to form with it angles equal to thoſe which were obſerved on the ground for each correſponding pole; the interſections of theſe lines are the places of the poles, which having marked with a fine dot or point of ink, and written cloſe to each point the proper number expreſſing its relative altitude, and cleaned the paper by rubbing out the lines forming the

 angles

angles by which the points were determined, there remained only the points with the figures expreſſing their altitudes diſtinctly exhibited in the plan (ſee tab. IX.)

It remains now to apply all the foregoing calculations and conſtructions to the determination of the effect of the attraction in the direction of the meridian. And here it ſoon occurred, that the beſt method was to divide the plan into a great number of ſmall parts, which may be conſidered as the baſes of as many vertical columns or pillars of matter into which the hill and the adjacent ground may be ſuppoſed to be divided by vertical planes, forming an imaginary group of vertical columns, ſomething like a ſet of baſaltine pillars, or like the cells in a piece of honey-comb; then to compute the attraction of each pillar ſeparately in the direction of the meridian; and laſtly, to take the ſum of all theſe computed effects for the whole attraction of the matter in the hill, &c. Now the attraction of any one of theſe pillars on a body in a given place may be eaſily determined, and that in any direction, to a ſufficient degree of accuracy, becauſe of the ſmallneſs and given poſition of the baſe; for, on account of its ſmallneſs, all the matter in the pillar may be ſuppoſed to be collected into its axis or vertical line erected on the middle of the baſe, the length of which axis, as the mean altitude of the pillar, is to be eſtimated

from

from the altitudes of the points in the plan which fall within and near the baſe of the pillar: then, having given the altitude of this axis, with the poſition of its baſe, and the matter ſuppoſed to be collected into it, a theorem can eaſily be given by which the effect of its attraction may be computed. But to retain the proper degree of accuracy in this computation, it is evident that the plan muſt be divided into a great number of parts, perhaps not leſs than a thouſand for each obſervatory, in order that they may be ſufficiently ſmall, and by this means forming about two thouſand of ſuch pillars of matter, whoſe attractions muſt be ſeparately computed, as mentioned above. The labour and time neceſſary for ſuch computation, it is evident, would be very great, perhaps not leſs than thoſe employed in all the preceding computations of the ſections, and all the other points and lines concerned in this buſineſs. For this reaſon I was deſirous of obtaining a theorem or method by which the attractions of the ſmall and numerous pillars might be computed with the ſame degree of accuracy, but with leſs expence of labour and time than when computed ſeparately as above mentioned. And in this inquiry the ſucceſs has been equal to my wiſhes, having at length met with a method by which the buſineſs has been effected in perhaps one-fourth or one-fifth of the time

time that would have been required in the other way. This method I have investigated partly from some hints of the honourable HENRY CAVENDISH, F. R. S. and partly from some of my own, which had been communicated to the Astronomer Royal in the years 1774 and 1775: of which method and its investigation I shall now give some account.

Of all the methods of dividing the plan into a great number of small parts, I have found that to be the most convenient for the computation, in which it is first divided into a number of rings by concentric circles, and these again divided into a sufficient number of parts by radii drawn from the common center, that center being the observatory where the plummet is placed on which the effect of attraction is to be computed. By this means the plan is divided into a great number of small quadrilateral spaces, two of the opposite sides of which are small portions of adjacent circles, and the other two are the intercepted small parts of two adjacent radii, as appears by fig. 1. tab. x. in which, for the present, let the circles and their radii be supposed to be drawn at any distances whatever from each other, till it shall appear from the theorem to be investigated what may be the properest distances and positions of those lines. In this figure A is the observatory, AN the meridian, NAE an

2 East-

Eaſt-and-Weſt line, BCDE one of the little ſpaces, and F its center or foot of the axis of the pillar whoſe baſe is BCDE; the figure AWNEA being a horizontal or level ſection through the point A. Join A, F, and with the center A deſcribe the middle circle GFH. Let a denote the length of the axis on the point F, or the mean height of the pillar on the baſe BD; and $s =$ the ſine of the angle of elevation of that pillar as obſerved at A, to the radius 1, or $s = \frac{a}{\sqrt{a^2 + AF^2}}$. Then will the magnitude of that column or its quantity of matter be expreſſed by $\frac{BC + ED}{2} \times BE \times a$, which is ſuppoſed to be all collected into the axis: conſequently, if the attraction of each particle of matter be in the reciprocal duplicate ratio of its diſtance, the attraction of the matter in the pillar, ſo placed on the plummet at A, in the direction of the meridian AN, will be $\frac{BC + ED}{2AF} \times BE \times a \times \frac{s}{a} \times c = \frac{BC + ED}{2AF} \times BE \times sc = \frac{GH}{AF} \times BE \times sc$ nearly, ſuppoſing F to be equally diſtant from BC and ED, and c the coſine of the angle FAN to the radius 1.

But $\frac{GH}{AF} \times c$ is nearly equal to d the difference of the ſines of the angles BAN, CAN, as is thus demonſtrated. Draw GK, FL, HM, perpendicular, and GP parallel to AW; and draw the chord GH. Then AK, AM are the ſines of the angles GAN, HAN, to the radius AF, their difference

being

being KM = GP; also FL is the cosine of FAN to the same radius: consequently GP : FL = $d : c$. But the triangles LFA, PGH are equiangular, and therefore GP : FL = GH : AF. Consequently GH : AF = $d : c$; or $\frac{GH}{AF} \times c = d$. This equation is accurately true when GH is the chord of the arc; and as the small arc differs insensibly from its chord, the same equation is sufficiently near the truth when GH is the arc itself. Substituting now d instead of the quantity $\frac{GH}{AF} \times c$ in the theorem above, it will become BE $\times ds$ for the measure of the attraction of the pillar whose base is BD in the direction AN. Which is as easy and simple an expression for the attraction of a single pillar as can well be desired or expected.

But to make the application of this theorem still more easy to the great number of small pillars concerned in this business, let us suppose BE and d to be constant or invariable quantities, and then it is evident that we shall have nothing more to do but to collect all the s's or sines of elevation of all the pillars into one sum, and then multiply that sum by the constant quantity BE $\times d$, by which there will be produced the measure of the attraction of all the pillars, or of the whole part of the ground on one side of WE. Now BE will be made to become constant, by making the circles equi-distant from one another,

another, or by taking the radii in arithmetical progreſſion. And *d* will be conſtant, by drawing the radii ſo as to form with AN angles whoſe ſines ſhall be in arithmetical progreſſion; for then *d* is the common difference of the ſines of thoſe angles. Hence then we are eaſily led to the beſt manner of dividing the plan into the ſmall ſpaces, *viz.* from the center A deſcribe a ſufficient number of concentric and equi-diſtant circles; divide the radius AI of any one of them into a ſufficient number of equal parts, and from the points of diviſion erect perpendiculars to meet the circle; then through the points of interſection draw radii, and they will divide the circles in the manner required.

In a computation of this kind, we need only calculate the attraction of the matter above the plane or horizon of each obſervatory, and the attraction of ſo much matter as is wanting to fill up the vacuity below that plane lying between it and the ſurface of the lower part of the hill. For the South obſervatory, the attraction of the Southern parts that are above it muſt be ſubtracted from that of the Northern parts, to obtain the attraction of the whole towards the North; that is, the Southern elevations are negative, and the Northern ones affirmative. The contrary names take place with reſpect to the depreſſions, or the vacuities below the plane of the obſervatory; for if the

the whole ſpace below this horizontal plane were full of matter to an equal extent both ways, its attraction need not be computed, as thoſe on the contrary ſides would mutually balance each other; but ſince there are unequal vacuities on each ſide, it is evident, that the attraction of the matter that might be contained in them muſt be deducted from the other two equal quantities, to leave the real attraction of thoſe two ſides; then ſubtracting the remainder to the South ſide from that of the Northern ſide, there will at laſt remain the joint effect of all the matter below the plane in the Northern direction: but as the one remainder is to be ſubtracted from the other, the two equal quantities may be omitted in both, and only the effects of the vacuities brought into the account, which being twice ſubtracted, their ſigns become contrary to thoſe of the parts above the horizontal plane; that is, the effect of the Southern vacuity is affirmative, and that of the Northern one negative. But for the Northern obſervatory, when the attraction towards the South is to be found, the contrary names take place; that is, in the elevations the Southern parts are affirmative, and the Northern parts negative; but in the vacuities or depreſſions, the Northern parts are affirmative, and the Southern ones negative.

According

According to the foregoing method the plan of the ground was divided into 20 rings by equidiſtant concentric circles, deſcribed about each obſervatory as a center; and each quadrant was divided into 12 parts or ſectors by lines forming, with the meridian, angles whoſe ſines are in arithmetical progreſſion; by which means the ſpace in each quadrant was divided into 240 ſmall parts, making almoſt a thouſand of ſuch parts in the whole round for each obſervatory, or near 2000 for the two obſervatories. This was judged to be a ſufficiently great number of parts to afford a very conſiderable degree of accuracy; or at leaſt that number was as great, and the parts as ſmall, as was well conſiſtent with the degree of accuracy afforded by the number of points whoſe relative altitudes had been determined.

In this diviſion the common breadth of the rings, or the common difference of the radii, is $666\frac{2}{3}$ feet; and the common difference of the ſines of the angles formed by the radii and the meridian is $\frac{1}{12}$th of the radius; and conſequently, thoſe angles are expreſſed in degrees and minutes as here follows, *viz.* 4° 47′, 9° 36′, 14° 29′, 19° 28′, 24° 37′, 30° 0′, 35° 41′, 41° 48′$\frac{1}{2}$, 48° 35′, 56° 26′$\frac{1}{2}$, 66° 26′$\frac{1}{2}$, 90° 0′.

Tab. IX. contains a ſmall plan of the principal and moſt central part of the ground, accurately divided in the

 above

above manner for one of the observatories, namely, the Northern one, with the places of all or most of the points which fall within this part of the ground, accurately laid down and marked with dots, as also such of the included letters as have been before mentioned in this paper.

In this plate RABCD, &c. is the chain of stations around the hill; N and K are the West and East cairns on the extremities of the ridge of the hill; O the Southern observatory, and P the Northern one. Of this kind were made two large plans, one divided for each observatory, from which were estimated the mean altitudes of the pillars erected on the spaces into which they are divided.

These altitudes are easily estimated when several of the points fall near and in the small spaces or bases, especially when they are near the middle of them; but, numerous as the points are, there are evidently many bases in which none at all are contained, nor even near them. This circumstance at first gave me much trouble and dissatisfaction, till I fell upon the following method by which the defect was in a great measure supplied, and by which I was enabled to proceed in the estimation of the altitudes both with much expedition and a considerable degree of accuracy. This method was the connecting together by a faint line all the points which were

of

of the ſame relative altitude: by ſo doing, I obtained a great number of irregular polygons lying within, and at ſome diſtance from, one another, and bearing a conſiderable degree of reſemblance to each other: theſe polygons were the figures of ſo many level or horizontal ſections of the hills, the relative altitudes of all the parts of them being known; and as every baſe or little ſpace had ſeveral of them paſſing through it, I was thereby able to determine the altitude belonging to each ſpace with much eaſe and accuracy. In this eſtimation I could generally be pretty ſure of the altitude to within ten feet, and often within five, which on an average might be about the 100th part of the whole altitude; and when we conſider that the number of ſuch eſtimated altitudes is very great, and that it is probable the ſmall errors among them would nearly balance one another, the defect of thoſe that might be reckoned too little being compenſated by the exceſs in thoſe which might be taken too great, we need not heſitate to pronounce, that the error ariſing from the eſtimation of the altitudes is probably ſtill much leſs than that part.

It was neceſſary to determine theſe altitudes of the pillars, in order to compute the ſines of the angles of elevation ſubtended by them, as the theorem requires the uſe of theſe ſines; and the very eaſy method uſed in

deducing the latter from the former ſhall be explained after we have, as below, regiſtered the altitudes of all the pillars as they were computed. This regiſter conſiſts of ſixteen tables, namely four quadrants of ſpaces in the altitudes, and four in the depreſſions, for each obſervatory, as ſpecified in the titles of them. The numbers are feet, like all the other dimenſions. The numbers on the ſame horizontal line from left to right are ſuch as are all in the ſame ring; and thoſe in one and the ſame vertical column are in the ſame ſector, or between the ſame two radii; the number of the ring, counted from the common center, is written in the left-hand margin; and the number of the vertical column or diſtance of the ſpace or ſector from the meridian, at the top; alſo the radius of each ring, that is, the line from the common center to the middle of the ring is written on the ſame line with it, in the right-hand margin. It may be further remarked, that in ſuch little ſpaces as were cut through by the boundary line between elevations and depreſſions, thereby making but a part of ſuch ſpaces in each of thoſe denominations, each ſpace was accounted as a whole one; but then the mean altitude or depreſſion in each part was diminiſhed in the proportion of the whole ſpace to the part of it ſo included in the boundary. The altitudes and depreſſions are put down firſt with reſpect to

 the

the Southern obſervatory o, and then for the Northern obſervatory P; and in each, the altitudes are placed firſt.

1. Altitudes above o in the N.W. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
1	215	215	215	215	210	205	200	190	170	145	120	75	333⅓
2	605	610	605	600	595	590	580	570	510	450	350	200	1000
3	965	1005	1010	1010	1020	1050	1040	900	810	600	415	220	1667
4	670	680	700	780	800	930	1040	1090	1100	700	480	210	2333
5	280	310	370	450	560	700	830	960	1180	890	545	200	3000
6	20	50	100	110	250	380	525	710	890	950	605	110	3667
7					10	70	120	415	620	780	600	120	4333
8							15	95	390	610	480	35	5000
9									135	310	220	5	5667
10										40	20		6333

2. Altitudes above o in the N.E. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
1	210	205	205	200	195	185	170	155	140	125	105	70	333⅓
2	550	545	540	530	520	510	500	465	430	370	270	130	1000
3	910	840	825	815	800	760	720	680	635	590	500	200	1667
4	645	640	635	640	645	650	675	715	730	700	580	300	2333
5	265	255	265	285	310	350	390	450	460	500	600	280	3000
6	10	12	20	65	100	130	160	180	180	320	460	300	3667
7								15	55	110	150	250	4333
8												50	5000

3. Altitudes above o in the S.W. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii
1												10	333⅓
2												15	1000
3												5	1667
12												40	7667
13												60	8333
14												100	9000
15												200	9667
16	40	120	200	250	280	280	260	170	60			300	10333
17	160	270	360	400	440	450	450	390	270	30		500	11000
18	310	420	500	580	620	650	660	630	500	200		700	11667
19	440	540	620	680	740	800	800	780	600	400	60	800	12333
20	550	650	750	800	900	950	960	960	800	500	200	900	13000

4. Altitudes above o in the S.E. quarter.

Rings.	1	2	3	4	5	6	7	8	Radii.
16	10								10333
17	80	60	50	50	10				11000
18	220	200	180	130	70	30			11667
19	340	300	260	240	170	120	20		12333
20	450	410	380	330	260	180	100	20	13000

5. Depressions below o in the N.W. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
6	70	40	15	5								15	3667
7	250	240	200	150	60	30						40	4333
8	460	450	430	390	280	200	80	10				80	5000
9	700	700	680	630	520	450	340	170	15			120	5667
10	840	830	800	780	650	600	520	380	180	40	70	220	6333
11	960	920	880	850	750	650	630	550	350	250	300	430	7000
12	1100	1000	950	900	820	780	780	780	580	530	500	600	7667
13	1130	1080	980	880	840	800	830	860	780	690	630	640	8333
14	1180	1100	1000	900	900	900	910	940	870	800	700	500	9000
15	1180	1100	1100	1080	1040	1050	1060	1070	1000	870	730	300	9667
16	1100	1100	1100	1100	1100	1140	1150	1150	1120	990	760	160	10333
17	1100	1100	1100	1130	1180	1200	1200	1200	1180	1080	700	80	11000
18	1100	1100	1150	1200	1200	1150	1100	1100	1200	1180	700	100	11667
19	1100	1120	1220	1230	1260	1200	1200	1200	1300	1240	620	60	12333
20	1120	1220	1320	1360	1390	1390	1390	1340	1440	1300	620	50	13000

6. Depressions below o in the N.E. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12.	Radii.
6	70	60	30	10								10	3667
7	260	240	200	150	110	80	30	40	30	10		10	4333
8	450	440	400	350	280	180	100	180	180	190	190	40	5000
9	700	690	680	610	520	400	200	240	290	350	330	200	5667
10	850	870	890	860	770	620	440	300	380	500	450	370	6333
11	1020	1060	1070	1050	980	860	700	520	400	650	600	530	7000
12	1140	1160	1180	1160	1140	1080	950	840	620	720	850	700	7667
13	1200	1190	1200	1220	1240	1250	1160	1050	900	840	950	880	8333
14	1230	1130	1050	1050	1100	1220	1260	1220	1070	950	1020	990	9000
15	1100	960	900	850	900	1100	1230	1210	1170	1060	1090	1100	9667
16	970	860	880	780	780	900	1120	1180	1200	1180	1160	1150	10333
17	970	800	760	750	750	780	1000	1200	1300	1240	1200	1100	11000

7. Depressions below o in the S.W. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
1	165	165	160	155	150	140	130	120	110	90	50	10	333
2	400	390	380	370	350	330	300	270	240	210	160	60	1000
3	600	580	560	530	500	570	540	400	370	340	280	100	1667
4	740	720	700	670	640	610	580	530	490	440	370	160	2333
5	800	800	800	770	740	710	660	610	570	510	440	230	3000
6	780	790	780	770	780	800	790	700	650	590	510	320	3667
7	700	710	720	730	750	750	750	750	730	670	600	400	4333
8	580	590	600	610	640	660	700	720	730	730	760	520	5000
9	490	490	490	480	490	510	600	650	660	690	580	450	5667
10	470	460	420	400	420	420	440	490	580	590	560	430	6333
11	340	340	340	340	340	330	350	390	450	480	380	370	7000
12	210	220	230	250	250	250	280	310	340	370	250	200	7667
13	160	150	140	120	130	150	200	230	280	290	230	110	8333
14	110	90	60	20	20	20	70	150	230	240	150	90	9000
15	50	20						40	140	180	90	50	9667
16									30	90	70	20	10333

8. Depressions below o in the S.E. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
1	165	165	160	155	150	140	130	120	110	100	95	40	333
2	400	400	400	400	400	390	380	360	330	300	250	110	1000
3	600	610	610	610	610	600	600	580	550	500	440	200	1667
4	760	750	740	740	740	730	720	710	680	640	560	300	2333
5	800	800	800	800	800	800	800	800	790	740	660	400	3000
6	780	780	780	780	780	790	800	840	880	850	750	470	3667
7	700	690	680	670	670	680	620	720	820	900	770	520	4333
8	580	570	570	570	570	580	590	600	660	800	800	600	5000
9	490	490	490	490	490	490	490	500	520	700	880	600	5667
10	470	460	450	440	430	420	410	430	470	530	880	680	6333
11	340	330	320	320	320	320	330	350	420	500	780	780	7000
12	210	200	200	200	210	220	240	280	390	480	680	900	7667
13	120	120	130	130	140	150	180	230	300	450	600	990	8333
14	110	110	110	120	130	150	160	200	280	440	580	980	9000
15	70	70	70	70	90	120	140	170	240	420	570	990	9667
16	10	20	30	40	50	80	120	160	220	400	550	1000	10333
17					10	40	90	140	200	340	540	950	11000
18						5	40	110	170	300	500	850	11667
19								50	150	280	470	780	12333
20								20	150	250	400	700	13000

9. Altitudes above P in the N.W. quarter.

Rings.		12	Radii.
4		10	2333
5		15	3000
6		15	3667
7		15	4333
8		15	5000

10. Altitudes above P in the N.E. quarter.

Rings		12	Radii
1		10	333⅓
2		10	1000
3		15	1667
4		60	2333
5		40	3000
6		5	3667

11. Altitudes above P in the S.W. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii
1	110	110	105	105	100	100	95	95	90	85	80	35	333⅓
2	340	330	320	310	300	290	280	270	240	210	170	90	1000
3	660	660	660	660	660	650	620	590	570	510	370	170	1667
4	1020	1030	1040	1050	1060	1070	1030	990	910	800	660	270	2333
5	1030	1110	1280	1270	1320	1330	1310	1280	1270	1170	910	460	3000
6	670	770	810	900	930	950	1020	1070	1150	1270	1100	600	3667
7	280	340	420	480	540	570	620	670	720	880	1050	660	4333
8	20	50	90	140	210	290	350	420	490	570	700	570	5000
9							120	210	270	320	370	270	5667
10									90	150	180	20	6333
15									140	170			9667
16									140	470	30		10333
17									130	500	170	170	11000
18								20	170	600	400	400	11667
19	150	150	140	120	90	60	30	70	170	500	500	500	12333
20	160	170	210	220	200	170	100	130	170	400	600	600	13000

12. Altitudes above P in the S.E. quadrant.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii
1	110	110	105	105	100	100	95	95	90	85	80	35	333⅓
2	340	330	320	310	300	290	280	270	240	210	170	90	1000
3	660	640	620	600	570	540	510	480	440	380	290	160	1667
4	1000	980	950	910	870	810	750	670	540	460	330	170	2333
5	1020	1020	1020	1030	1030	1020	970	770	570	470	390	130	3000
6	670	710	770	810	840	860	910	890	720	650	400	30	3667
7	290	320	360	390	470	590	700	750	780	600	280		4333
8				20	70	170	250	420	630	550	170		5000
9								110	360	420	170		5667
10									70	180	120		6333
18		20	40	50	40								11667
19	100	100	100	100	80	30							12333
20	130	130	130	120	110	80							13000

13. Depreſſions below P in the N.W. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
1	100	95	90	85	80	75	70	60	50	40	30	15	333⅓
2	390	380	360	330	310	290	270	240	210	180	150	60	1000
3	520	510	500	490	480	470	450	430	410	370	270	80	1667
4	650	640	620	610	590	570	550	530	500	460	390	90	2333
5	830	820	760	720	690	660	630	590	560	500	380	130	3000
6	880	860	850	790	730	700	670	640	580	480	340	280	3667
7	910	900	860	830	790	720	630	620	540	550	440	185	4333
8	930	890	840	800	830	710	610	610	580	530	520	430	5000
9	830	830	830	830	830	830	760	700	670	620	600	330	5667
10	730	740	755	770	785	800	815	830	780	750	720	460	6333
11	730	750	780	800	830	860	860	880	880	860	820	530	7000
12	750	770	810	860	910	930	950	960	950	930	880	580	7067
13	770	840	910	950	990	1030	1050	1050	1030	980	950	650	8333

14. Depressions below P in the N.E. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
1	80	75	70	65	60	55	50	45	40	35	30	20	333⅓
2	330	325	320	300	280	260	240	220	190	150	110	30	1000
3	520	515	505	490	475	460	440	420	400	330	240	60	1667
4	660	675	690	700	700	690	660	620	560	470	350	25	2333
5	840	840	840	840	840	830	820	770	720	620	440	100	3000
6	860	880	900	920	930	930	910	870	830	740	570	240	3667
7	920	920	920	880	880	900	930	940	930	840	680	610	4333
8	920	840	780	780	740	720	770	870	920	970	900	630	5000
9	720	670	600	600	600	560	580	670	850	950	940	600	5667
10	700	620	520	500	500	500	500	520	720	920	960	650	6333
11	700	600	600	600	620	600	580	560	540	840	920	770	7000
12	720	700	680	700	720	740	700	740	570	800	920	820	7667
13	720	720	720	720	700	700	720	720	620	820	900	920	8333

15. Depressions below P in the S.W. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
9	230	140	100	70	40	40							5667
10	400	340	280	230	190	150	110	30					6333
11	500	470	410	340	290	260	230	200	160	30		140	7000
12	500	510	510	490	410	370	330	310	350	280	150	260	7667
13	480	500	500	510	500	460	430	260	150	230	280	360	8333
14	370	390	400	430	450	450	400	210	10	110	280	230	9000
15	260	260	250	260	330	330	310	130			130	130	9667
16	200	200	150	160	170	220	230	130					10333
17	130	130	90	80	90	110	140	30					11000
18	10	20	30	10	10	20	30						11667

16. Depreſſions below P in the S.E. quarter.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12	Radii.
7												80	4333
8	20	30	30									210	5000
9	260	290	290	280	240	150	30					270	5667
10	420	440	450	440	4[illegible]0	370	270	140				330	6333
11	530	540	560	560	550	480	430	330	150	40	40	430	7000
12	500	510	520	550	630	600	500	430	290	230	200	630	7667
13	450	430	420	410	430	570	630	530	430	480	340	710	8333
14	360	330	310	290	280	330	510	670	590	630	5[illegible]0	850	9000
15	240	230	220	200	180	[illegible]00	330	530	770	760	710	870	9667
16	180	160	150	130	110	140	23[illegible]	330	630	830	790	880	10333
17	110	80	50	40	30	90	190	280	500	860	830	860	11000
18	10					10	150	260	400	760	830	760	11667
19							70	230	330	600	770	630	12333
20							10	180	290	530	690	530	13000

It remains now to find the ſines of the vertical angles ſubtended by all the foregoing altitudes and depreſſions, ſince the ſum of theſe ſines is what we are in queſt of. Each altitude or depreſſion is the perpendicular of a right-angled triangle, of which the given radius ſtanding on the ſame line with it in the right-hand margin is the baſe, or other ſide about the right-angle; and by the reſolution of the right-angled triangle, for each perpendicular, the ſame number of correſponding ſines will be found. But with ſuch *data* the tangent of the angle is much eaſier to be found than the ſine, and the analogy for that purpoſe is this, as the baſe : to the perpendicular :: 1 (radius) : the tangent required, which will therefore

fore be found by barely dividing the given perpendicular by the base; and if we find this number in its proper column in a table of sines and tangents, on the same line with it, in the column of sines will be found the sine of the angle required. This seems to be the easiest way of resolving all the triangles when computed separately. But as the labour would be very great in performing so many hundreds of arithmetical divisions, &c. either by logarithms, or by the natural numbers, instead of it, the following method, proposed by the Hon. Mr. CAVENDISH, was adopted, as being a much more expeditious way of obtaining the sum of the sines required. This method consists in finding, in a very easy manner, the difference between each tangent and its corresponding sine, from the given base and perpendicular, and then, subtracting the sum of all the differences from the sum of the tangents, there remains the sum of the sines. Several advantages attend this method of proceeding: for, to find the tangents we need not divide every perpendicular separately by its corresponding base, but add together all the perpendiculars that are on the same line, and divide their sum by their common base, which is the radius of the middle of the ring, and is placed on the same line with them towards the right-hand; for thus we shall have little more than a twelfth part of the number of

divisions

diviſions to perform: alſo a great part of the tangents are ſo ſmall that they do not at all differ from their correſponding ſines in the number of decimals that it is neceſſary to continue the computations to, in all which caſes the trouble of finding the differences is ſaved; and thoſe differences which it is neceſſary to compute, are very readily found by inſpection on a peculiar kind of ſliding rule, which was conſtructed for this purpoſe, and of which I ſhall here give a ſhort deſcription.

This rule (the figure of which is repreſented tab. x. fig. 2.) conſiſts of three columns; one marked AF or baſe, which is moveable by ſliding it up or down by the ſide of the other two which are fixed; of theſe two the one contains the perpendicular altitudes or depreſſions, and the other the differences between the ſines and tangents to the radius 1. To conſtruct the numbers on this rule; form a ſeries of logarithmic tangents in arithmetical progreſſion, of which the firſt term is 9·000, and the common difference ·025; take out from a table the correſponding natural tangents, and place them in the firſt and ſecond columns of baſe and perpendicular, and the difference between the natural ſine and natural tangent in the laſt column, marked Diff. To make uſe of this ſcale; look out any baſe and its correſponding perpendicular in their proper columns, that is, any radius and its cor-

correſponding altitude or depreſſion in the ſixteen foregoing tables, without regarding the number of places they contain, and bring them to correſpond; then, if they conſiſt of the ſame number of places, the lower index on the ſlider or firſt column, or that anſwering to 1000, points to the true difference between the ſine and tangent in the laſt column; but if the number of places in the baſe exceed that in the perpendicular by one, the upper index 100 muſt be uſed. And in this manner were computed all the differences which were neceſſary to be found, and placed in their proper ſquares formed by the meeting of the horizontal and vertical lines, or rings and ſectoral ſpaces, in the following ſet of ſixteen tables, which correſpond to the foregoing ſet of ſixteen, each to each, according to the number of them, and marked at the tops with the numbers 1, 2, 3 &c. to 12 for the ſectoral ſpaces, and with the number of the rings on the left-hand margin. Alſo, in the column immediately after the number of the ring are placed the radii which formed the laſt column in the preceding tables; then, in the third column, are placed the ſums of the altitudes and depreſſions found in each line of the former tables; and, in the next column, the quotients found by dividing the numbers in the third by thoſe in the ſecond column; theſe quotients are the ſums of the tangents

belonging

belonging to each line or ring, which being all added together, their total is placed at the bottom of the column: after this follow the twelve columns of differences before mentioned, which are ſucceeded by one more column containing the ſums of each line of theſe differences, which ſums being added together, their total is placed at the bottom of them; and this total is the ſum of all the differences between the ſines and the tangents, and it is therefore ſubtracted from the total of the tangents in the fourth column, when there remains the ſum of the ſines as required.

1. For the ſum of the ſines of alt. above 0 in the N.W. quarter.

Rings.	Radii.	Sum of Perpen.	3 ÷ 2 = Sum of Tang.	1	2	3	4	5	6	7	8	9	10	11	12	Sum of Diff.
1	$333\frac{1}{3}$	2175	6·525	103	103	103	103	97	97	86	75	55	36	20	5	·883
2	1000	6265	6·265	88	90	88	85	84	82	78	75	56	40	20	4	790
3	1667	10045	6·027	79	86	87	87	88	97	93	64	48	21	7	1	758
4	2333	9300	3·986	12	12	13	17	23	27	39	43	45	16	4		251
5	3000	7275	2·425		1	1	2	3	6	11	16	27	13	3		83
6	3667	4695	1·280						1	1	3	7	9	2		23
7	4333	2735	0·631								1	1	3	1		6
8	5000	1625	0·325										1	1		2
9	5667	670	0·118													
10	6333	60	0·009													
			27·591 = ſum of tangents.												Sum of diff.	2·796
			2·796 = ſum of the diff.													
			24·795 = ſum of the ſines of alt. above 0 in the N.W. quarter.													

2. For the sum of the sines above o in the N.E. quarter.

Rings.	Radii.	Sum of Perp.	3 ÷ 2 = Sum of Tang.	1	2	3	4	5	6	7	8	9	10	11	12	Sum of Diff.
1	333⅓	1965	5·895	97	92	92	87	80	71	56	43	33	24	15	5	·695
2	1000	5360	5·360	68	67	65	62	59	56	53	43	35	23	9	1	541
3	1667	8275	4·965	66	53	51	49	47	41	36	31	25	20	13	1	433
4	2333	7555	3·238	10	10	10	10	10	11	12	14	15	13	8	1	124
5	3000	4410	1·470					1	1	1	2	2	2	4		13
6	3667	1937	·528										1	1		2
7	4333	580	·134													
8	5000	50	·010													
			21·600 = sum of the tangents.													1·808
			1·808 = sum of the differences.													
			19·792 = sum of the sines.													

3. For the sum of the sines above o in the S.W. quarter.

1	333⅓	10	0·030	
2	1000	15	15	
3	1667	5	3	
12	7667	40	5	
13	8333	60	7	
14	9000	100	11	
15	9667	200	21	
16	10333	1960	190	
17	11000	3720	338	
18	11667	5770	495	
19	12333	7260	573	
20	13000	8920	686	
			2·374 = sum	

of the tangents, or sum of the sines, as the diff. between them are nothing in this quadrant.

4. For the sum of the sines above o in the S.E. quarter.

16	10333	10	0·001	
17	11000	230	21	
18	11667	830	71	
19	12333	1450	118	
20	13000	2130	164	
			0·375 = sum	

of the tangents, or of the sines, the diff. being nothing.

5. For the sines below o in the N.W. quarter.

Rings.	Radii.	Sum of Perp.	3÷2 = Sum of Tang.	1	2	3	4	5	6	7	8	9	10	11	12	Sum of Diff.
6	3667	145	0·040													
7	4333	970	224													
8	5000	2370	474													
9	5667	4325	763	1	1	1	1	1								5
10	6333	5910	933	1	1	1	1	1	1							6
11	7000	7520	1·074	1	1	1	1	1	1	1						7
12	7667	9280	1·210	2	1	1	1	1	1	1	1					9
13	8333	10140	1·219	2	1	1	1	1	1	1	1	1				10
14	9000	10700	1·078	1	1	1	1	1	1	1	1	1				9
15	9667	11580	1·198	1	1	1	1	1	1	1	1	1	1			10
16	10333	11970	1·159	1	1	1	1	1	1	1	1	1				9
17	11000	12250	1·105	1	1	1	1	1	1	1	1	1	1			10
18	11667	12280	1·052	1	1	1	1	1	1	1	1	1	1			10
19	12333	12750	1·034			1	1	1	1	1	1	1	1			8
20	13000	13940	1·072			1	1	1	1	1	1	1	1			8
																·101

13·635 = sum of the tangents.
·101 = sum of the differences.
13·534 = sum of the sines.

6. For the sum of the sines below o in the N.E. quarter.

				1	2	3	4	5	6	7	8	9	10	11	12	
6	3667	180	0·049													
7	4333	1160	268													
8	5000	2980	596													
9	5667	5210	919	1	1	1	1	1								5
10	6333	7300	1·153	1	1	1	1	1	1							6
11	7000	9440	1·349	1	2	2	2	1	1	1						10
12	7667	11460	1·495	2	2	2	2	2	1	1	1		1	1		15
13	8333	13080	1·570	1	1	1	1	2	2	1	1	1	1	1	1	14
14	9000	13290	1·477	1	1	1	1	1	1	1	1	1	1	1	1	12
15	9667	12670	1·311	1	1	1		1	1	1	1	1	1	1	1	11
16	10333	12160	1·177		1		1		1		1		1		1	6
17	11000	11850	1·077			1			1		1	1		1	1	6
																·085

12·441 = sum of the tangents.
·085 = sum of the differences.
12·356 = sum of the sines.

7. For the sum of the sines below o in the S.W. quarter.

Rings.	Radii.	Sum of Dep.	3÷2= Sum of Tang.	1	2	3	4	5	6	7	8	9	10	11	12	Sum of Diff.
1	333⅓	1445	4·335	51	51	47	43	40	35	27	21	17	10	2		·344
2	1000	3460	3·460	28	27	26	25	19	17	13	9	6	5	2		177
3	1667	5370	3·222	21	19	17	15	13	18	16	6	5	4	2		136
4	2333	6650	2·850	15	14	13	12	10	8	8	5	5	3	2		95
5	3000	7640	2·547	9	9	9	8	6	6	5	4	3	3	2		64
6	3667	8260	2·253	5	5	5	5	5	5	5	3	3	2	1		44
7	4333	8260	1·906	2	2	2	2	3	3	3	3	2	2	1		25
8	5000	7840	1·568	1	1	1	1	1	1	1	1	2	2	2	1	15
9	5667	6580	1·161	1	1	1	1	1	1	1	1	1	1	1		11
10	6333	5680	897	1							1	1	1	1		5
11	7000	4450	636				1				1				1	3
12	7667	3160	412						1						1	2
13	8333	2190	263										1			1
14	9000	1250	139													
15	9667	570	59													
16	10333	210	20													
			25·728 = sum of the tangents.													·922
			0·922 = sum of the differences.													
			24·806 = sum of the sines.													

8. For the ſum of the ſines below O in the S.E. quarter.

Rings.	Radii.	Sum of Dep.	3÷2= Sum of Tang.	1	2	3	4	5	6	7	8	9	10	11	12	Sum of Diff.
1	333⅓	1510	4·530	51	51	47	43	40	35	27	21	17	13	11	1	·357
2	1000	4120	4·120	28	28	28	28	28	27	25	21	17	13	8	1	252
3	1667	6510	3·906	21	21	21	21	21	21	21	19	16	13	9	1	205
4	2333	8070	3·459	16	16	15	15	15	15	14	14	13	10	6	1	150
5	3000	8990	2·997	9	9	9	9	9	9	9	9	9	8	5	1	95
6	3667	9280	2·531	5	5	5	5	5	5	5	5	6	6	4	1	57
7	4333	8440	1·948	2	2	2	2	2	2	2	2	3	4	3	1	27
8	5000	7490	1·498	1	1	1	1	1	1	1	1	1	2	2	1	14
9	5667	6630	1·170			1		1				1	1	1	1	6
10	6333	6070	958			1			1			1		1	1	5
11	7000	5110	730				1				1		1	1	1	5
12	7667	4210	549						1				1		1	3
13	8333	3540	425								1			1	1	3
14	9000	3370	374								1			1	1	3
15	9667	3020	313										1		1	2
16	10333	2680	259											1	1	2
17	11000	2310	210												1	1
18	11667	1975	169												1	1
19	12333	1730	140												1	1
20	13000	1520	117												1	1

30·403 = ſum of the tangents. 1·190
1·190 = ſum of the differences.
29 213 = ſum of the ſines.

9. For the ſum of the ſines above P in the N.W. quarter.

4	2333	10	·004	
5	3000	15	5	
6	3667	15	4	
7	4333	15	3	
8	5000	15	3	

0 019 = ſum of the tangents, or ſum of the ſines, as they have no difference in this quadrant.

10. For the ſum of the ſines above P in the N.E. quarter.

1	333⅓	10	·030	
2	1000	10	10	
3	1667	15	9	
4	2333	60	26	
5	3000	40	13	
6	3667	5	2	

0·090 = ſum of the tangents, or ſum of the ſines, they being equal in this quadrant.

11. For the ſum of the ſines above P in the S.W. quarter.

Rings.	Radii.	Sum of Alt.	3÷2 = Sum of Tang.	1	2	3	4	5	6	7	8	9	10	11	12	Sum of Diff.
1	333⅓	1110	3·330	17	17	15	15	13	13	12	12	10	8	6	1	·139
2	1000	3150	3·150	18	17	16	14	13	12	11	10	6	5	2	1	125
3	1667	6780	4·068	28	28	28	28	28	27	23	20	18	14	5	1	248
4	2333	10930	4·684	37	38	39	40	41	42	38	34	26	18	7	1	361
5	3000	13740	4·580	18	23	34	33	38	39	37	34	33	26	14	2	331
6	3667	11240	3·065	3	5	5	6	6	7	14	16	19	21	14	2	118
7	4333	7230	1·668			1	1	1	1	1	2	2	4	5	2	20
8	5000	3900	780							1		1	1	1	1	5
9	5667	1560	275											1		1
10	6333	440	70													
15	9667	310	32													
16	10333	640	62													
17	11000	970	88										1			1
18	11667	1590	137										1			1
19	12333	2480	201												1	1
20	13000	3130	241										1		1	2
			26·431 = ſum of the tangents.													1·353
			1·353 = ſum of the differences.													
			25·078 = ſum of the ſines.													

12. For the ſum of the ſines above P in the S.E. quarter.

				1	2	3	4	5	6	7	8	9	10	11	12	
1	333⅓	1110	3·330	17	17	15	15	13	13	12	12	10	8	6	1	·139
2	1000	3150	3·150	18	17	16	14	13	12	11	10	6	5	2	1	125
3	1667	5890	3·534	28	26	23	21	18	16	14	12	9	5	2	1	175
4	2333	8420	3·609	34	33	30	26	24	18	15	12	6	4	1		203
5	3000	9440	3·147	18	18	18	18	18	18	16	9	3	2	1		139
6	3667	8260	2·253	3	4	5	5	5	6	8	7	4	3	1		51
7	4333	5530	1·276			1		1	1	2	3	3	1			12
8	5000	2280	456								1	1	1			3
9	5667	1060	187										1			1
10	6333	370	58													
18	11667	150	13													
19	12333	510	42													
20	13000	700	54													
			21·109 = ſum of the tangents.													0·848
			0·848 = ſum of the differences.													
			20·261 = ſum of the ſines.													

13. For the fum of the fines below P in the N.W. quarter.

Rings.	Radii.	Sum of Dep.	3÷2 = Sum of Tang.	1	2	3	4	5	6	7	8	9	10	11	12	Sum of Diff.
1	333⅓	790	2·370	13	12	9	8	7	5	5	3	2	1	1		066
2	1000	3170	3·170	26	25	21	17	14	12	9	6	5	3	2		140
3	1667	4980	2·988	15	14	13	12	12	11	9	8	7	5	2		108
4	2333	6200	2·657	10	10	9	9	8	7	6	5	5	4	2		75
5	3000	7270	2·423	10	10	8	7	6	5	5	4	3	2	1		61
6	3667	7800	2·127	7	6	5	5	4	3	3	3	2	1	1		40
7	4333	7975	1·840	5	4	4	3	3	2	2	1	1	1	1		27
8	5000	8280	1·656	3	3	2	2	2	1	1	1	1	1			17
9	5667	8660	1·528	2	1	2	1	2	1	1	1	1	1	1		14
10	6333	8935	1·411	1	1	1	1	1	1	1	1	1	1	1		11
11	7000	9580	1·369	1		1		1	1	1	1	1	1	1		9
12	7667	10280	1·341	1		1		1	1	1	1	1	1	1		9
13	8333	11200	1·344		1	1	1	1	1	1	1	1	1	1		10
																0·587

26·224 = fum of the tangents.
0·587 = fum of the differences.
25·637 = fum of the fines.

14. For the fum of the fines below P in the N.E. quarter.

				1	2	3	4	5	6	7	8	9	10	11	12	
1	333⅓	625	1·875	7	5	5	4	3	2	2	1	1	1	1		·032
2	1000	2755	2·755	17	16	15	13	11	9	6	5	3	2	1		98
3	1667	4855	2·913	15	14	13	12	11	10	9	8	6	4	2		104
4	2333	7000	3·000	11	12	13	13	13	13	11	9	6	4	2		107
5	3000	8500	2·833	11	11	11	11	11	10	10	9	7	5	2		98
6	3667	9580	2·613	6	7	7	8	8	8	8	6	5	4	2		69
7	4333	10350	2·389	5	5	5	4	4	4	5	5	5	4	2	1	49
8	5000	11840	2·368	3	2	2	2	2	1	2	3	3	3	3	1	27
9	5667	8400	1·482	1	1	1	1	1	1		1	2	2	2	1	14
10	6333	7610	1·202	1	1		1		1		1	1	1	1	1	9
11	7000	7930	1·133	1		1		1		1			1	1	1	7
12	7667	8810	1·149	1		1		1		1	1		1	1	1	8
13	8333	8980	1·078		1		1		1		1		1	1	1	7
																·629

26·790 = fum of the tangents.
·629 = fum of the differences.
26·161 = fum of the fines.

15. For the sum of the sines below P in the S.W. quarter.

Rings.	Radii.	Sum of Dep.	3 ÷ 2 = Sum of Tang.	1	2	3	4	5	6	7	8	9	10	11	12	Sum of Diff.
9	5667	620	·051													
10	6333	1730	273			1										·001
11	7000	3030	433	1						1						2
12	7667	4470	583		1							1				2
13	8333	4660	559				1				1					2
14	9000	3730	414				1							1		2
15	9667	2390	247							1						1
16	10333	1460	141								1					1
17	11000	800	73													
18	11667	130	11													

2·785 = sum of the tangents. ·011
·011 = sum of the differences.
2·774 = sum of the sines.

16. For the sum of the sines below P in the S.E. quarter.

				1	2	3	4	5	6	7	8	9	10	11	12	
7	4333	80	·018													
8	5000	290	58													
9	5667	1810	319				1									·001
10	6333	3280	518			1				1						2
11	7000	4640	663		1				1						1	3
12	7667	5590	729			1			1						1	3
13	8333	5830	700				1			1					1	3
14	9000	5700	633				1				1			1		3
15	9667	5240	542							1			1		1	3
16	10333	4560	441								1			1		2
17	11000	3920	356									1			1	2
18	11667	3180	273											1		1
19	12333	2630	213											1		1
20	13000	2230	172											1		1

5·635 = sum of the tangents. ·025
·025 = sum of the differences.
5·610 = sum of the sines.

Having now obtained the ſums of the ſines for the ſeveral quadrants, the next buſineſs is to collect them together, and deduct the negatives from the affirmatives. And this may be done either for each obſervatory ſeparately, or for both together. I ſhall do them ſeparately, in order thereby to diſcover alſo the ratio of their effects.

And, firſt, for the Southern obſervatory O.

Affirmatives.			Negatives.		
1 . . 24·795	N.W.	Alt.	3 . . 2·374	S.W.	Alt.
2 . . 19·792	N.E.		4 . . 0·375	S.E.	
7 . . 24·806	S.W.	Dep.	5 . . 13·534	N.W.	Dep.
8 . . 29·213	S.E.		6 . . 12·356	N.E.	
98·606 = ſum of affirm.			28·639 ſum.		

98·606 = ſum of affirm.
28·639 = ſum of negat.

69·967 = effective ſum of the ſines for O.

Secondly, for the Northern obſervatory P.

Affirmatives.			Negatives.		
11 . . 25.078	S.W.	Alt.	9 . . 0·019	N.W.	Alt.
12 . . 20·261	S.E.		10 . . 0·090	N.E.	
13 . . 25·637	N.W.	Dep.	15 . . 2·774	S.W.	Dep.
14 . . 26·161	N.E.		16 . . 5·610	S.E.	
97·137 = ſum of affirm.			8·493		

97·137 = ſum of affirm.
8·493 = ſum of negat.

88·644 = effective ſum of the ſines for P.
69·967 = the ſame for O.

158·611 = the ſum of the ſines for both obſerv.

 From

From these numbers it appears, that the effect of the attraction at the Northern observatory is to that at the Southern one, nearly as 70 is to 89, or as 7 to 9 nearly. This difference is to be attributed chiefly to the effect of the hills on the South of the Southern observatory, which were considerably greater and nearer to it than those on the back of the Northern observatory. For although the Southern observatory was placed 273 feet above the level of the Northern one, which removed it considerably more above the center of gravity of the hill than the latter was, it was at the same time placed considerably nearer than the other to the middle in a horizontal direction; so that probably the one difference nearly balances the other; and accordingly we find that the sum of the affirmative altitudes for O is 44·587, and of those for P 45·339, which differ by only a 45th part nearly.

It only remains now to multiply the sum of the sines by the common breadth of the rings, and by the common difference of the sines of the angles made by the meridian and the several radii. It has already been observed, that the former is $666\frac{2}{3}$, and the latter $\frac{1}{12}$; therefore $\frac{1}{12} \times 666\frac{2}{3} = \frac{2000}{36} = \frac{500}{9}$ is their product: consequently, $158{\cdot}611 \times \frac{500}{9} = 8811\frac{2}{3}$ nearly, is the sum of the two opposite attractions made by the hill, &c. at the two observatories.

In order now to compare this attraction with that of the whole earth, this body may be conſidered as a ſphere, and the obſervatories as placed at its ſurface; ſince the very ſmall differences of theſe ſuppoſitions from the truth, are of no conſequence at all in this compariſon. Now the attraction of a ſphere, on a body at its ſurface, is known to be $= \frac{2}{3} c\, d$, where d is = the diameter of the ſphere, and $c = 3{\cdot}1416$ = the circumference of the circle of which the diameter is 1. But $c\,d$ is = the circumference of the circle to the diameter d; and therefore the attraction of a ſphere will be expreſſed by barely $\frac{2}{3}$ of its circumference; which is a theorem well adapted to the computation in hand. The length of a degree in the mean latitude of 45°, is 57028 French toiſes (ſee p. 327. Phil. Tranſ. 1768): and the ſame reſult nearly is obtained by taking a mean among all the meaſures of degrees there put down, that mean being 57038 toiſes. I ſhall therefore uſe the round number 57030 as probably nearer the truth. This number being multiplied by 6, the product 342180 ſhews the number of French feet in one degree; but, by p. 326. of the ſame volume, the lengths of the Paris and London feet are as 76·734 to 72, that is, as 4·263 to 4; therefore, as 4 : 4·263 :: 342180 : 364678 = the Engliſh feet in one degree; and this being multiplied by 360 the whole number of degrees, there reſults

 131284080

131284080 feet for the whole circumference, which are equal to $24864\frac{1}{2}$ miles, making $69\frac{1}{15}$ to a degreee in the mean latitude. Laſtly, $\frac{2}{3}$ of 131284080 give 87522720 for the meaſure of the attraction of the whole earth.

Conſequently, the whole attraction of the earth is to the ſum of the two contrary attractions of the hill, as the number 87522720 to $8811\frac{2}{3}$, that is, as 9933 to 1 very nearly, on ſuppoſition that the denſity of the matter in the hill is equal to the mean denſity of that in the whole earth.

But the Aſtronomer Royal found, by his obſervations, that the ſum of the deviations of the plumb line, produced by the two contrary attractions, was 11·6 ſeconds. From hence it is to be inferred, that the attraction of the earth is actually to the ſum of the attractions of the hill, nearly as radius to the tangent of 11·6 ſeconds, that is, as 1 to ·000056239, or as 17781 to 1; or as 17804 to 1 nearly, after allowing for the centrifugal force ariſing from the rotation of the earth about its axis.

Having now obtained the two reſults, namely, that which ariſes from the actual obſervations, and that belonging to the computation on the ſuppoſition of an equal denſity in the two bodies, the two proportions compared muſt give the ratio of their denſities, which is

that of 17804 to 9933, or 1434 to 800 nearly, or almoſt as 9 to 5. And ſo much does the mean denſity of the earth exceed that of the hill.

Thus then we have at length obtained the object which we have been in queſt of through the very laborious calculations that have been deſcribed in this paper, and in the ſurvey and meaſurements from which theſe computations were made; namely, the ratio of the mean denſity of all the matter in the earth, in compariſon with the denſity of the matter of which the hill is compoſed. And that ratio we have found to be equal to the ratio of 9 to 5. And, for the reaſons before mentioned, I think we may reſt ſatisfied, that this proportion is obtained to a conſiderable degree of proximity, probably to within the fiftieth part, if not the hundredth part of its true magnitude. Another queſtion, however, ſtill ariſes, namely, what is the denſity of the matter in the hill? Is its mean denſity equal to that of water, of ſand, of clay, of chalk, of ſtone, or of ſome of the metals? For, according to the matter, or different ſorts of matter, of which it is formed, and according as it is conſtituted with or without large vacuities, its mean denſity may be greater or leſs, and that in a degree which is not certainly known. A conſiderable degree of accuracy in this point could only be obtained by a cloſe examination of the internal

ſtructure

ſtructure of the hill. And the eaſieſt method of doing this would be to procure holes to be bored, in ſeveral parts of it, from the ſurface to a ſufficient depth, after the manner that is practiced in boring holes to the coal mines from the ſurface of the ground; for by ſuch operation it is known what kind of ſtrata the borer is paſſed through, together with their dimenſions and denſities. The proper mean among all theſe would be the mean denſity of the hill, as compared to water or to any other ſimple matter; and thence we ſhould obtain the comparative denſity of the whole earth with reſpect to water: but in the preſent inſtance, we muſt be ſatisfied with the eſtimate ariſing from the report of the external view of the hill; which is, that to all appearance it conſiſts of an intire maſs of ſolid rock. It is probable, therefore, that we ſhall not greatly err, if we aſſume the denſity of the hill equal to that of common ſtone; which is not much different from the mean denſity of the whole matter near the ſurface of the earth, to ſuch depths as have actually been explored either by digging or boring. Now the denſity of common ſtone is to that of rain water as $2\frac{1}{2}$ to 1; which being compounded with the proportion of 9 to 5 above found, there reſults the ratio of $4\frac{1}{2}$ to 1 for the ratio of the denſities of the earth and rain water; that

that is to ſay, the mean denſity of the whole earth is about $4\frac{1}{2}$ times the denſity of water.

To what uſeful purpoſes the knowledge of the mean denſity of the earth, as above found, may be applied, it is not my buſineſs here to ſhew. I ſhall therefore put an end to this paper with a reflection or two on the premiſes before delivered. Sir ISAAC NEWTON thought it probable, that the mean denſity of the earth might be five or ſix times as great as the denſity of water; and we have now found, by experiment, that it is very little leſs than what he had thought it to be: ſo much juſtneſs was even in the ſurmiſes of this wonderful man! Since then the mean denſity of the whole earth is about double that of the general matter near the ſurface, and within our reach, it follows, that there muſt be ſomewhere within the earth, towards the more central parts, great quantities of metals, or ſuch like denſe matter, to counterbalance the lighter materials, and produce ſuch a conſiderable mean denſity. If we ſuppoſe, for inſtance, the denſity of metal to be 10, which is about a mean among the various kinds of it, the denſity of water being 1, it would require ſixteen parts out of twenty-ſeven, or a little more than one-half of the matter in the whole earth, to be metal of this denſity, in order to compoſe a maſs of ſuch mean denſity as we have found the earth to poſſeſs by

our experiment: or $\frac{4}{15}$, or between $\frac{1}{3}$ and $\frac{1}{4}$ of the whole magnitude will be metal; and consequently $\frac{20}{31}$, or nearly $\frac{2}{3}$ of the diameter of the earth, is the central or metalline part.

Knowing then the mean density of the earth in comparison with water, and the densities of all the planets relatively to the earth, we can now assign the proportions of the densities of all of them as compared to water, after the manner of a common table of specific gravities. And the numbers expressing their relative densities, in respect of water, will be as below, supposing the densities of the planets, as compared to each other, to be as laid down in Mr. DE LA LANDE'S astronomy.

Water . . .	1
The Sun . .	$1\frac{2}{15}$
Mercury . .	$9\frac{1}{6}$
Venus . . .	$5\frac{11}{15}$
The earth . .	$4\frac{1}{2}$
Mars	$3\frac{2}{7}$
The Moon . .	$3\frac{1}{11}$
Jupiter . . .	$1\frac{1}{24}$
Saturn . . .	$\frac{13}{32}$

Thus then we have brought to a conclusion the computation of this important experiment, and, it is hoped, with no inconsiderable degree of accuracy. But it is the

first

first experiment of the kind which has been so minutely and circumstantially treated; and first attempts are seldom so perfect and just as succeeding endeavours afterwards render them. And, besides, a frequent repetition of the same experiment, and a coincidence of results, afford that firm dependance on the conclusions and satisfaction to the mind, which can scarcely ever be had from a single trial, however carefully it may be executed. For those reasons it is to be wished, that the world may not rest satisfied barely with what has been done in this instance, but that they will repeat the experiment in other situations, and in other countries, with all the care and precision that it may be possible to give to it, till an uniformity of conclusions shall be found, sufficient to establish the point in question beyond any reasonable possibility of doubt. What has been already done in the present case will render any future repetition more easy and perfect. But improvements may be made, perhaps both in the mode of computation and in the survey; in the latter, especially, there certainly may. Some improvements of this kind I have hinted at in some parts of this paper, which with others I shall here collect together, that they may readily be seen in one point of view. They are principally these. Procure one base, or more if convenient, very accurately measured, in such situation, that

as many more points as poſſible in the ſurvey may be ſeen from it. Aſſume as many principal or eminent points and objects as may be proper and convenient; and from each one of them meaſure the angles formed by all the reſt that can be ſeen, both horizontal and vertical angles, and repeat theſe obſervations, if convenient, with the inſtrument varied or reverſed, taking the means among the ſeveral quantities of each angle. Take then as many ſections of the ground, and as far extended in all directions, as the time and circumſtances will poſſibly admit. Of the ſections, thoſe that are horizontal or level are the beſt, as they require no calculation; procure therefore as many as poſſible of them. In vertical ſections obſerve the vertical angles, not in the plane of the ſection, but at ſome other point of which the bearing is alſo taken from the beginning of the ſection line, and where the horizontal angles of the poles are taken, for the reaſons before mentioned in p. 723. And it will be a ſtill farther convenience if the ſection be made in ſuch direction as to form a right angle with the line drawn to the point or ſtation from whence the vertical angles of the poles are obſerved, as may be ſeen from what is ſaid in p. 721. It might, perhaps, be proper to make ſome experiments on a valley inſtead of a hill, taking two obſervatories at the two oppoſite ſides of it, both for the

greater

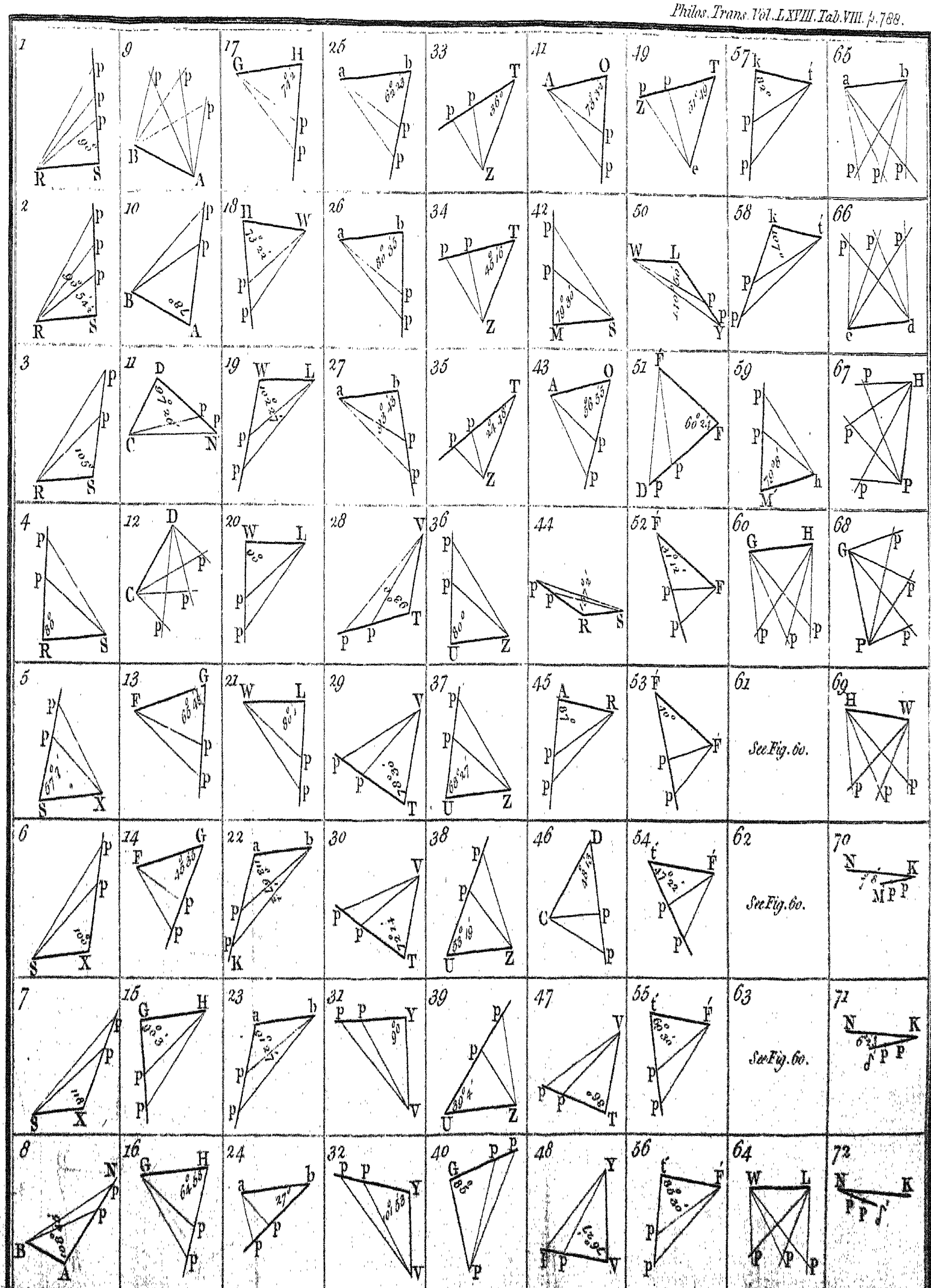
greater variety in this interefting problem, and becaufe alfo the furvey would be more eafily made, on account of the ground being more in view at each ftation than in the cafe of a hill, which generally hides more than half the compafs from the obferver. In computing the relative altitudes of all the principal ftations, let the operations be performed mutually both backwards and forwards, that is, from both of every two objects, having for that purpofe obferved at each of them the vertical angle of the other, namely, both the angle of elevation and the angle of depreffion, and take the mean between the two computed differences of altitude; for this excludes the neceffity of making the proper allowances for refraction, and for the curvature of the earth; fince the effect of each of thefe is balanced and corrected by that of the counter obfervation. But as to thofe points in the fections which are far diftant from the obferver, and where great accuracy is required, it may be proper to make the allowance for refraction and curvature, as there is generally no back obfervation by which their effects may be balanced. Thefe are the chief hints which at prefent occur to me, befides the general information to be derived by the computer from the perufal of the modes of computation that have been defcribed in this paper. As to the furveyor, he will ftrike out other

 convenient

convenient ways of meaſurement adapted to the circumſtances with which the nature of the ſurvey may happen to be attended.

A map of the country about Schehallien is hereunto annexed, to convey a general idea of the nature of the ground, and for the better illuſtration of the deſcription given in the former parts of this paper. This map is tab. XI.

Woolwich,
April 27, 1778.



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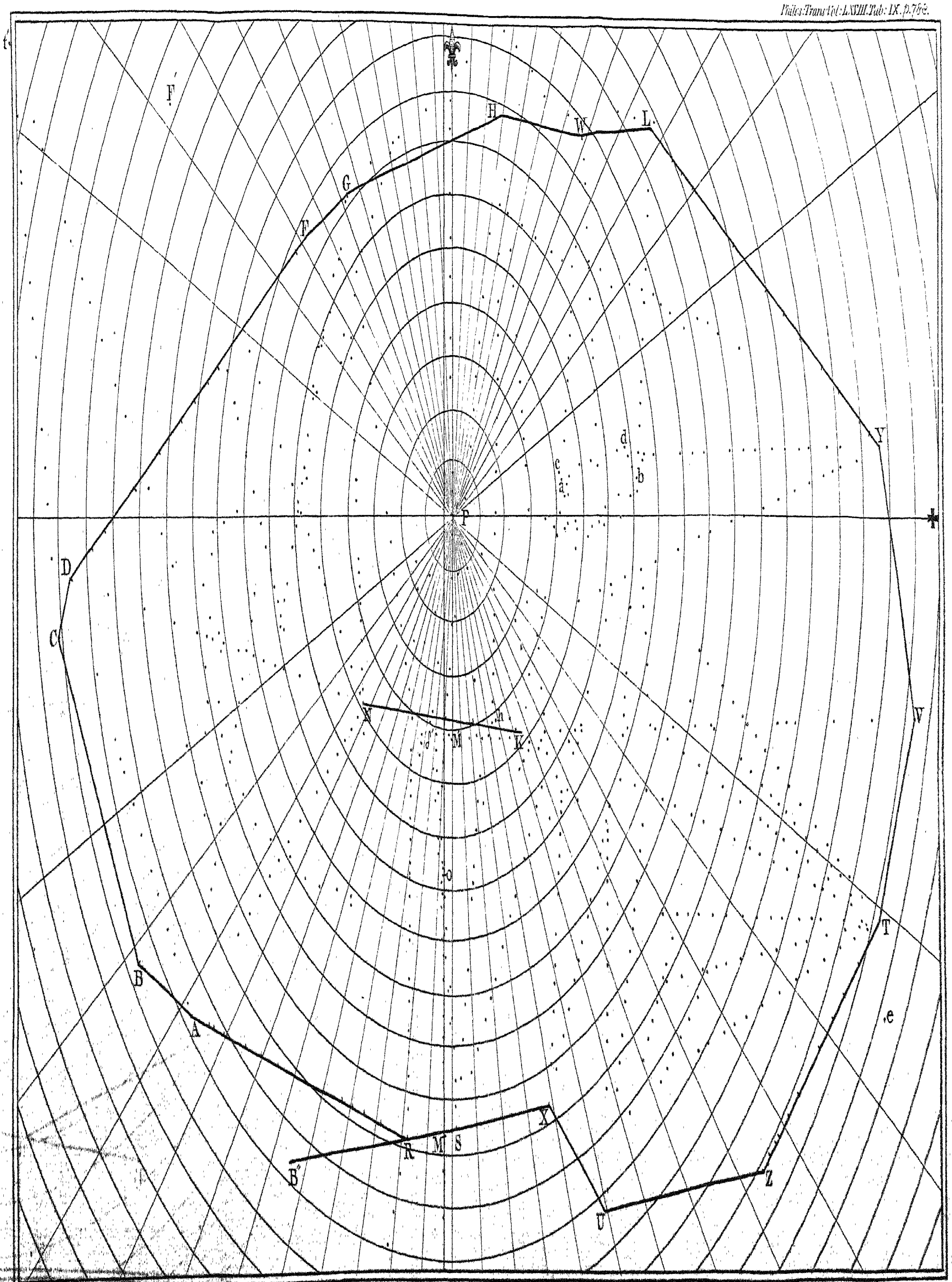

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Fig. 1.

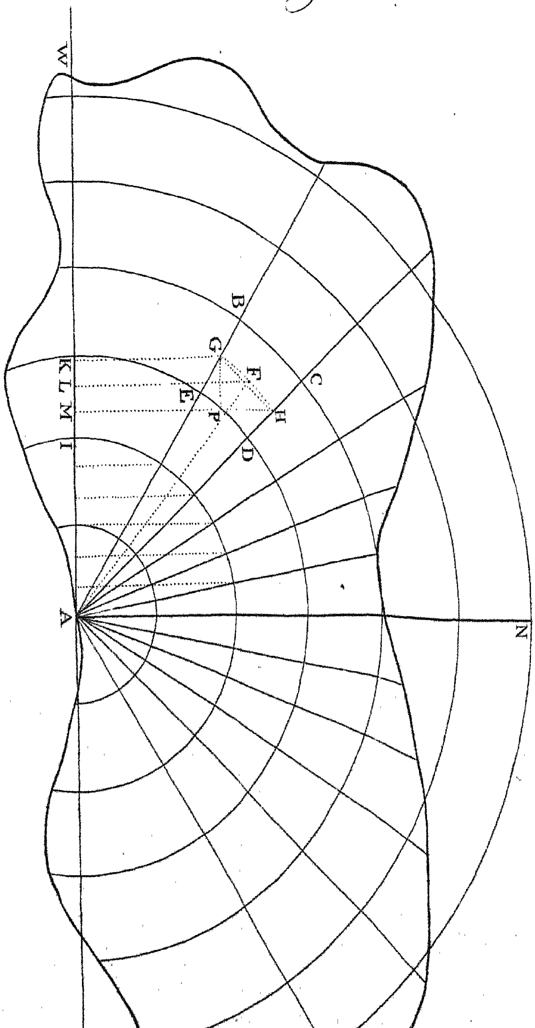
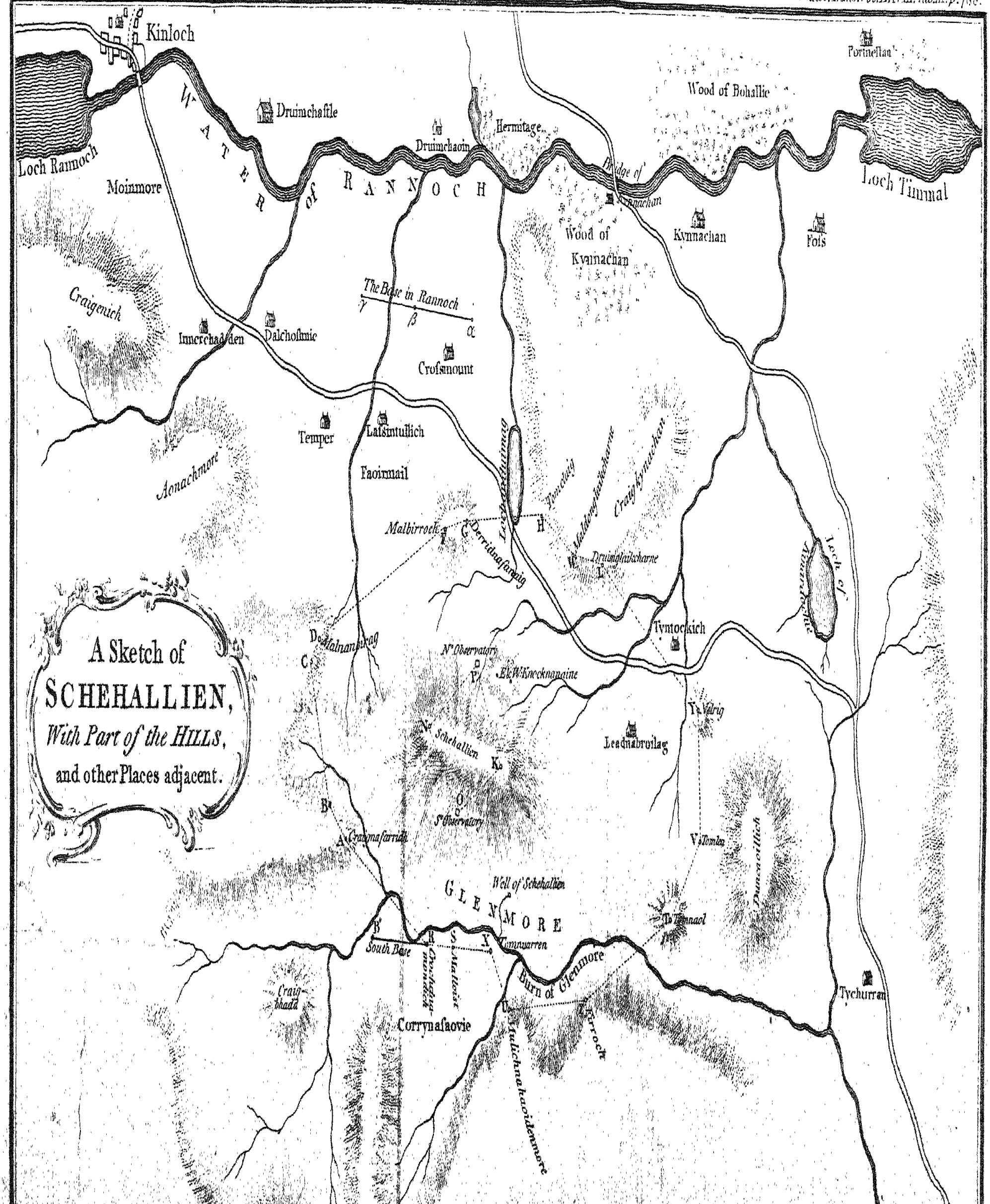


Fig. 2.

A F or Base	Perpendicular	Diff.
100	100	·001
106	106	·001
112	112	·001
119	119	·001
126	126	·001
133	133	·001
141	141	·001
150	150	·002
159	159	·002
168	168	·002
178	178	·003
188	188	·003
199	199	·004
211	211	·005
224	224	·005
237	237	·006
251	251	·008
266	266	·009
282	282	·011
299	299	·013
316	316	·015
335	335	·017
355	355	·020
376	376	·024
398	398	·028
422	422	·033
447	447	·039
473	473	·045
501	501	·053
531	531	·062
562	562	·072
596	596	·084
631	631	·097
668	668	·113
708	708	·130
750	750	·150
794	794	·172
842	842	·198
891	891	·226
944	944	·258
1000	1000	·293

Basire sc.


A Sketch of
SCHEHALLIEN,
With Part of the HILLS,
and other Places adjacent.
Kinloch
Loch Rannoch
Moinmore
WATER of RANNOCH
Druimchastle
Druimchaoin
Hermitage
Wood of Bohallie
Portnellan
Loch Tummal
Wood of Kynnachan
Kynnachan
Fois
Craigenich
The Base in Rannoch
Innerchadden
Dalchosnie
Crossmount
Temper
Lassintullich
Faoirmail
Aonachmore
Malbirrock
Craigkynachan
Tyntockich
Schehallien
Leadnabroilag
GLENMORE
Well of Schehallien
Burn of Glenmore
South Base
Corrynafaovie
Tychurran

XXXIV. *An Account of the Blue Shark, together with a Drawing of the same. By* W. Watson, *Jun. M. D. F. R. S.*

Read June 4, 1778.

THE fish from which the drawing was made was taken on the Coast of Devonshire. It had got into shallow water, by which accident Mr. MARTIN, a very great lover of natural history, and who happened to be on the shore, was enabled to drag it out by the tail, and to kill it on the spot.

LINNÆUS places this animal in the class of amphibia, under the name of *squalus glaucus*, and makes use of ARTEDE's description, viz. *squalus fossula triangulari in extremo dorso, foraminibus nullis ad oculos.* As this fish is well described by RONDELETIUS and others, I shall only subjoin the following remarks. There are two triangular dents at the origin of the tail, both above and below; that which is on the back is biggest and deepest. No orifices are to be seen behind the eyes, as is usual with fishes of this *genus*. Two white membranes, one to each eye, perform the office of eye-lids. They are placed beneath under the external integuments, and

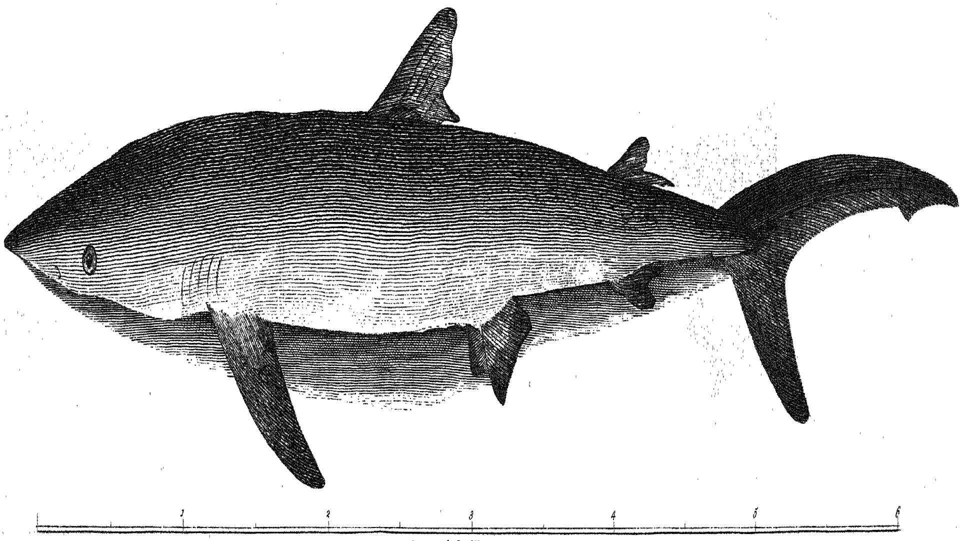
move

move upwards when they cover the eyes. It is furniſhed with five rows of teeth; theſe are triangular, and finely ſerrated[(a)]. The body is of a fine blue colour, dark on the back, lighter on the ſides; the belly and all the under part of the fiſh white; the fins and tail were of a dirty blue; the colour of the blue part is exactly repreſented by different ſhades of indigo blue. When the head was placed downwards, a pretty large white pouch came out of its mouth. ÆLIAN ſuppoſes this to have ſerved as an aſylum to its young brood in time of danger. The length of the fiſh from the tip of the noſe to the end of the tail was ſix feet eight inches; the length of the pectoral fin one foot four inches: the ſcale annexed to the drawing will give the meaſurement of the other parts. It was a female, and weighed fifty-five pounds. An outline of the under ſide of the head is added, in order to ſhew the ſituation of the mouth.

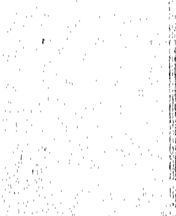
As I have never been able to ſee an accurate drawing of this fiſh, and as Mr. PENNANT, in his Britiſh Zoology, wiſhes a farther account may be given of it, I thought it not unworthy of the Society's notice. The fiſh itſelf was ſtuffed, and is now at the Britiſh Muſeum.

(a) I cannot aſcertain how many rows of teeth belonged to each jaw, they being ſeparated by boiling before they were properly examined.

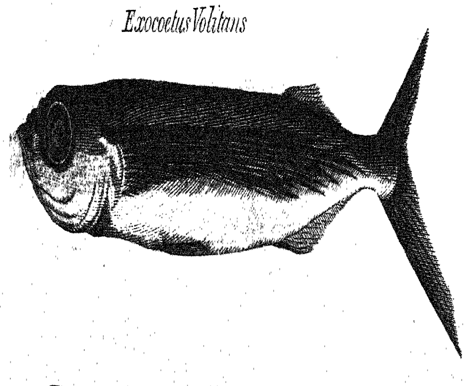
Squalus Glaucus



Scale of Feet.



Exocoetus Volitans



T. Brown del.

Basire Sc.

XXXV. *A Defcription of the* Exocœtus Volitans, *or* Flying Fifh. *By* Thomas Brown, *Surgeon, near* Glafgow. *Communicated by Dr.* Fothergill, *F. R. S.*

Read June 4, 1778.

THE beft reprefentation I can give of the flying fifh is in the accompanying fketch, drawn from one of the middling fize, about nine inches long, and full four round at the thickeft part[(a)].

From the largenefs of the head, and the body being neither prominent above or on the fides, the eyes are fituated in fuch manner as to difcover their danger or prey almoft all around them; but when they are pufhed out of their fockets, which the fifh is capable of doing confiderably, their fphere of vifion is greatly increafed.

The fkin is uncommonly firm for the fize of the animal, and their fcales large and thick. As they have no membrane to fhade the eye, they are not able to cover the pupil in any of its motions.

The wing is no other than a large pectoral fin, compofed of feven or eight ribs or pinions, the largeft of which being uppermoft, reaches almoft to the tail, the

(a) The naked out-line fhows the form of the mouth when opened.

reft

reſt gradually ſhortening to the bottom, are connected by thin membranous pellucid films or webs from their roots which ſpring near the gills to the very ſummit, where they loſe themſelves in ſlender points: at their thickeſt ends approaching each other, they unite in a line, which, in correſpondence with the form of the gills, is nearly the ſegment of a circle; though there they are connected, it is in ſuch a manner as to allow of being drawn a little aſunder, which ſeparation is conſiderable at the other extremity. The united ends are grooved or hollowed, to receive a ridge or protuberance of the ſcapula, to be afterwards mentioned, forming a joint capable of little motion, excepting backward and forward; in the one caſe, the wing lies cloſe to the ſide; in the other, it is moved from the ſide forward, forming an acute or rectangle with the body of the fiſh; but neither at this time expanded. Theſe two motions are performed, I preſume, in common ſwimming.

The fore-part of its body, from near the back bone downward to the bottom, where it terminates in a point, is fortified juſt behind the gills by a flat bone on each ſide, which anſwer all the purpoſes both of clavicles and ſcapulæ in land animals: they are firmly united before, or at the inferior part where they are narrower, and running upward, widening as they approach the back, they become

become ſomewhat hollow towards the body, and a little convex outwardly at the broadeſt part; but towards the gills the edge of the bone on each ſide is turned outward, like the cape of a garment, to form a ſmooth ſurface for them, and at the ſame time to give lodgement to a ſtrong muſcle under it, which fills the whole ſpace, on the ſuperior part of the bone; for on the poſterior part of it the articulation is made with the wing.

Juſt above the joint the ſcapula is ſmooth and hollowed, in the manner of a creſcent, to allow a tendon to paſs from a ſmall muſcle which lies on the inferior part of it, next to the body of the fiſh.

The upper part of the ridge that forms the joint, and is received by, or articulated with the wing, is rounded and ſomewhat enlarged, over which the ſtrong tendon, bound down by a ligament, together with ſome fibres of the muſcle lodged under the inverted edge of the bone, is obliged to paſs, and, going over the joint, is inſerted into the root of the ſtrongeſt and uppermoſt pinion; near to which place, the tendon, paſſing in the ſemi-lunated part of the ſcapula before mentioned as over a pulley, is alſo inſerted a little way beyond the joint.

By the action of theſe two muſcles, pulling in oppoſite directions, though both upwardly, at the ſame time

 that

that the lower pinions are kept down by the mufcles on the anterior, by thofe on the pofterior and inferior part of the fcapula; I fay, the effect of the action of thefe two mufcles is, to pull the pinions upward, and at a greater diftance from one another, or in other words to expand the wing; for the joint does not allow of any motion upward, and if it did, it would not in the leaft influence the fize of it.

The other mufcles that lie on the external, internal, and inferior parts of the fcapula, together with feveral fmall ones that run backward: thefe, I fay, alfo ferve to move the wing backward and forward. This fcapula and wing, with all its apparatus of mufcles, can be eafily divided, except at the fuperior part, from the mufcles that form the fore part of the body of the fifh, being only connected by a cellular medium.

The globe of the eye is large in proportion to the animal; the pupil large too, and nearly, if not altogether, circular. The cornea is lefs tranfparent than in the generality of fifhes; the fore part of the globe is a good deal flatted, as if a fegment or portion had been cut off, for fo fmall a part of the aqueous humour is contained between the cornea and iris, which is of a filver colour, that they are nearly in contact.

The optic nerve, though at its egreſs from the ſkull it is united by a common external membrane with that of the other eye, does not ſeem blended with it; this nerve, which is very large, pierces the external coat on the bottom of the ball, but not in the center; it enters on the ſide of the axis next the fiſh's body. The external tunic into which the muſcles are immediately inſerted, and which gives ſtrength and figure to the whole, is very firm, tough, and almoſt horny: when the eye is boiled, it ſeems to have a continuation of fibres, and indeed is of the ſame colour with the ſeptum of the eye or iris, the cornea ſeparating readily from it, and having then the appearance of the ſmall ſegment of a great circle or globe, applied to the great ſegment or ſide of a much ſmaller one. All the bottom of the ball is covered with this ſtrong membrane, except in the poſterior part, where it becomes abruptly much thinner, more pliant, and of a ſhape nearly reſembling the ſpace left by the union of four circles, or a kind of ſquare with its ſides bent inward; in the center of this the optic nerve enters, cloſe to the ſide of which an opening, like a pin-hole, appears, through which I imagine a ſmall artery paſſes.

The cryſtalline humour, both in the recent and boiled ſubject, is entirely ſpherical; in one it had the appearance of bottle glaſs; in the other it was bright as cryſtal.

When boiled it feemed to be attached to the vitreous humour, which was not then coagulated, it had an oblong blackifh fubftance fixed to it like the fragment of a blood veffel, which I could with difficulty feparate.

In the frefh fifh, the bottom of the eye, except where the optic nerve entered like a fmall elevated white fpeck, was laid over with a downy pearl-coloured paint; a part of which, upon fqueezing out the vitreous humour, fometimes floats on its furface. Upon removing this, a black foft painting appeared, which in the botttom of the globe, and fomeway round the entrance of the nerve, had a reddifh caft, or ftreaks of red, buried in it: thefe were maffes of fine blood veffels, which I imagine had fprung from the fmall perforation before mentioned. In the boiled eye, thefe paints were not much altered, except the red part, which, like all coagulated blood, was now become dufky. On the back part of the iris, or rather the pofterior part of the aqueous humour, it was only covered over with the black coloured pigment.

The mufcles of the eyes were remarkably ftrong, broad, and diftinct; for in fmall fifhes they are in general fo pappy and tender, that it is very difficult to examine them with accuracy.

Their throat or fwallow is formed of an oblong, rounded protuberance on the backpart of the fauces,

 and

and a receiving hollowed ſubſtance on the fore part, both plentifully armed with ſmall tenter hooks, pointing backward. They ſeem to have no remarkable dilatation in the canal of the bowels, in the manner of a ſtomach; but one tube paſſes directly from the mouth to the anus, on the upper or anterior part of which lies the heart; on the lower or poſterior the liver and gall-bladder; and on the ſides of this laſt are ſituated the rows, which conſiſt of two lobes. On each ſide, and at ſome little diſtance from the heart, is a pale aſh-coloured ſubſtance, ſomewhat reſembling the lungs of ſmall birds, which ſeem to join at the back, and to run united all along the hollow depreſſion there as far as the anus. Theſe parts were ſo very tender, and ſo little fit for examination with the hands or knife, that it was impoſſible for me to diſcover their uſe, or to trace any communication they might have with the throat.

Noſtrils they have, and I could paſs a hog's briſtle through them, by the palate, into the mouth.

In the recent ones the abdomen was near two-thirds full of air. At the baſis of the ſkull I found two little flat ſnow-coloured bones, irregular and rough, ſuch as we find in cod and many other ſea fiſhes.

Upon examining the wings after being ſome time expoſed to the air, I find they become ſo dry, and the fine thin

thin intervening membrane fo rigid, that it is difficult to expand them without violence, at the fame time that the motion of the whole wing backward and forward is nothing impaired; this circumftance, which only happens after the fifh has been a confiderable time out of the water, may have given rife to the common tradition amongft fea-faring people, that it can fly no longer than its wings are wet, and that in its flight it fkims along the furface and dips, fkims and dips again, with no other purpofe than to moiften and keep them in a flying trim.

That in the courfe of one flight, at leaft once, twice, or perhaps thrice, it flightly touches the water is certain; but the whole is performed in fo fmall a fpace of time, and its continuance in the air is of fo fhort duration, that even in the dryeft, warmeft weather little is to be apprehended from the too great rigidity of the wings. In my opinion, though this circumftance of moiftening them may be of fome ufe, and a fecondary advantage, yet they feem to touch the water for a more important purpofe, for the fame reafon that a diver or fwimmer, when below the furface of another element, is very frequently obliged to emerge into his own. It may alfo be of fome ufe in giving the animal new force and vigour for another departure.

But as flying is only a fudden expedient, in order to efcape the jaws of their enemies, and by no means their

natural

natural or usual mode of existence, there seems not to be any particular or remarkable apparatus necessary for a long subsistence, nothing is wanted but the power of motion in our atmosphere, and the drying of their wings, appears to be the only inconvenience they are likely to suffer. Hence it is, that in every other part of their frame and structure, small provision is made by all-bountiful nature for this transmigration.

In flying not only their fins and wings are much expanded, but also their tail; they skim along the surface of the deep with great velocity, somewhat in the manner of a swallow, but in straight lines, and from the blackness of their backs, the whiteness of their bellies, and forked expanded tails, they have much the same appearance[(b)].

They can fly fifty, sixty, or more yards at one stretch, and repeat it a second or even a third time, only the slightest momentary touch of the surface that can be conceived intervening.

They are seldom solitary, but rise in flocks or shoals[(c)]. In taste they somewhat resemble a mackerel.

(*b*) Since writing the above, I find the ancients were acquainted with this species; PLINY mentions it under the name of the Hirundo.

(*c*) We found them in greatest quantities between the latitude of 15° and 10° N. from 20° to 30° W. of the meridian of London; but they abound between the Tropics in many other places of the vast Atlantic, as well as in the Indian Ocean.

They

They are drove out of their own element by the ſhark[(d)], the porpoiſe[(e)], the albicore[(f)], the bineto[(g)], and dolphin[(h)], to become a prey in ours to the booby[(i)], the man of war[(k)], and tropic bird[(l)]; but I ſuſpect their viſion in air is not very diſtinct, as they often in their flight fall a ſhip-board, or ſtrike againſt whatever happens to be in their way, as was the caſe with all theſe I examined: and indeed the form of the cryſtalline humour of the eye ſeems to countenance this opinion, being of the ſame ſpherical figure with that of the greateſt part of thoſe fiſhes that altogether inhabit the watery element.

(d) Squalus Conductor.
(e) Delphinus Phocæna.
(f) Scomber Thynnus.
(g) Scomber Pelamis.
(h) Delphinus Coryphæna.
(i) Pelicanus Piſcator.
(k) Pelicanus Aquilus, or Man of war bird.
(l) Phaeton æthereus.

XXXVI. *Reasons for dissenting from the Report of the Committee appointed to consider of Mr.* Wilson's *Experiments; including Remarks on some Experiments exhibited by Mr.* Nairne. *By Dr.* Musgrave, *F. R. S.*

Read June 25, 1778.

I DO not find that Dr. FRANKLIN, in any of the passages where he speaks of the efficacy of sharp-pointed conductors to prevent electrical explosions, has expressed any doubt of their being universally preferable for this purpose to those which have a blunt or spherical termination. The same observation may be made of the other gentlemen who are the advocates for his doctrine. It may therefore be assumed, that both he and they mean to assert an universal proposition, " That " sharp points will, in all cases, draw off the electrical " fluid silently within the distance at which rounded " ends will explode; or, at least, that the former sort will " in no case receive an explosion at a greater distance " than the latter." I think it necessary to observe, that, though I dissent from this doctrine, I do not mean to assert the contrary universal proposition, but only to deny the universality of that asserted by Dr. FRANKLIN, which I appre-

I apprehend to be fometimes true, and fometimes alfo falfe.

But before I attempt to fpecify the particular cafes in which the fharp and the blunt terminations are refpectively more liable to electrical explofions, it may be of ufe to fhew (what many gentlemen feem not to be thoroughly aware of) that fharp points having the moft perfect communication with the earth, are not wholly exempt from receiving them. My firft authority fhall be Dr. FRANKLIN himfelf. " Let a perfon," fays he, p. 60. " ftanding on the floor, prefent the point of a needle at " twelve or more inches from it [the prime conductor], " and while the needle is fo prefented, the conductor " cannot be charged, the point drawing off the fire as " faft as it is thrown on by the electrical globe. Let it " be charged, and then prefent the point at the fame dif- " tance, and it will *fuddenly* be difcharged." The word *fuddenly* means, I fuppofe, that it will receive an explofion; that being the moft natural and obvious proof of the *fuddennefs* of the difcharge. The fame thing is more directly afferted by Mr. HENLY, in vol. LXIV. of the Phil. Tranf. p. 138. where he informs us, that in difcharging three of his large jars, to the coating of which he had connected a wire nicely tapered to a point, the fire flew to the point, and the jars were difcharged with a full and

loud

loud explofion. A third, and equally decifive, proof is furnifhed by Mr. NAIRNE's own experiments, though feemingly made with a contrary view. For when the double or interrupted conductor was ufed, and the fecond conductor fixed down by fcrews at about three inches diftance from the firft, the point prefented to the contrary end of the fecond conductor was found to receive a ftrong and loud explofion, with a white light at the diftance of at leaft three inches.

If we compare this experiment with another, very common one, exhibited at the fame time by Mr. NAIRNE, the comparifon will, perhaps, lead us to the difcovery of a principle upon which electrical explofions very frequently depend. Though the point, in the circumftances above defcribed, received fo ftrong an explofion, yet when it was prefented directly to the prime conductor, it received no explofion whatever at any diftance, unlefs a fucceffion of weak fparks, at the diftance of about a quarter of an inch, can be called fo. To what muft this difference be attributed? Plainly to the different quantity of electric fluid accumulated on the prime conductor in the one and the other cafe. Where the point is prefented to the prime conductor, from the time the machine begins to work, the property which is attributed to them, and which, in fome cafes, they really poffefs, of ftealing

away the electricity filently; this property, I fay, operating from the very beginning, prevents the electric fluid from being accumulated in the prime conductor, and of courfe the quantity of it will always be fmall. But when a double or interrupted conductor is ufed, the fecond conductor receives no electricity till the prime conductor is pretty highly charged, and, if put at the greateft ftriking diftance, not till it is fully charged, and confequently the fharp point prefented to the oppofite end can carry away none of it till that time; when the whole quantity is thrown off at once. It fhould feem then, that the explofion in one cafe, and the non-explofion in the other, depended wholly upon the different quantities to be thrown off: whence it will follow, that though a fmall quantity of electricity will pafs off filently upon a point, yet that this power is very limited; for that if a fomewhat greater quantity be applied fuddenly to a fharp point, it will not pafs off filently, but create an explofion in proportion to its denfity.

The facts above related are a fufficient anfwer to that other experiment of Mr. NAIRNE's, in which he exhibited a fharp point, that when perfectly communicating with the earth drew off the electricity filently from the prime conductor; but received explofions freely, when the communication was broken by interpofing little ifthmufes of fealing wax. This experiment, it is true, demon-

demonſtrates that a broken communication will occaſion the ſharp point to receive an exploſion; and ſo far it muſt be owned to be concluſive. But if it was intended to ſuggeſt, that whenever ſharp points do receive an exploſion, it is owing to this circumſtance, and, conſequently, that Mr. WILSON's experiments at the Pantheon were unfairly made; in this view it has no weight, becauſe we have already ſeen that an interrupted communication is not the only circumſtance that will produce an exploſion, for that increaſing the quantity of electricity will have the ſame effect.

I cannot omit the opportunity here offered me, of remarking the unfairneſs of the inſinuations that have been thrown out to the prejudice of Mr. WILSON. Had there been any juggle in making his experiments, it would certainly have been detected by the committee appointed to examine them. And in caſe of ſuch a detection, it was the duty of the committee to lay open the impoſture both to the Society and the Public. Inſtead of which, inſtead of diſputing or even doubting the fairneſs of them, they have in a manner admitted it, by only ſaying in their report, that they appear to be inconcluſive. This, I ſay, is admitting the facts to be fairly ſtated: unleſs we could ſuppoſe their regard for Mr. WILSON, and tenderneſs for his reputation, had induced

them, after detecting the fallacy of his experiments, to pass it over in silence; of which improper partiality I do not know that they are so much as suspected. What therefore the committee, after a strict scrutiny of the matter, did not think themselves warranted to say, I take for granted they would not insinuate; and that therefore such insinuations can only arise from the levity of more obscure persons, puzzled perhaps by the seeming contradiction between Mr. WILSON's experiments and those of Mr. NAIRNE, and too impatient to investigate the real causes of that difference.

I am persuaded, however, that the known property of sharp points to carry off electricity silently, when the quantity is small, together with that other principle, which I apprehend I have here established, that they cease to do so when the quantity is large; that these two (taken together) will clear up the whole difficulty, and account for Mr. NAIRNE's experiments, without any impeachment to those of Mr. WILSON. I have already had occasion, in the course of this argument, to consider two of those experiments, of which therefore I shall say no more; but proceed, without further digression, to examine those that remain.

The first I shall mention is that, in which the prime conductor, being previously charged with electricity, a

sharp

ſharp point is preſented to it within the attracting, and without the exploding diſtance, and then brought ſlowly on towards it. In this caſe no exploſion follows; neither is there any reaſon to expect it ſhould, becauſe the quantity of electricity is gradually diminiſhed by the approach of the point, ſo that when it comes within the ſtriking diſtance there is not enough left to make an exploſion.

It is equally eaſy to explain what happens when a tranſverſe arm is hung ſo as to oſcillate freely upon the prime conductor, and two equal cylinders of tin-foil are ſuſpended, one at each end of this arm in perfect equilibrio with each other. The apparatus being in this ſtate, if the machine be worked, the two cylinders will remain ſtationary, neither of them aſcending or deſcending. They will alſo remain ſtationary, if a point be preſented to one, and a rounded end to the other. In the firſt caſe they are electrified, but remain motionleſs, becauſe there is no conducting body within the ſphere of their action. In the ſecond, the reſult is the ſame; becauſe, making in fact part of the prime conductor, the point preſented to one of them prevents any accumulation of electricity. When the point is withdrawn, and the rounded end ſuffered to remain, an accumulation takes place, becauſe there is nothing now to ſteal it away; and the conſequence is, that the cylinder deſcends towards the rounded end, and explodes

explodes as soon as that accumulation arrives at a certain period.

Thirdly, when an intermediate conductor is used, terminated at each end with a ball, and the middle of it resting in equilibrio upon a pivot, on which it has a free oscillation upwards and downwards; if in this state a point is placed under the end most distant from the prime conductor, the machine being then worked, the other end will approach so near the prime conductor, as that the stream of electricity will flow freely into it, as fast as it is produced by the action of the wheel. In this case there will be no explosion; and the reason is obvious, because the second conductor, when it approaches so near the first as to form an uninterrupted channel for the electric stream, becomes virtually a part of the first. Hence the point operates upon both together, just as it does when presented directly to the prime conductor, that is, it steals away the electricity by little and little, leaving not enough to give an explosion. When instead of the point a polished ball is placed under the same end as before, this being less disposed to receive the electric fluid, conveys away none of it; so that accumulating to a certain degree upon the prime conductor, it explodes upon the contiguous end of the second, which, having a free oscillation, flies up with the stroke, and carries the

opposite

oppofite end towards the ball, where, being faturated, it gives a fnap; the recoil of which fnap throws that end up, and the contrary end back towards the conductor, and fo on alternately, as long as the machine continues working.

The event, however, is widely different when the fecond conductor, inftead of having a free ofcillation, is fcrewed down in one place, and at fuch a diftance from the prime conductor, as not to receive the electric fluid till confiderably accumulated. For then the fharp point, previoufly oppofed to its other end, difcharges it, as was before obferved, not in a continued ftream and filently, but at intervals, and with a ftrong explofion.

The laft of Mr. NAIRNE's experiments, and the only one yet unconfidered, is that of the fharp point, which, being fixed to a kind of inverted pendulum, ofcillated with great velocity under the prime conductor, without receiving any explofion. Now from this experiment I do not comprehend how any general conclufion can poffibly be drawn. It has been already fhewn, from the acknowledgement of Dr. FRANKLIN, and the experiments of Mr. HENLY and Mr. NAIRNE, that electricity, accumulated to a certain degree, will explode upon a point. If, therefore, in any particular inftance it does not explode, what can we infer from it, but that the accumulation in

 every

every fuch inftance was not fufficiently great; which may happen either from the fmallnefs of the apparatus, or from want of care in making the experiment.

And now if we look back upon Mr. NAIRNE'S experiments (which, by the by, have not all of them the merit of novelty) we fhall find them to be nothing more than different exemplifications of this well-known principle, that fharp points giving lefs refiftance to the ingrefs of the electric fluid will draw it off at a greater diftance than blunt or fpherical terminations, and where the quantity is fmall will draw it off filently. This, I fay, is the whole amount of his experiments; the only one of them in which the electric fluid had time to accumulate, being attended with a different event from the reft, and producing, as might reafonably be expected, a ftrong explofion.

It is not, however, this fingle property of fharp-pointed conductors, which muft decide the queftion. We have already feen, that there are two properties infeparable from them, both of which muft be taken into the account, before we can determine the propriety of affixing them to buildings, particularly powder magazines, as prefervatives from lightning: firft, their greater propenfity to admit the electric fluid, in confequence of which they act upon electrified bodies at a greater diftance than

rounded

rounded ends will; and, fecondly, their incapacity to draw away more than a certain quantity of electricity without an explofion.

The firft quality enables them, when electricity is accumulated gradually, or when they are brought gradually towards the electrified body, to fteal away the fluid by little and little, till there is not enough left to give an explofion. And hence, in common experiments, the point, placed at a greater diftance than the ball, will prevent the electricity from exploding, as it otherwife would do, upon the latter. But if we combine this quality with the fecond, the fuperior propenfity to admit, with the incapacity in certain circumftances of difcharging filently, it will be evident, *à priori*, that the phenomena muft in fuch cafes be reverfed, juft as they appear to be in Mr. WILSON's experiments; that the point muft ftrike at a greater diftance, and the rounded end at a leffer.

What puts this matter beyond a doubt is, that when the double or interrupted conductor is ufed, the experiment may be fo managed, as that the ball fhall receive an explofion at a greater diftance than the point, or the point at a greater diftance than the ball, at the pleafure of the operator. If care be taken, at the beginning of the experiment, to fet the fecond conductor at the greateft

diſtance from the firſt, compatible with its giving a full and ſmart exploſion, the point in that caſe will receive the exploſion at a much greater diſtance than the ball. I was myſelf once preſent at an experiment, when the difference was as 6 to 1, that is, the ball would receive no exploſion at a greater diſtance than $\frac{7}{8}$ths of an inch, when the point received it at ſix times that diſtance. On the contrary, if the ſecond conductor be put conſiderably within the diſtance above deſcribed, the ball will receive an exploſion much farther off than the point. Upon repeating both theſe experiments lately with a ſmall machine, I found the reſult to be as follows. When the diſtance between the firſt and ſecond conductor was $1\frac{5}{8}$th of an inch, the point was ſtruck at $2\frac{1}{16}$th of an inch; but a ball of $1\frac{5}{8}$ths of an inch in diameter would not take the ſtroke at more than one inch. But when the diſtance between the conductors was only $\frac{5}{8}$ths of an inch, the point could not be ſtruck at more than $\frac{7}{8}$ths, whereas a ball of the ſame diameter as before was ſtruck at 7 inches and $\frac{3}{4}$, and a leſſer ball $\frac{3}{10}$ths of an inch in diameter at 6 inches and $\frac{3}{16}$ths. I have been told alſo, but have not yet had time to verify it, that a medium diſtance may be found, at which if the ſecond conductor be ſet, the point and ball preſented to the other end will be exactly upon a par with reſpect to the exploding diſtance.

These

Thefe phenomena, to perfons who have not carefully confidered them, muft appear fo extraordinary, that unlefs the caufe of the diverfity is explained, they will perhaps be led to fufpect fome unfairnefs in making the experiment. The truth, however, is this; that when the two conductors are fet at the greater of the two diftances, the abfolute quantity of electricity collected before the explofion is exactly the fame in each experiment; and therefore the diftances of the ball and point from the fecond conductor being equal, and the greateft at which either of them will be ftruck, the explofion will go to the point, as being more fufceptible, and giving lefs refiftance than the ball. But in the fecond fuppofed cafe, when the fecond conductor is fet confiderably within the former diftance, the quantity of electricity which explodes upon the point and the ball is not the fame; the point in this cafe exerting its known property of ftealing away the electricity filently, which the ball from its greater refiftance is incapable of doing. The confequence is, that the quantity accumulated to give an explofion upon the ball is greater than that which explodes upon the point, and being greater will very naturally explode to a greater diftance.

I might fafely have refted the matter upon this ground; but another proof, equally decifive, having fince

 occurred,

occurred, it is but doing justice to my argument to insert it here. In the experiments made with a view to settle this dispute, and published by Mr. HENLY about four years ago, there is one, the fifth of that set, which it is difficult to reconcile with the doctrine here laid down, that electricity strongly accumulated, and moving with great velocity, will explode upon a sharp point rather than a ball.

He describes it thus: "Having insulated the jar, and "connected *by chains* with the external coating, on one "side a knob, and on the other side a sharp-pointed wire, "both being insulated and standing five inches from "each other, I placed a large copper ball, eight inches "in diameter (insulated also) so as to stand exactly at "half an inch distance both from the knob and the "point. The jar being fully charged, I delivered it upon "the copper ball by my discharging rod, whence it "leaped to the knob, which was three quarters of an "inch in diameter, and the jar was discharged by a loud "and full explosion, and the chain was very luminous." Phil. Transf. vol. LXIV. p. 136.

It must be obvious to any careful electrician, and Mr. WILSON had in his answer observed, that an experiment thus loosely and unphilosophically made, was not greatly to be relied upon; because two chains being made use of,

 one

one to connect the ball, and another to connect the ſharp point with the coating of the phial, a different number of links, or different degree of tightneſs in the two chains, would produce a difference in the reſult: for it bing a known property of electricity to paſs moſt readily where it has the feweſt and the ſmalleſt intervals to leap over, the exploſion would naturally paſs that way, where the links were drawn tighteſt; or if both chains were left looſe, as from the plate they appear to have been, then where they were feweſt in number, inſtead of being determined by the circumſtances of bluntneſs and ſharpneſs.

It was however poſſible, that Mr. HENLY might be right in attributing it to the ſharpneſs of the point; and therefore, in order to ſettle this doubt, I deſired that the experiment might be tried over again in ſomewhat different circumſtances. Mr. CAVALLO accordingly tried it in the following manner. Upon an inſulated ſtand he placed a ball, about $\frac{6}{10}$ths of an inch in diameter, and a ſharp point, directly oppoſite to the place where a Leyden phial, when charged, was to be ſet down. Both of theſe, the ball and point, were connected to his diſcharging rod by copper wire. He then took the phial, which held about a quart, and having charged it, ſet it down before the ball and point, took his diſcharging rod, and

completed

completed the circuit. After the exploſion the diſtances were meaſured. In the experiments which I ſaw the phenomena were as follows: in the firſt, the diſtance of the ball was $\frac{19}{20}$ of an inch, that of the point $\frac{26}{20}$; in the ſecond, diſtance of the ball $\frac{16}{20}$, of the point $\frac{27}{20}$; in the third, diſtance of the ball $\frac{15}{20}$, of the point $\frac{26}{20}$; in the fourth, diſtance of the ball $\frac{18}{20}$, of the point $\frac{29}{20}$. In all theſe experiments the point, though conſiderably farther off from the coating of the phial, was ſtruck in preference to the ball. In a fifth experiment, when the point ſtood at double the diſtance, the ball was ſtruck, and not the point.

The reſult of theſe experiments being ſo widely different from the reſult of that made by Mr. HENLY, is a clear proof that he formed his concluſions too haſtily, having attributed to ſharpneſs and bluntneſs a phenomenon cauſed by the unequal reſiſtances of the chains. As this experiment may be made with almoſt any machine, thoſe who do not chuſe to repeat it will have no right to plead the want of a ſufficient apparatus, and muſt look out for ſome other reaſon to evade the force of it.

I come now to conſider more particularly the practical queſtion, whether the ſharp-pointed or the blunt conductors are moſt proper to be affixed to buildings, as preſervatives

preſervatives from lightning. And here it is neceſſary to obſerve, that buildings may be expoſed to a ſtroke of lightning in ſeveral different ways. The lightning which, to avoid prolixity, I ſhall only ſpeak of as poſitive electricity: the lightning, I ſay, may accumulate directly over the building; or it may be brought towards the building by a ſmall cloud fetching it in ſeveral ſucceſſive trips from a large cloud at ſome diſtance; or a large electrified cloud may be carried rapidly towards it by the wind: a circumſtance this by no means rare, there being no leſs than four inſtances of it upon record in the Phil. Tranſ. vol. XLIX. p. 16. and p. 309. vol. LXI. p. 72. and vol. LXIV. p. 351. In the firſt of theſe ſuppoſed caſes a ſharp-pointed conductor might poſſibly drain the cloud of its lightning as faſt as it began to accumulate, and thereby prevent any exploſion whatever. In the ſecond, as the cloud, by ſuppoſition, not being driven in one direction by the wind, could not move with any remarkable velocity, it is reaſonable to imagine, that in this caſe alſo there might be no exploſion; and that the electricity of the larger cloud might be gradually exhauſted. But if, according to the third ſuppoſition, a cloud of great extent and highly electrified ſhould be driven with great velocity in ſuch a direction, ſo as to paſs directly over the ſharp-pointed conductor, there can

 be

be no doubt but that such a point, from its superior readiness to admit electricity, would take the explosion at a much greater distance than a rounded end, and in proportion to the difference of that striking distance would do mischief instead of good.

But perhaps it will be said, that every stroke of lightning falling upon a sharp point is previously diminished by that point, and therefore may more easily be transmitted through the conductor, than when it falls undiminished upon a rounded end. Upon this supposition I must observe, that it not only contradicts Mr. WILSON's experiments at the Pantheon, but also Mr. HENLY's experiment already referred to in this paper, where the fire flew to a very taper point, and melted the end with a strong and loud explosion. So also the sharp-pointed conductors affixed in America to the houses of Mr. WEST, Mr. RAVEN, and Mr. MAYNE, do not *seem* to have diminished the force of the explosion, if we may judge from the violence of its effects as related at large in Dr. FRANKLIN's works. It should seem, therefore, that the power of diminishing a stroke, like that of preventing it, is only contingent, and depends, as we said before, upon the degree of velocity with which the lightning moves.

The

The ſum of the whole is, that conductors, terminated by ſharp points, are ſometimes advantageous, and at other times prejudicial. Now as the purpoſe for which conductors are fixed upon buildings is, not to protect them from one particular ſort of clouds only, but, if poſſible, from all, it cannot ſurely be adviſeable to uſe that kind of conductors, which if they diminiſh danger on one hand, will increaſe it on the other. It is the duty of a pilot to keep out of the way of rocks; but it is alſo incumbent upon him, in avoiding the rock, not to take ſo large a compaſs as to run his ſhip upon a quickſand.

When I ſay that ſharp-pointed conductors may in ſome caſes diminiſh danger, I ſpeak of them, perhaps, rather too favourably: for their power of ſtealing away the electric fluid being confined to caſes where the accumulation is ſmall, it follows, that they only operate where their operation is not wanted. The caſes againſt which we wiſh principally to provide, are the exploſions of extenſive and highly electrified clouds; and here we have ſeen, that blunted ends, as acting to a much ſmaller diſtance, are entitled to the preference.

If it be admitted, that ſharp-pointed conductors are attended with any, the ſlighteſt degree of danger, how much muſt that danger be augmented by carrying them

 high

high up into the air, by fixing them upon every angle of a building, and making them project in every direction? Ought this to be advifed while there is ftill a doubt of the poffibility of their doing mifchief? And can the committee, therefore, be perfectly juftified for giving fuch a decided preference to the ufe of fharp conductors, in defiance of numerous experiments, not one of which they have attempted to controvert?

I have now done with the report of the committee, and fhall next proceed to enquire whether, as fome gentlemen apprehend, the termination of conductors is a matter of indifference: but this I muft referve for the fubject of a future paper.

A P P E N D I X.

I think it neceffary to apprize the reader, that the foregoing remarks were drawn up before Mr. NAIRNE'S paper appeared, and are therefore to be confidered as relating only to his experiments.

I muſt at the ſame time obſerve, that, in ſtating the different circumſtances in which lightning may be collected ſo as to affect a building, I have ſuppoſed a caſe which poſſibly may never exiſt. I have ſaid, that lightning may accumulate directly over a building, and admitted, that in that caſe the cloud might be drained of it by a pointed conductor. Now we have no evidence, that the accumulation of lightning is confined to a ſingle cloud, or ſmall circumſcribed ſpot in the heavens. On the contrary, the numerous exploſions, which in moſt thunder ſtorms happen nearly at one and the ſame inſtant, rather lead us to imagine that a great part of the horizon is at thoſe times full of lightning, and therefore incapable of being drained. I would therefore wiſh to have my ſuppoſition underſtood as a mere imaginary ſuppoſition, for the ſake of rendering the argument more perſpicuous, and not as the admiſſion of a real fact.

Laſtly, I beg leave to correct an expreſſion I have uſed with reſpect to pointed conductors, that they only operate where their operation is not wanted. Now this is not accurately true: for if by operating upon a quantity of electricity too ſmall in itſelf to do miſchief, they prevent its growing to a great and dangerous quantity, this would, as far as it goes, be a very conſiderable advantage.

I ought

I ought therefore to have ſaid only, that pointed conductors afford no protection where the danger is great and imminent, and only obviate that which is diſtant and problematical; and that theſe laſt are not the caſes againſt which we principally wiſh to provide.

March 18, 1779.

XXXVII. *Experiments on Electricity, being an Attempt to shew the Advantage of elevated pointed Conductors. By Mr.* Edward Nairne, *F. R. S.*

N. B. This paper is misplaced through a mistake of the Secretary's; it should have been inserted before Dr. MUSGRAVE's.

Read June 18 and 25, 1778.

A DIFFERENCE of opinion prevailed some time ago and has of late been revived, in regard to the termination of conductors for the preservation of buildings from the effects of lightning.

Some gentlemen think that they should not terminate in a point, but be blunted; and also that they should not exceed the highest part of the buildings[(a)]; they likewise think, that to prevent lightning from doing mischief to great works, high buildings, and large magazines, the several buildings should remain as they are at top, that is, without having any metal above them, either pointed or not, by way of a conductor; but that on the inside of the highest part of such a building, and within a foot or two of the top, it may be proper to fix a rounded bar of me-

(a) Mr. WILSON's new Experiments on the Nature and Use of Conductors, p. 7.

tal.

tal, and thence continue it down along the ſide of the wall to any kind of moiſture in the ground[(b)].

Others again are of a directly contrary opinion; thinking a conductor ſhould not only terminate in a point, but be conſiderably elevated above the higheſt part of the building[(c)].

As it moſt certainly would be of great conſequence to mankind to know which is the moſt eligible of theſe opinions, I have attempted, by what I could learn from the artificial lightning of our electrical machines, to determine which method is beſt to ſecure buildings from the effects of lightning: whether I have ſucceeded I leave to the judgement of others to decide from the following experiments and obſervations, which are ſubmitted with all due deference.

In pl. XIII. fig. 1. is a repreſentation of the electrical machine and the apparatus uſed in the following experiments. The diameter of the glaſs cylinder A, fig. 1. was eighteen inches; the length of the conductor B, which was of wood covered with tin-foil, was ſix feet, and the diameter of it one foot. At the end of this conductor was ſcrewed a braſs ball C, of four inches and a half diameter. This conductor, when charged by the

(b) Mr. WILSON'S Letter to the Marquis of ROCKINGHAM, Phil. Tranſ. vol. LIV. p. 247.

(c) Ibid. p. 203.

glaſs

glafs cylinder, being intended to reprefent a cloud charged with electricity or matter of lightning will, for diftinction fake, be called the *artificial cloud*, in the following experiments. D reprefents a brafs rod on a ftand covered with tin-foil, having a good metallic communication with the earth; at one end of this rod were fcrewed other rods, terminating with different fized balls, or a rod terminating with a point. This rod D was moveable in a focket, in order that it might be placed with its termination at different diftances from the ball C at the end of the artificial cloud. As the terminations on this rod were to receive from our artificial cloud the ftroke or fparks of our artificial lightning, it will be called the *receiving rod* in the following experiment. The receiving rod with its ftand was intended to reprefent a conductor to a houfe, on which different terminations might be placed.

Before I relate the experiments it may be proper firft to premife, that electric fire, drawn off gradually from an electric cloud, was never known to do any mifchief, if the fubftance drawing it off had a good metallic communication with the moift earth; and that when any damage is done, it is occafioned by a ftroke of lightning, or in other words the electric fire of the charged cloud fuddenly difcharged through that body.

EXPERIMENT I.

I ſcrewed a braſs ball, of four inches diameter, at the end of the rod D, then placed it nearly in contact with the ball C, at the end of the artificial cloud: on charging the artificial cloud, the electric fire ſtruck from the ball C to the ball at the end of the rod, and continued ſtriking all the while it was gradually removing to the diſtance of ſeventeen inches and four tenths, and ſometimes on to nineteen inches: I have had ſtrokes twenty inches in length, but it has been very rare.

EXPERIMENT II.

The apparatus remaining as in the laſt experiment, I changed the ball of four inches diameter on the rod D, and in its place ſcrewed a ball of one inch diameter, then I placed this very near to the ball C as before: on charging the artificial cloud, the electric fire now ſtruck to the ball at the end of the rod D of one inch diameter, and continued ſtriking whilſt it was gradually removing to the diſtance of about two inches. It then gave over ſtriking, and was ſucceeded by a hiſſing noiſe and a continued light on the one inch ball, whilſt it was removing very gradually from the ball C, until the diſtance between the

two

two balls was about ten inches; the hiffing noife then ceafed, and the light difappeared on the inch ball. It now began to ftrike again, and continued ftriking to the inch ball all the time it was very gradually removed, till the diftance was about fourteen inches eight tenths; and fometimes would continue to ftrike to fixteen inches and three tenths.

This ftriking to the ball ceafing, and then beginning again, when the artificial cloud is ftrongly charged, is a fact which I believe has not been taken notice of by any one before; I fhall have occafion to fpeak of it again in fome of the following experiments.

EXPERIMENT III.

The apparatus remaining as in the laft experiment, I changed the ball of one inch diameter, and in its place fcrewed one of three tenths of an inch diameter. This fmall ball was alfo placed nearly in contact with the ball c: on charging the artificial cloud, the electric fire ftruck to this ball of three tenths, and continued ftriking to it whilft it was very gradually removed to the diftance of half an inch; beyond that, it would not ftrike to it. But the ball was luminous all the while it was removed beyond the ftriking diftance as far as thirty-three inches.

EXPERIMENT IV.

The apparatus remaining as in the laſt experiment, I only changed the ball of three tenths, and in its place ſcrewed a wire about three inches and a half long, terminating in a point: on charging the artificial cloud, I could not now get the electric fire to ſtrike the point, though the point was almoſt in contact with the ball c; but when it was about half a tenth of an inch diſtant from it, then the electric fire ran in a very ſmall ſtream to the point; but beyond that diſtance, though moved very gradually, it was only luminous, and continued ſo at the point all the while it was gradually removing to the diſtance of ſix feet from the ball c, at the end of the artificial cloud.

EXPERIMENT V.

The apparatus remaining as in the laſt experiment, I changed the wire, and in its place ſcrewed the ball of four inches diameter, uſed in the firſt experiment, having now a ſmall hole through it. I then put into this hole a wire, leaving the end, which terminated in a fine point, projecting out only one tenth of an inch beyond the ſurface of the ball, and directly pointing to the ball c:

on charging the artificial cloud, the ball with the point being firft placed nearly in contact with the ball c, it was then gradually removed; but not at any diftance would it ftrike to the ball, or the point projecting out of it. The point was luminous at the diftance of thirty inches.

EXPERIMENT VI.

Every thing remained the fame as in the laft experiment, except only that I now preffed in the point, till it was even with the furface of the four inch ball: on charging the artificial cloud, the electric fire did now ftrike to the ball at any diftance, from being nearly in contact, all the while it was very gradually removed to as far as feventeen inches and a quarter, though before in the laft experiment, where the point projected from the ball only one tenth of an inch, it would not ftrike at any diftance.

EXPERIMENT VII.

The apparatus remaining as in the laft experiment, I took a ball of three inches and a half in diameter, which had a fmall hole through it, and fcrewed it to a hollow brafs ftem. Then I put into this hole one end of a wire, and the other end, which was pointed, projected one inch beyond

beyond the ſurface of the three inch and half ball. This ball and ſtem, with the pointed wire to it, I fixed to a ſtand covered with tin-foil; having a good metallic communication with the earth, I placed this ſtand ſo that the point was directly oppoſite to the ſide of the artificial cloud, and exactly at five feet diſtance from it: then, on charging the artificial cloud, the greateſt ſtriking diſtance from the ball C to the ball of four inches diameter, on the receiving rod D, was found to be ſixteen inches and ſeven tenths.

EXPERIMENT VIII.

Every thing continued as in the laſt experiment, only now I drew the wire out of the ball and ſtem ſo far that the point projected nine inches beyond it: on charging the artificial cloud, the greateſt ſtriking diſtance now was found to be but ſix inches and eight tenths.

Now, in order to ſee how far a point, or different ſized balls fixed on the ſtand, and having a very ſmall ſeparation in the metallic communication with the earth, would viſibly act to carry off the electric fire of the artificial cloud, I made the following experiment.

EXPE-

EXPERIMENT IX.

I took a ſtick of common ſealing-wax, and having fixed a ſcrew to each end, I paſted a ſlip of tin-foil the whole length of the ſurface, and having made a ſeparation of the foil of about one fiftieth of an inch, I ſcrewed the pointed wire into one end, and the other end of the wax to the braſs rod, where the ball with the point projecting from it was placed in the laſt experiment. I alſo removed the other ſtand with the ball, to which the artificial cloud likewiſe ſtruck in the ſame experiment; the artificial cloud was then charged, and the ſtand being placed in ſuch a manner that the point was directly oppoſite to the ſide of the artificial cloud; it was then removed till I found the diſtance at which the light between the ſeparation of the tin-foil no longer became viſible. This diſtance of the point on the wax was above ſeven feet, how much farther it might have been luminous I had no opportunity of trying, this diſtance being the fartheſt I could remove it in my room, and under the diſadvantage of having the end of the artificial cloud within thirty-three inches of the edge of the wainſcot. When a ball of three tenths of an inch was put in the place of the point, the light was viſible at the diſ-

tance

tance of four feet ſix inches, but with a ball of three inches diameter only at two feet.

EXPERIMENT X.

I took another ſtick of ſealing wax, one inch and three tenths diameter, and about ten inches long, and paſted on it round pieces of tin-foil of half an inch in diameter, at about half an inch diſtance from each other. One end of this ſtick of wax was ſcrewed to the receiving rod D, fig. 2.; and into the other end was ſcrewed the pointed wire uſed in the fourth experiment. I then laid a piece of braſs on this wax, ſo as to connect all the ſeparations of the round pieces of tin-foil except two; then the point of this wire on the wax was placed nearly in contact with the ball. On charging the artificial cloud the electric fire now ſtruck to the point, and continued to ſtrike to it all the while it was gradually removed to the diſtance of one inch and one tenth: beyond that diſtance it would not ſtrike, but the point continued luminous till it was removed to the diſtance of three feet.

EXPERIMENT XI.

The apparatus remaining as in the laſt experiment, I only took away the piece of braſs which laid on the wax to

to connect the pieces of tin-foil together. The charged artificial cloud did not now ſtrike to the point until it was removed from the ball C to the diſtance of four inches and a half; it then began to ſtrike to it, and continued ſtriking whilſt it was gradually removing ſometimes to ten inches; but when the point was removed beyond the greateſt ſtriking diſtance, the point was not luminous as in the laſt experiment, except when the artificial cloud diſcharged its electric fire out into the air, in a diverging pencil from the ball C: then it was luminous, but at that inſtant only. Every time the artificial cloud ſtruck to the point, the electric fire made a beautiful appearance in paſſing off between the ſeparations of the pieces of tin-foil. I then connected all the tin-foil on the wax ſo as to leave no ſeparation, then the charged artificial cloud would not ſtrike to the point at any diſtance.

EXPERIMENT XII.

I placed the rod D, with the four-inch ball at the end as in the firſt experiment, this I put on a glaſs pillar to inſulate it; then from the rod I made a communication to the earth, with about three feet of ſilver wire, which was only $\frac{1}{800}$th part of an inch diameter: on charging the

the artificial cloud, it ſtruck to the ball D, as in the firſt experiment, *viz.* ſeventeen inches four tenths. Now as the wire was ſo ſmall which conducted the ſtroke, I thought I might be ſenſible of its paſſage if I held the wire between my fingers; I accordingly preſſed my fingers together, with the wire between, but there was not the leaſt ſenſation; nor ſhould I have known it had paſſed my fingers if I had not ſeen or heard the ſtroke from the artificial cloud to the ball with which the wire was connected. I then tried if it was viſible in the dark, but there was not the leaſt appearance of light, except where there happened to be kenks in the wire. I was accidentally very ſenſible of one of thoſe ſparks: for when I was trying the experiment in the dark, I happened to get ſo near as to receive the ſtroke juſt on my forehead; it made me reel till I fell againſt the wall. It may be proper to obſerve in this experiment, that if the fingers are held at a little diſtance from the wire, that a ſmall quantity of electric fire will ſtrike out to them the ſame as it does when conducted off by a larger quantity of metal.

OBSER-

OBSERVATION I.

From the three firſt experiments it appears, that our artificial cloud ſtrikes at diſtances greater as the termination of the conductor is more blunted, or as it terminates with the largeſt ball; and that the ſtriking diſtance is leſs as the end of the conductor tends more to a point; and in the fourth experiment, that when the end of the conductor is pointed, the point is not ſtruck at any diſtance whatever; but continues luminous to a certain diſtance, carrying off ſilently the electricity of our artificial cloud.

It ſeems from theſe experiments, that pointed conductors are to be preferred before thoſe terminating with a large ball, the pointed one depriving the cloud ſilently of its electric fire; whereas the ball receives the electric fire in a ſtrong ſpark. And in the fifth experiment, where a point projects but one tenth of an inch from a ball of four inches diameter, neither the ball, or point projecting from it, is ſtruck at any diſtance. This ſeems to ſhew the utility of a pointed rod, even if it projects but a ſmall diſtance above the higheſt part of a building.

The ſixth experiment ſhews, that a point within the ſurface of a ball does not prevent the ball being ſtruck. The ſeventh and eighth experiments likewiſe ſhew, that

our artificial cloud ſtrikes to a ball of four inches diameter, only at the diſtance of ſix inches and eight tenths, when the point is drawn out nine inches from the three inch and a half ball, placed oppoſite to the ſide of the artificial cloud; and that when the point projects only one inch, that then it ſtrikes to the four inch ball at ſixteen inches and four tenths diſtance.

May one not from theſe two laſt mentioned experiments conclude, that the more elevated our pointed conductors are, the greater is the chance of preſerving our buildings from the effects of lightning?

For here our point being elevated or projecting nine inches out of the ball, repreſenting the higheſt part of a building, was found continually depriving our artificial cloud of its electric fire to ſuch a degree (though it was kept charging all the time) that it would not ſtrike half the diſtance that it did when the point was elevated only one inch.

And from the ninth experiment we learn, that the conductor terminating in a point acts at a far greater diſtance than one terminating with a ball, in carrying off the electric fire, or matter of lightning from our artificial cloud. It muſt be further remarked, that though the point was luminous ſo far, yet there was no diſtance whatever at which our artificial cloud would ſtrike to it.

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Fom the tenth and eleventh experiments we learn, that the metallic part of our conductor being feparated or difcontinued is the reafon that our artificial cloud does ftrike to the point; and that it ftrikes further to the point as the number of the feparations are increafed; and that if the metallic communication with the moift earth be made compleat, that then our charged cloud will not ftrike to the point.

When a conductor to a building, terminating in a point, has been ftruck, I am inclined to think, that there had not been a compleat and fufficient metallic communication with moift earth; and from all the accounts I have met with, this feems to have been the caufe of their having been ftruck. From the twelfth experiment we learn, that a very fine wire will conduct a ftrong fpark.

Fig. 3. reprefents a moveable artificial cloud: it confifts of a hollow tube of wood, with a ball at each end, being together about fix feet in length: from each end was fufpended a light hollow wooden cylinder EE; thefe with the balls and tube were covered with tin-foil: it was placed with its axis refting on two femi-circular hollows in a piece of brafs fixed on a glafs pillar, by which it was infulated: it moved very eafily on its axis, and was brought to a horizontal pofition by means of two moveable pieces FF.

EXPERIMENT XIII.

I firſt put this moveable artificial cloud into an horizontal poſition, and placed it ſo that the braſs on which the axis reſted was in contact with the end of the artificial cloud B. Then, under each of the hollow cylinders EE, I placed a ſtand GG, having a good metallic communication with the earth. On one of the ſtands there was put a pointed wire, the ſame as was uſed in the fourth experiment; and on the other, a braſs ball of three inches diameter. I then placed the point and ball each twelve inches from the middle of the bottom of its correſpondent hollow cylinder: on charging the artificial cloud (which conſequently charged the moveable artificial cloud in contact with it) the point was luminous, and the moveable artificial cloud ſtill remained in an horizontal poſition, though there was now a point under one end and a ball under the other; and on ceaſing to charge the two clouds, it was found directly after, that the point had drawn off almoſt all the electric fire from both.

EXPE-

EXPERIMENT XIV.

The two clouds being charged, I took away the ftand with the three inch ball on it, the point remained luminous, and the moveable artificial cloud ftill continued horizontal, not being attracted to the point, though there was now only the ftand with the pointed wire under one end of it, the point having carried of the electric fire as in the laft experiment.

EXPERIMENT XV.

The two clouds being again charged, I replaced the ftand with the ball on it; and now, inftead of taking away this ftand, as I did in the laft experiment, I took away the ftand with the pointed wire on it: the confequence was, that the end of the moveable artificial cloud was now attracted down to the ball till it came to its ftriking diftance, where it then difcharged its electricity on it in a ftrong fpark (fee fig. 4.). The moveable artificial cloud then receded a little till it was charged, it then was attracted by the ball as before, till it came to its ftriking diftance, when it again difcharged its electricity at once, and fo continued ftriking and then receding to a little diftance as long as the two clouds were charged.

EXPERIMENT XVI.

The moveable artificial cloud continuing to ſtrike to the ball as in the laſt experiment, I now replaced the ſtand with the pointed wire on it, then immediately the point became luminous, and the moveable artificial cloud ceaſed ſtriking to the ball, and ſoon returned to its horizontal poſition as at firſt (ſee fig. 3.).

EXPERIMENT XVII.

The apparatus remaining as in the laſt experiment, and the two clouds continuing to be charged, I took away the ſtand with the point; then the moveable artificial cloud was attracted down to the ball, and ſtruck as before. I then placed the ſtand with the point cloſe to the ſtand with the ball; on which the point became inſtantly luminous, and immediately the moveable artificial cloud gave over ſtriking, ſoon returning from the ball and ſettling nearly in an horizontal poſition. There the point carried off the electric fire as in the thirteenth and fourteenth experiments.

OBSERVATION.

From the thirteenth experiment, with the point under one end of the moveable artificial cloud, and a three

inch ball under the other end, it ſeems as if neither the ball or point attracted either end; or that they both equally attracted, or repelled each end, as in either caſe the moveable artificial cloud would remain horizontal.

And in the fourteenth experiment, in order to try whether the point would attract or repel the moveable artificial cloud, the ball was taken away, and only the point was left under one end, as now all the action of the point either to attract or repel would be exerted on that end which was now over the point, and conſequently that end ſhould either be attracted down to it, or repelled from it: but from the experiment it appears, that the point drew off all the electricity ſilently, without either attracting or repelling the end of the moveable artificial cloud which was over it, as it continued horizontal all the time it was charged.

The fifteenth experiment was made to ſee if the ball would either attract or repel the moveable artificial cloud, as in this experiment the ball only was under one end, and every thing elſe exactly the ſame as when the point only was under. But here we find the effect of the ball very different from that of the point; for inſtead of drawing off the electricity ſilently, as the point did, without attracting the end of the moveable artificial cloud; on the contrary, the moveable artificial cloud was

 attracted

attracted down towards the ball, till it came within its ſtriking diſtance, where it diſcharged its electric fire all at once on the ball with a loud and ſtrong ſpark.

And again, in the ſixteenth experiment, where the ſtand with the point is replaced at the other end, whilſt the cloud is attracted down to the ball, it inſtantly prevents its ſtriking to the ball by carrying off the electric fire as faſt as the moveable artificial cloud receives it from the artificial one.

And from the ſeventeenth experiment we learn, that when the ſtand with the point is placed cloſe to the ſtand with the ball, whilſt the moveable artificial cloud is ſtriking to it, the cloud even in this caſe inſtantly ceaſes to ſtrike to the ball, returning from it and ſoon ſettling nearly in an horizontal poſition.

EXPERIMENT XVIII.

I took off the cylinders EE from the ends of the moveable artificial cloud (the height of my room not allowing them to be ſuſpended in the following experiments); I then placed it, together with the glaſs pillar whereby it was inſulated, upon another foot of ſuch a height that when the ball at one of the ends was three inches above the ball C at the end of the artificial cloud, then the

moveable

moveable artificial cloud was horizontal. I then placed the ftand with the point on it at the diftance of eighteen inches, and directly under the ball at the other end (fee fig. 5.): on charging the artificial cloud, the point was luminous; and that end of the moveable artificial cloud which was three inches above the ball c was attracted down to it, then receded from it about one inch, and then the artificial cloud kept conftantly ftriking to it, as long as it continued to be charged. On ceafing to charge the artificial cloud, it was found immediately after, that the point had carried off almoft all the electric fire.

EXPERIMENT XIX.

Every thing remaining as in the laft experiment, and the artificial cloud being charged, I took away the ftand with the point, and placed in its ftead the ftand with the three inch ball on it, exactly at the fame diftance as the point: then inftantly that end of the moveable artificial cloud, which had continued to be attracted down near to the artificial cloud, was repelled from it, and at the fame time the other end was attracted by the three inch ball till it came fo near as to difcharge its electricity on it in a ftrong fpark. The end of the moveable artificial cloud then receded from the three inch ball,

 the

the other end being now attracted by the artificial cloud, which charged it almoſt inſtantly again; it then receded with rapidity from it, and diſcharged its electric fire on the ball as before, and thus continued in great motion receiving ſtrong ſparks from the artificial cloud, and diſcharging them on the ball, repreſenting in miniature a ſtorm of lightning where an electrical cloud ſtrikes into another cloud, and that diſcharges itſelf on a building that is without a regular conductor, or one terminating with a ball (ſee fig. 6.).

EXPERIMENT XX.

While this ſtorm of lightning in miniature continued, I removed the ſtand with the three inch ball, and placed in its ſtead the ſtand terminating with the point; the point was immediately luminous, and in an inſtant the artificial ſtorm ceaſed.

The end of the moveable artificial cloud, next the charged artificial one, was now attracted to it, as in the eighteenth experiment.

EXPERIMENT XXI.

The apparatus remaining as in the laſt experiment, I unſcrewed the pointed wire from the ſtand, and ſcrewed

it into one end of a ſtick of wax of ſix inches in length, with eleven pieces of tin-foil ſtuck on it at one fortieth part of an inch aſunder; then I ſcrewed this wax with the point on the ſtand, and placed it ſo that the point was directly under the end of the moveable artificial cloud, and at eighteen inches diſtance as before: on charging the artificial cloud, the moveable artificial cloud was firſt attracted, then repelled, and ſo alternately as when the ſtand with the three inch ball was under; but with this difference, that inſtead of ſtriking in a ſtrong ſpark, as it did to the three inch ball, it now ſtruck with a very ſmall ſpark to the point, the point depriving the moveable cloud of moſt of its electricity as it approached it, which was very viſibly paſſing away between the ſeparations of the tin-foil.

EXPERIMENT XXII.

Every thing as in the laſt experiment, a chain only being hung on the pointed wire, thereby compleating the metallic communication with the earth. As ſoon as the chain was hung on, the moveable artificial cloud inſtantly ceaſed ſtriking to the point, and the other end of it was then attracted to the artificial cloud, which then kept conſtantly ſtriking to it: the moveable artificial cloud

cloud did not return to the point as it did before the chain was hung on, as in the laſt experiment.

OBSERVATION.

In the eighteenth experiment, where the moveable artificial cloud was intended to repreſent a cloud in its natural ſtate receiving electric fire from a charged cloud, we find, that the point deprived it of its electric fire which it received from the charged one ſo faſt, that the artificial cloud could keep conſtantly ſtriking to the other end, without repelling it from it; but that in the nineteenth experiment, when the ball was under the end of the moveable artificial cloud in the place of the point; then, inſtead of the artificial cloud continuing to ſtrike to the other end without repelling that end, it now firſt attracted and charged it with electricity, or the matter of lightning; then immediately repelled it, and being attracted by the ball under the other end, it moved down with an acquired velocity, till it came within its ſtriking diſtance, diſcharging then its electricity on the ball with a loud and ſtrong ſpark, and ſo continued alternately receiving and diſcharging its electric fire on the ball. It being firſt attracted, at which time it received an additional quantity of electricity, and then repelled till it had diſcharged that

that additional quantity, is exactly agreeable to all the known laws of electricity.

This experiment may possibly throw some light on what we sometimes see in nature, *viz.* one cloud continuing to strike towards the earth a considerable time; for should a cloud in its natural state be so situated between a charged cloud and the earth, it may be first attracted and charged, and then repelled, and if it should be repelled so as to come within the attracting distance of any blunt body with a good or partial conductor, it would then continue to be attracted till it came within its striking distance, and then discharge its lightning suddenly on it; and if it was not repelled or attracted beyond the attracting distance of the charged cloud, it would again be attracted to it and charged, then repelled as before, and so may continue receiving and discharging the lightning till the charged cloud is nearly exhausted of its electricity or matter of lightning.

But if a cloud, in its natural state, should be so situated within the striking distance of a charged cloud, and at the same time within the power of a good metallic conductor terminating in a point; then from these experiments it does appear, that the charged cloud would continue striking to the natural cloud, and that would again

part

part with it silently, by means of the point, without striking on it till the charged cloud is nearly exhausted.

When we see a cloud striking into another cloud several times together, we conclude from all the known laws of electricity, that the cloud which first received the stroke must have discharged part or the whole of what it received before it could receive another stroke.

In the twentieth experiment we find, that though our moveable artificial cloud was in great motion, receiving and discharging its electric fire on the ball, that, on taking away the ball, and putting the point in its place, the artificial storm immediately ceased.

In the twenty-first experiment, where the point was on a stick of wax, with separations in the metallic communication with the earth, we find that, even in that situation, the stroke on the point was very small to what it was on the ball with a good communication, great part of the electric fire visibly passing off as the cloud approached the point; and when the metallic communication was made compleat by hanging on the chain, it then ceased striking to the point.

EXPERIMENT XXIII.

The tube which I before called the moveable artificial cloud in the former experiments, from its moving very eaſily on its axis, was, by means of two ſcrews now fixed, immoveable, with the ball at one of its ends above the ball c at the end of the artificial cloud, at the height of three inches; and underneath the ball, at the other end, was placed the ſtand with the point, at the diſtance alſo of three inches. The artificial cloud was then charged, and an electric ſpark ſtruck from the ball c at the end of it to the ball of the now fixed cloud above it, and at the ſame inſtant ſtruck from the ball at the other end to the point at three inches.

EXPERIMENT XXIV.

The tube uſed in the laſt experiment (which I now again call the moveable artificial cloud from its being made again to move freely on its axis) was placed exactly in every reſpect as in the laſt experiment; the only difference was, that it could now move eaſily on its axis, whereas in the laſt experiment it was fixed immoveable at the diſtances: on charging the artificial cloud, the moveable artificial cloud, inſtead of receiving a ſpark,

 and

and diſcharging it on the point, as in the laſt experiment, was now attracted down to the artificial cloud there remaining, not ſtriking to the point, or returning to it ſo long as the artificial cloud continued to be charged.

OBSERVATION.

By the twenty-third experiment we ſee, that if our cloud is fixed at a certain diſtance between the artificial cloud and the point, the fixed cloud, at the inſtant it receives the electric ſpark, directly diſcharges it again on the point. But in the twenty-fourth experiment, where there is no other alteration than making the cloud moveable on its axis, the diſtances being exactly the ſame, the end of the cloud then recedes from the point and will not ſtrike to it. This twenty-fourth experiment is much more agreeable to nature than the twenty-third, for clouds are not fixed but floating bodies.

In order to ſee the effect of rods terminating with balls of different ſizes, or terminating with a point, moving ſwiftly under my artificial cloud, I made uſe of the following apparatus.

In fig. 7. H is a hollow tube of wood covered with tinfoil, with a heavy weight faſtened to one end of this tube; and at about three inches above the weight was

an axis, it was then fuſpended by this axis between two wooden pillars: in this wooden tube was a braſs rod, which was moveable, ſo that a ball or point fixed on it could be raiſed to the height required.

EXPERIMENT XXV.

A ball of one inch and three tenths diameter was fixed to the under part of the artificial cloud at K, and then this apparatus was placed under it with a point, the ſwinging rod was held down to the floor, as in fig. 8. and the point covered: then the artificial cloud was charged by a certain number of turns of the glaſs cylinder; the ſwinging rod with the point was then let go, and paſſed ſwiftly and very near to the ball under the artificial cloud at K. This was repeated ſeveral times, removing the point lower each time till the greateſt ſtriking diſtance to the point was found, which was generally one inch and ſix tenths.

EXPERIMENT XXVI.

The point being removed, a ball of three tenths diameter was placed in its ſtead and tried, as the point in the preceding experiment; the ſtriking diſtance was generally found to be two inches and one tenth.

EXPERIMENT XXVII.

The three tenth ball being removed, another of one inch and three tenths was tried as in the two laſt experiments, and the ſtriking diſtance was generally fifteen inches.

But when the weather has been favourable for electrical experiments, I have ſeveral times had ſtrokes to the point from its paſſing ſwiftly, and as near as it could without touching the ball, till it was brought down to one inch and ſeven tenths, then the artificial cloud would ceaſe ſtriking to it till it was removed down to three inches and five tenths; it would then begin ſtriking again, and continue ſtriking to it all the while it was removing to the diſtance of ten inches and three tenths.

And when the three tenth ball was on in the place of the point, the artificial cloud would ſtrike to it from its paſſing ſwiftly, and very near to the ball, and continue ſtriking all the while it was removing to the diſtance of two inches and nine tenths; then the artificial cloud ceaſed ſtriking to the ball till it was removed to three inches and ſeven tenths, and after that diſtance continued ſtriking till it was removed down to ten inches and eight tenths. But when a ball of one inch and three

tenths

tenths was ufed, the artificial cloud has ftruck, as it paffed fwiftly, very near to the ball on it, and all the time it was removed down to fixteen inches, there being no diftance with this one inch and three tenth ball at which the artificial cloud left off ftriking, and then began again; but here with the point and three tenth ball there were two ftriking diftances, as was before mentioned in the fecond experiment.

This remarkable phenomenon in electricity is, I believe, new to electricians, and may be worthy their confideration.

OBSERVATION.

In the twenty-fifth experiment it appears, that the point is ftruck by means of a fwift motion; and from the twenty-fixth experiment, that the ball of three tenths was ftruck further than the point; and the ball of one inch and three tenths, in the twenty-feventh experiment, at a much greater diftance than either, even with the fwift motion.

From thefe experiments I fhould be induced, firft, to prefer elevated pointed conductors; next to them thofe that are pointed, though they project but a little diftance above the higheft part of the building; and after them

thofe

thoſe terminating in a ball, and placed even with the higheſt part of a building, though it does appear from theſe experiments, that they are more liable to be ſtruck, and likewiſe have not the property of guarding the diſtant parts of a building as elevated points have; but if they have a good metallic communication with the earth, the building might not be hurt, though the lightning ſhould ſtrike on the conductor; yet, I believe, there are not many who would not ſhudder at the tremendous blow, if they were in a houſe when the conductor was ſtruck. Thoſe conductors which are recommended to be within the inſide of a building, and one or two feet below the higheſt part[(d)], are certainly very dangerous, eſpecially for all that part of the building above the conductor.

I was a witneſs of the dreadful effects of a ſtroke of lightning on a houſe that had an accidental partial conductor within the inſide of the upper part of the houſe.

It happened to a houſe near Ratcliff Highway, on the 29th of July, 1775. In the uppermoſt room ſtood a large iron triblet, of about three feet in height; the lightning made its way through the roof of the houſe, throwing off a number of tiles, rending and tearing the laths and plaſter on the inſide, to get to the triblet, on which it ſtruck from thence to a hammer, which laid on the

(*d*) Mr. WILSON's Letter to the Marquis of ROCKINGHAM, Phil. Tranſ. vol. LIV. p. 247.

floor

floor near it: it then made its way, by partial conductors, down into the cellar to the leaden pipe, which conveyed water from the main, and in its way rent the houſe in various parts, ſo as to make it ſcarcely habitable. It left marks of fuſion on different metallic utenſils, ſome of which I have now in my poſſeſſion. If the conductor from the triblet had happened to have been made by a compleat and ſufficient metallic communication with the earth, all parts of the houſe below would have been preſerved; but the parts above would have been equally rent and deſtroyed.

I now beg leave to make a few remarks on Mr. WILSON's paper, intitled, New Experiments and Obſervations on the Nature and Uſe of Conductors. In p. 2. Mr. WILSON mentions, that he had declared his diſſent in the year 1772 againſt pointed conductors: I will here copy part of his diſſent as it is in Phil. Tranſ. vol. LXIII. p. 48. His words are, "Every point, as ſuch, I conſider as "ſoliciting the lightning, and by that means not only "contributing to increaſe the quantity of every actual "diſcharge, but alſo frequently occaſioning a diſcharge "where it might not otherwiſe have happened. Whereas, "if inſtead of pointed we make uſe of blunted conduc-"tors, thoſe will as effectually anſwer the purpoſe of "con-

"conveying away the lightning safely, without that ten-"dency to increase or invite it."

In answer to this I can only say, that, from these experiments of mine, the direct contrary appears to be the fact; that the point, instead of increasing an actual discharge, prevents a discharge where it otherwise would happen; and that the blunted conductors tend to invite the clouds charged with lightning.

The eleven first experiments of Mr. WILSON's are intended to shew, that pointed conductors draw off the electricity from a cloud at a much greater distance than those which are blunted. My ninth experiment proves the truth of those experiments of his; the only difference is, that in mine the point acted on my artificial cloud at a much greater distance; from which it appears, to use his own words, p. 4. "that a charged body is exhausted "of more of the fluid by a pointed than by a blunted "conductor." In answer to his twelfth experiment, and on to the eighteenth, where the model of the house moved swiftly, under his large artificial cloud, and where the point was struck at five inches, and sometimes at a quarter of an inch further than his three tenth ball[e]; I must observe, that I have sometimes seen his apparatus at the Pantheon, with which he made his experiments,

(e) Page 8.

strike

ſtrike as far to the three tenth ball as the point; but in my experiments I have had it ſtrike ten inches three tenths to a point, and ten inches and eight tenths to a three tenth ball; but to a one inch and three tenths ball it commonly ſtruck to fifteen inches, and ſometimes to ſixteen inches. In anſwer to the eighteenth and following experiments I muſt obſerve, that the ſubſtitute being fixed is unnatural; for clouds are compoſed of a fluid matter, moving with the utmoſt facility in another fluid ſubſtance; and from my twenty-third experiment, where the ſubſtitute was fixed, the point was ſtruck; yet in the twenty-fourth experiment, where there was no other alteration than allowing the cloud to move freely, then the point was not ſtruck. I imagine, if Mr. WILSON's large artificial cloud at the Pantheon, which was 155 feet long and 16 in diameter, had been properly inſulated, and there had been ſeveral cylinders properly mounted to have charged it, he would have found the ſtriking diſtance, and every other of his experiments, very different from what he did, particularly thoſe where his ſubſtitute was fixed about one inch and a half from his large artificial cloud.

My reaſons for thus thinking are, that when I placed a ſubſtitute of exactly the ſame dimenſions in every reſpect as his, and placed it alſo about one inch and a half

 from

from my artificial cloud, that then the longeſt ſpark that I could get to a point was one inch and one tenth; but to a three tenth ball it ſtruck eight inches and ſeven tenths: and what further confirmed me in my opinion was, that when I placed a braſs cone, half an inch at the baſe and two inches high, on my artificial cloud, to carry off part of its electricity, in order to prevent its being charged ſo high, every thing elſe being the ſame, that then I could get a ſtroke to the point one inch and one tenth as before, and ſometimes not longer to a three tenth ball; but to a one inch and three tenth ball the diſtance was leſs, being not more than half an inch. But when I made no other difference than taking off the cone from the artificial cloud, it then ſtruck to the point as before, *viz.* one inch and one tenth, and to the three tenth ball eight inches and ſeven tenths; but to the one inch and three tenth ball nine inches and one tenth, inſtead of only half an inch, as it did when the cone was on, and of conſequence the conductor not ſo highly charged. If the ſubſtitute was placed in contact with the artificial cloud, then there was no diſtance at which it would ſtrike to the point, but only to the balls, as Mr. WILSON obſerves was the caſe with his apparatus. His words are, p. 11. "So that bringing the two ſubſtitutes into con-"tact occaſions the ſame phenomena that the great cy-

 "linder

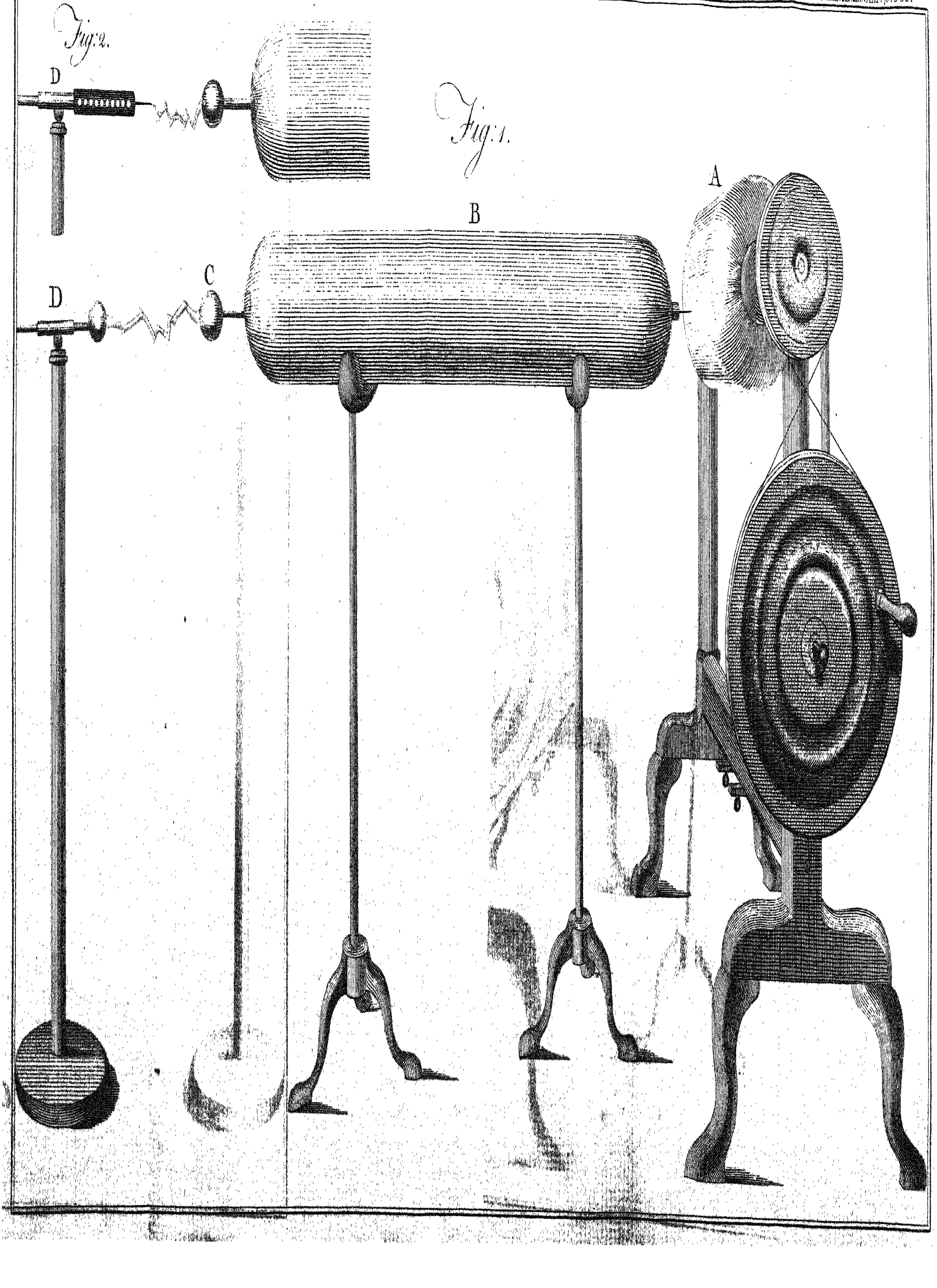
"linder did alone; that is, the rounded end would cauſe "an exploſion at a conſiderable diſtance, and the point "little or none, notwithſtanding it was brought cloſe to "the ſubſtitute."

I muſt beg to intrude a little more on your time to remark on that part of Mr. WILSON's paper, where from his experiments he ſeems to conclude, that the lightning at Purfleet firſt ſtruck on the point of the rod of the conductor, and then, by a lateral part of that ſtroke, ſtruck the cramp on the coping ſtone. I believe, if he had examined the ſituation of the ſtone, and the place where the cramp was ſtruck, he would have found, that if the lightning had ſtruck on the point of the conductor, that to have produced that effect on the ſtone, it muſt after it had ſtruck on the point, and paſſed down a quantity of metal, have ſtruck from the metal up into the air, then down again on the cramp, and then again to the metal it had left, for the ſmall dent or hollow made by the lightning was on the upper ſurface of the ſtone, and yet the metallic communication to the earth continued from the point under the ſtone which was ſtruck. It appears more probable to me, from the trifling damage it did, that the charged cloud had paſſed over the pointed conductor, and had been exhauſted of a great part of its electricity in paſſing; and that after it had paſſed,

 it

it was attracted down lower by a ridge of hills that was beyond, and that the cloud being out of the influence of the point to prevent its ſtriking, the end of the cloud might ſtrike at an angle in the cramp, and ſo to the metallic part of the conductor, which was only about ſeven inches below.

I ſhall conclude with obſerving, that Mr. HENLY and myſelf had the pointed rod of the conductor at Purfleet taken down to examine the point; but we found no appearance on it that ſhewed that it had been ſtruck.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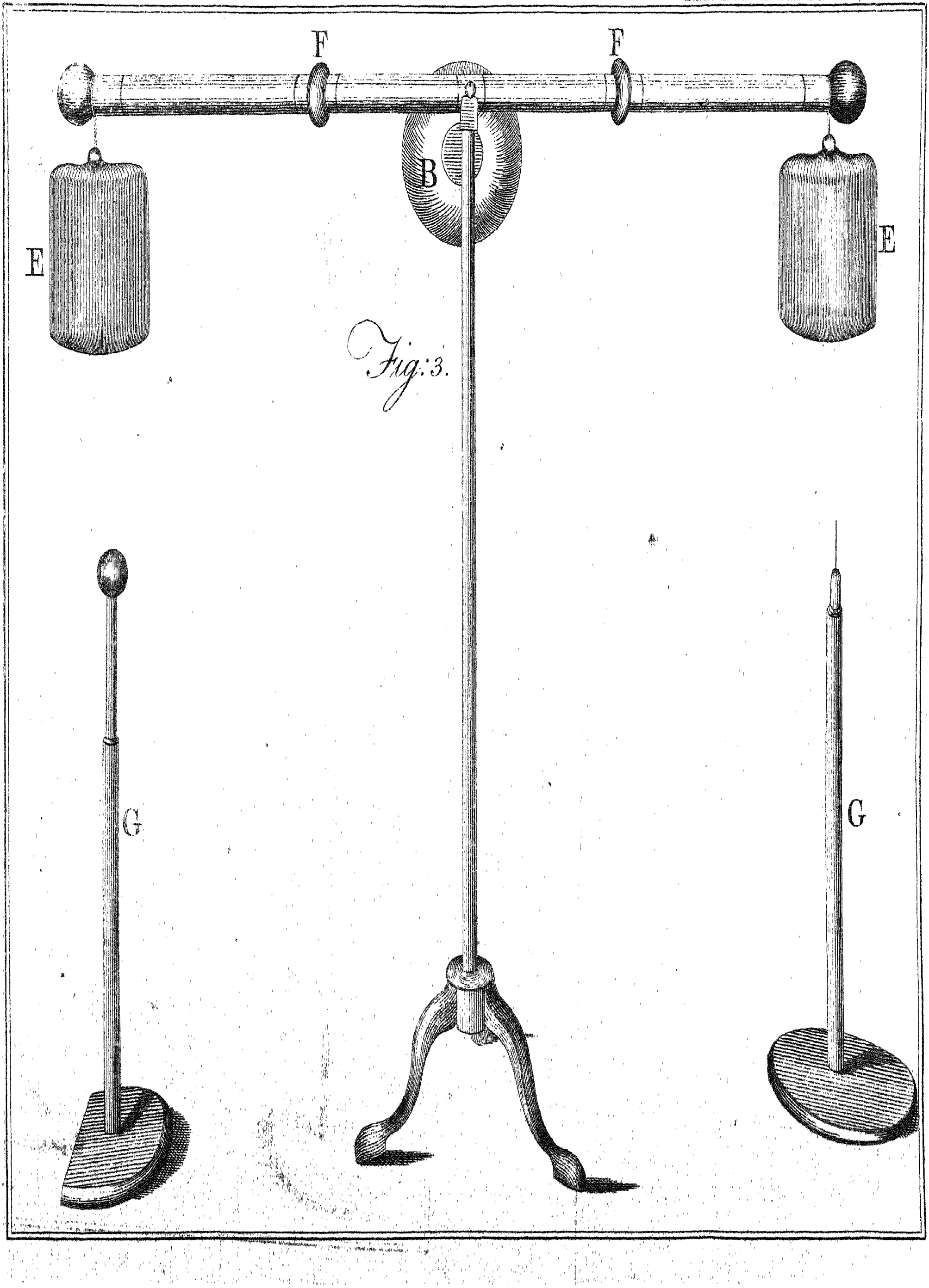
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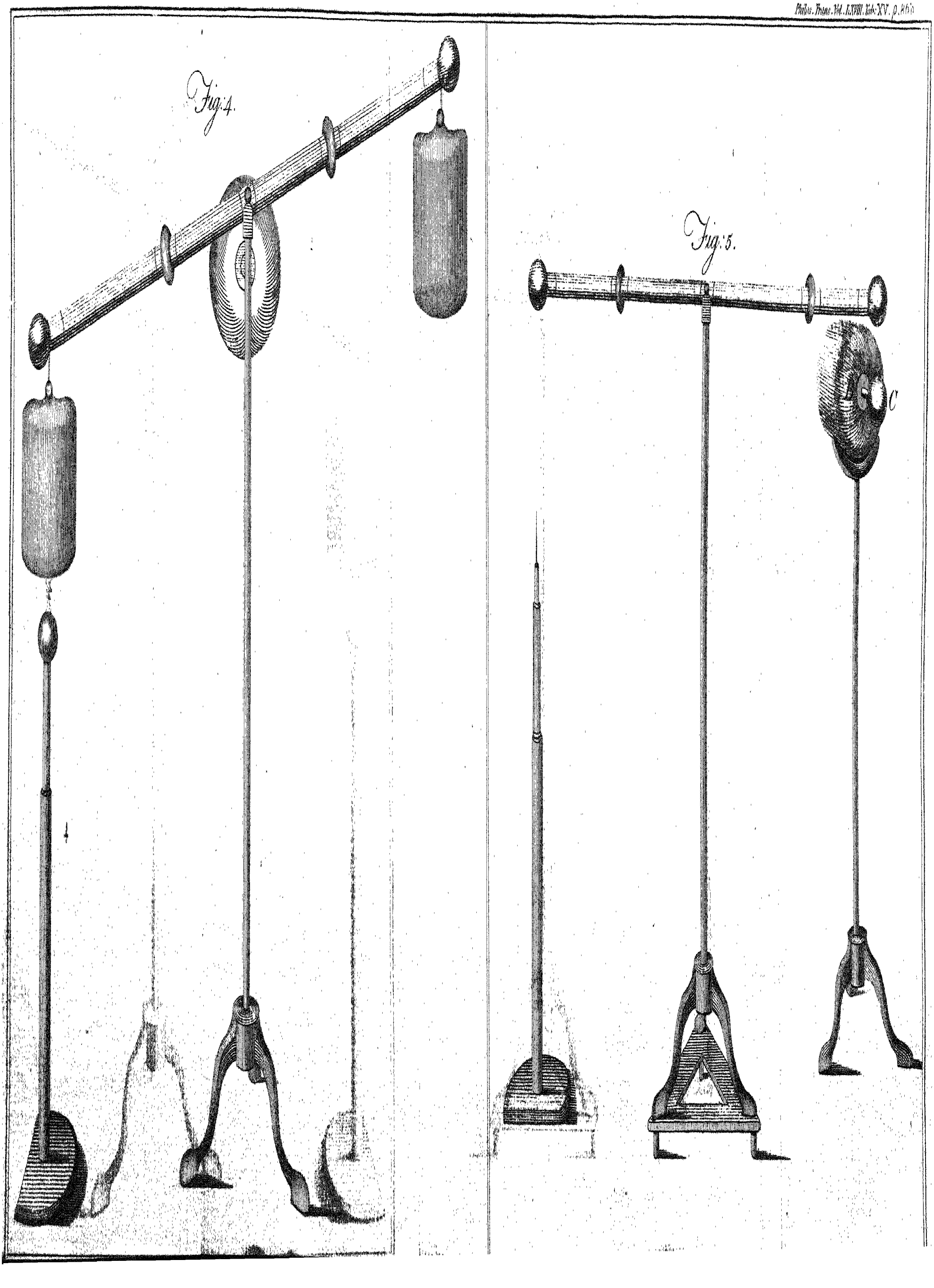
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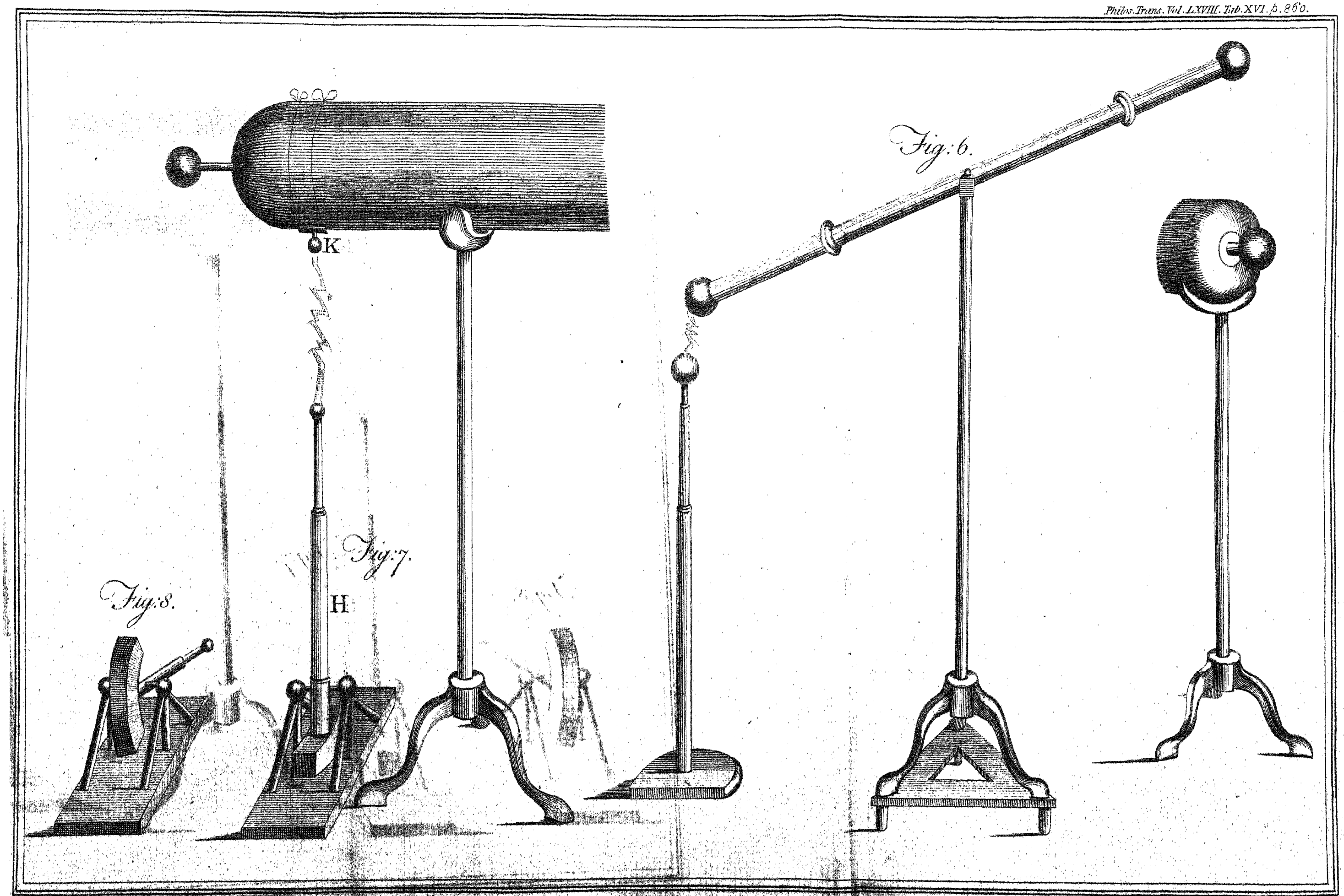
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Fig: 7.
Fig: 8.
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XXXVIII. *On the Uſe of an Amalgam of Zinc, for the Purpoſe of electrical Excitation*, &c. *By* Bryant Higgins, *M. D. in a Letter to* Richard Brocklesby, *M. D. F. R. S.*

TO DR. BROCKLESBY.

DEAR SIR,

Greek Street, Soho, May 8, 1778.

Read July 4, 1778.

BY divers experiments lately made by myſelf, and repeated by others, I find that, agreeable to the ſuggeſtion made in my laſt courſe of chemiſtry, the amalgam of zinc, which contains four times more quickſilver than zinc, is much better for electrical excitation than the tin amalgam of the ingenious Mr. CANTON, when uſed in the ſame circumſtances.

I alſo find, that electrical cylinders are eaſily and effectually cleanſed by applying to them a piece of the dry ſkin of the dog-fiſh whilſt the cylinders are turned

 round;

round; and that in this method of cleanſing the glaſs cylinders, we avoid the inconvenience of removing the cuſhion, and the danger of ſcratching the glaſs, to both which we are expoſed in the uſe of whiting and other cleanſing powders.

I am, &c.

XXXIX. *Chemical Experiments and Obſervations on Lead Ore. By* Richard Watſon, *D. D. F. R. S. in a Letter to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R, Cambridge, June 13, 1777.

Read July 9, 1778.

THE following experiments and obſervations, it is apprehended, will not be thought unintereſting by perſons verſed in chemiſtry. May I beg the favour of you to communicate them to the Royal Society?

I am, &c.

LEAD ORE, as dug out of the mine, is generally much mixed with ſpar, lime-ſtone, and other ſubſtances, bulk for bulk, heavier than the ore itſelf. It undergoes various dreſſings before it becomes a merchantable commodity, the general tendency of which is to free it, as much as poſſible, from every heterogeneous impurity.

Suppoſe

Suppofe that a cubic foot of lead ore, which contained no fpar or other extraneous matter, would weigh 7800 ounces, and that a cubic foot of fpar, which contained no lead ore or other foreign fubftance, would weigh 2700 ounces, then would a mixture, confifting of a cubic foot of pure lead ore and a cubic foot of pure fpar, weigh 10500 ounces, and one cubic foot of fuch a mixture would weigh 5250 ounces. It is obvious that, according to the different proportions in which the particular kinds of fpar and lead ore here affumed are fuppofed to be mixed together, a cubic foot of the mixture will have different weights, the limits of which are on the one hand 7800, and on the other 2700 ounces; it never can weigh fo little as 2700 ounces, for then it would confift intirely of fpar without any lead ore; nor can it ever weigh fo much as 7800 ounces, for then it would confift intirely of lead ore without any fpar.

From this view of the matter it is evident, that the purchafing of lead ore by the meafure, which is the general though not the univerfal cuftom in Derbyfhire, is a mode liable to fome exception; fince a difh, containing any definite meafure, muft have different weights, according as the ore with which it is filled is more or lefs free from fpar. And it is fcarce poffible, by repeated

dreffings,

dreſſings, to ſeparate all the ſpar from an ore, or equal portions of it from equal portions of ore.

There is a diverſity, however, in the weights of equal meaſures of lead ore, which probably does not ariſe from ſparry or other heterogeneous accretions, but from the nature of the ore itſelf. I have carefully taken the ſpecific gravities of many of the Derbyſhire lead ores; the weight of a cubic foot of the lighteſt which I met with was 7051 ounces, and the weight of a cubic foot of the heavieſt was 7786 ounces; the difference amounting to between a ninth and a tenth part of the weight of the lighteſt. There are probably other ores of lead which differ more in their ſpecific gravities than theſe here mentioned; but the difference between theſe is ſufficient to ſhew the great uncertainty of purchaſing lead ore by the meaſure, ſince ten diſhes of one ſort of ore may not weigh more than nine diſhes of another ſort, though both the ſorts be equally well dreſſed.

Lead ore is not always of the ſame goodneſs in the ſame mine, nor even in the ſame part of the ſame mine; and, what is more remarkable, the different parts of the ſame lump of ore have different ſpecific gravities. I could not eaſily have believed this, unleſs a variety of experiments had convinced me of the fact.

They were employed this year at Holywell in ſmelting a lead ore from the Iſle of Man; the ore was rich in ſilver. A lump of this ore, weighing about ten ounces, was broken into ſeveral pieces, and ſuch of the pieces were ſelected as appeared to the eye to be wholly pure. By taking the ſpecific gravities of ſix of theſe pieces I found, that a cubic foot of the lighteſt kind would have weighed 6565 ounces, and a cubic foot of the heavieſt kind would have weighed 7636 ounces. Suppoſing the ſpecific gravity of water to be denoted by 1,000, the mean ſpecific gravity of the ſix different pieces of this ore may be expreſſed by 7.115.

A very pure ſpecimen of teſſellated lead ore, from a mine near Aſhover in Derbyſhire, was broken into ſix pieces, weighing near one ounce each. A cubic foot of the lighteſt of theſe pieces would have weighed 7326 ounces, and a cubic foot of the heavieſt would have weighed 7786 ounces. The mean ſpecific gravity of the ſix pieces was 7.566.

At the ſame mine they frequently meet with ſmall quantities of ſteel-grained lead ore. Six different pieces of the ſame lump of this kind of ore were choſen, each of which appeared quite free from ſpar and every other impurity. A cubic foot of the lighteſt of theſe pieces would have weighed 7188 ounces, and a cubic foot of

the heavieſt would have weighed 7442 ounces. The mean ſpecific gravity of the ſix pieces was 7.342.

Other lumps of ore, from different mines, were reſpectively broken into different pieces, and ſcarcely any two pieces of the ſame lump were obſerved to agree in their ſpecific gravities. This diverſity in the ſpecific gravities of the ſeveral pieces of the ſame lump of ore may be owing, either to the different proportions in which the conſtituent parts of the ore are combined in the ſeveral pieces; or to the different quantities of extraneous ſubſtances imperceptibly mixed with them, or, which ſeems moſt probable, to a diverſity in the ſize or configuration of theſe pores.

But be the cauſe of the diverſity in the ſpecific gravities of different pieces of the ſame lump of ore what it may, the fact, I believe, is certain, and by no means ſingular; for not to mention the varieties obſervable in the ſpecific gravities of different pieces of roll brimſtone, of corroſive ſublimate, of caſt ſteel, and other factitious ſubſtances, the natural ſpars generally found along with lead ore are ſubject to a ſimilar diverſity, though not perhaps in an equal degree.

A piece of rhomboidal, otherways called refracting or lantern ſpar, was broken into four ſmaller pieces, the ſpecific gravities of which were 2.675, 2.687, 2.715,

 2.723;

2.723; the medium of the four is 2.700. Mr. COTES fixes the ſpecific gravity of Iceland cryſtal at 2.720, and WALLERIUS fixes it at 2.700.

The ſpecific gravities of four pieces of the ſame lump of cubical ſpar were 3.204, 3.218, 3.222, 3.231; the medium of the four is 3.219.

Six ounces of fine teſſellated lead ore were put into a crucible and expoſed, at firſt, to a gentle, afterwards to a ſtrong fire: the ore grew red, and emitted fumes which ſmelled of ſulphur; at length it melted, and the fumes became very copious; they were accompanied with a yellowiſh flame upon the ſurface of the melted ore, and when collected had a whitiſh appearance. The crucible after the ore had continued a full hour in perfect fuſion was taken from the fire, and when it was cold it was broken. The maſs which it contained weighed five ounces and an half; there were no ſcoria obſervable on its ſurface, nor were any particles of metal formed, it was ſtill an ore of lead.

The maſs remaining from the laſt experiment was put into a freſh crucible, and expoſed to a ſtrong melting heat; the fumes which aroſe from it ſeemed to be heavy; they brooded over the ſurface of the melted maſs in undulating flames, which now and then appeared like burning zinc. The lead was now formed, and many particles

particles of it were sublimed to at least six inches above the surface of the liquid in the crucible. After letting the crucible continue two hours in this state, I poured out its contents, and found them consisting partly of lead, partly of lead ore, and partly of a very minute portion of brownish scoria.

These experiments prove, that some substance or other is contained in lead ore, which must be dispersed before the ore can be formed into lead; and they shew too, that it requires a considerable time to effect the dispersion of this substance, since six ounces of ore, though kept three hours or more in complete fusion, were not wholly brought into the form of lead; and, lastly, they render it probable, that the fumes, arising from melted ore, carry off with them no inconsiderable portion of the lead itself. At the great smelting houses in Derbyshire, they put a ton of ore at a time into the furnace, and work it off in eight hours; the ore might be wholly melted in one hour, but the lead perhaps is not formed in the greatest possible quantity in eight hours.

Some fine tessellated lead ore from Derbyshire was pounded into small lumps, each about the size of a pea, and carefully picked from spar and other impurities. Sixteen ounces of this ore, thus previously cleansed, were distilled in an earthen retort; as soon at the ore felt the

 fire,

fire, the ſtopple of the quilled receiver had a ſtrong ſmell reſembling that of the inflammable air ſeparable from ſome metals by ſolution in acids; ſoon after a ſmall portion of a liquid came over into the receiver; the fire was then raiſed till the retort was of a white heat, when a black matter began to be ſublimed into the neck of the retort; the operation was then diſcontinued. This experiment was undertaken with a view of ſeeing whether ſulphur could be ſeparated from lead ore, as it may be from ſome ſpecies of the pyrites, by diſtillation, and it appears from the iſſue of the experiment that it cannot. What might have been the event of the experiment if it had been conducted with a very gentle heat for a long time, I cannot pretend to ſay. Upon breaking the retort I found, that the ore had been melted during the operation, for there was a conſiſtent cake of ore of the figure of the bottom of the retort; the weight of this cake was fifteen ounces and an half, the weight of the liquid in the receiver, and of the black matter which had been ſublimed, did not together amount to one quarter of an ounce, ſo that a quarter of an ounce or more had been diſperſed, probably in the form of air, or ſome elaſtic fluid. The ore by this proceſs had loſt one thirty-ſecond part of its weight; but I am of opinion, that if the operation had been conducted with a leſs degree of heat and

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continued for a longer time, the quantity of liquid would have been augmented. The liquid did not effervefce with either acids or alkalies; nor did it produce any change in the colour of blue paper, though I am certain, from experiment, that one drop of oil of vitriol, though diluted with two ounces of water, would have produced a fenfible rednefs on the blue paper which I ufed. The liquid, notwithftanding, had an acid tafte, and a pungent fmell, refembling that of the volatile vitriolic acid. The experiment ought to be repeated with a larger quantity of ore, in order to afcertain the quantity and quality of the liquid, feparable therefrom by fimple diftillation. The black matter which had been fublimed into the neck of the retort, was examined with a microfcope, and it appeared to be pure lead ore; hence it is probable, that by a due degree of heat in clofe veffels, lead ore might be intirely fublimed without being decompofed; for the melted ore which was found at the bottom of the retort, had not any appearance of either fcoria, or of lead, upon its furface. Finding that fulphur could not be feparated from lead ore by diftilling it without addition, and yet being much difpofed to think, that it contained a confiderable portion of fulphur, I firft thought of diftilling it with charcoal duft, iron filings, fand, and other additions; but recollecting that fulphur might be feparated from anti-

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mony by ſolutions in acids, I thought it not improbable, that it might be ſeparated from lead ore by the ſame means, and the ſucceſs of the following experiment abundantly juſtified the conjecture.

Upon ten ounces of lead ore, cleanſed as in the preceding experiment, I poured five ounces of the ſtrongeſt fuming ſpirit of nitre; this ſtrong acid not ſeeming to act upon the ore, I diluted it with five ounces of water; a violent ebullition, accompanied with red fumes, immediately took place; the ſolution of the ore in this menſtruum became manifeſt, and when it was finiſhed, there remained floating upon the ſurface of the menſtruum a cake of fine yellow ſulphur, perfectly reſembling common ſulphur.

I repeated this experiment a great many times, in order to aſcertain the quantity of ſulphur contained in lead ore, and ſeparable therefrom by ſolution in acid of nitre. The reſults of different experiments were ſeldom the ſame: the matter ſeparable from the ore by ſolution, after being repeatedly waſhed in large quantities of hot water, in order to free it from every ſaline admixture, ſometimes amounted to more, ſometimes to leſs than one-third the weight of the ore. This matter may, for the ſake of diſtinction, be called crude ſulphur. Its apparent purity might induce a belief that it contained no hetero-

heterogeneous mixture, yet the following experiments ſhew how much we ſhould be deceived in forming ſuch a conjecture, and how rightly it is denominated crude ſulphur.

From one hundred and twenty parts, by weight, of lead ore, I obtained, by ſolution in acid of nitre, ſubſequent waſhing in hot water, and drying by a gentle fire, forty parts of a ſubſtance which looked like ſulphur: theſe forty parts were put on a red-hot iron, the ſulphur was made manifeſt by a blue flame and pungent ſmell. When the flame went out, there remained upon the iron unconſumed twenty-ſix parts of a greyiſh calx; the weight of the ſulphur which was conſumed muſt therefore have amounted to fourteen parts, or between one eighth and one ninth part of the weight of the ore. It has been obſerved, that the weight of the matter, ſeparable from lead ore by ſolution in acid of nitre, ſometimes exceeded, and ſometimes fell ſhort of, one third part of the weight of the ore; this variety, as far as I have been able to obſerve, does not extend to the quantity of ſulphur contained in a given quantity of ore, but depends upon the quantity of calx remaining after the burning of the ſulphur. Different lead ores will, doubtleſs, contain different quantities of ſulphur; but that the ſulphur contained in the lead ore which I examined conſtitutes between one eighth and one ninth part of the weight of

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the ore, is a concluſion upon which, from a variety of experiments, I am diſpoſed to rely.

There are annually ſmelted in Derbyſhire about ten thouſand tons of lead ore: now if means could be invented (which I think very poſſible) of ſaving the ſulphur contained in ten thouſand tons of ore, ſuppoſing that the ore ſhould only yield one tenth of its weight of ſulphur, though it unqueſtionably contains more, Derbyſhire alone would furniſh annually one thouſand tons of ſulphur, the value of which would annually be about fifteen thouſand pounds. I mention this circumſtance thus publicly, in hopes that the lead ſmelters may be induced to proſecute the object. If the ſulphur contained in lead ore could be collected, it would not only be a lucrative buſineſs to the ſmelters, but a great ſaving to the nation. We at preſent import the ſulphur we uſe, and the conſumption of this commodity is exceeding great, in the making of gunpowder, in forming the mixture for covering the bottom and ſides of ſhips, and in a great variety of arts. The ſmelters need not be apprehenſive leſt the quality of the ore ſhould be injured by extracting the ſulphur. Eighteen hundred weight of ore, from which the ſulphur has been extracted, will certainly yield as much lead as twenty hundred weight of ore, from which the ſulphur has not been extracted, and it will probably yield more.

Arſenic

Arſenic is extracted from a particular ore in Saxony, by roaſting the ore in a furnace, which has a long horizontal chimney; the chimney is large, has many windings and angles, that the arſenical vapour which ariſes from the ore may be the more eaſily condenſed: the arſenic attaches itſelf like ſoot to the ſides of the chimney, and is from time to time ſwept out. It is very probable, that by ſome ſuch contrivance the ſulphur contained in lead ore might be collected. The ſmelters call every thing ſulphur which is volatilized during the roaſting or fluxing of an ore; but none of thoſe with whom I have converſed had any notion that common ſulphur could be ſeparated from lead ore.

The greyiſh calx which remained upon the iron after the ſulphur was conſumed, was put upon a piece of lighted charcoal; the heat of the charcoal being quickened by blowing upon it, a great number of globules of lead were formed upon its ſurface. From hence it appears, that this calx is not an unmetallic earth contained in the ore, which the acid of nitre could not diſſolve; but a calx of lead, probably produced by the violent action of the acid, and which, by the addition of phlogiſton, may be exhibited in its metallic form. The quantity of this calx depends much upon the action of the acid upon the ore; if that action is violent, the calx is in greater abundance

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than if it be moderate; and I am not certain whether the experiment might not be ſo managed, that there would be little or no calx remaining; that is, a given quantity of ore might be ſo diſſolved in the acid of nitre, that nothing would remain undiſſolved except the ſulphur. But I have not yet perfectly ſatisfied myſelf as to the conſtituent parts of lead ore. I am certain that it contains lead and ſulphur, a liquid and air: of the exiſtence of the three firſt there can be no doubt, from what has been ſaid, and the air is rendered beautifully apparent by the following experiment.

Let ſome lead ore be reduced into a fine powder, put it into a narrow-bottomed ale glaſs, fill the glaſs three parts with water, drop into the water a portion of the ſtrong acid of nitre, you may judge of the requiſite quantity by ſeeing the ſolution commence, and you will obſerve the ore univerſally covered with bubbles of air, theſe will buoy the ore up in large tufts to the ſurface, and the air will continue to be ſeparated from the ore till the acid becomes ſaturated with the lead. The ſalt ariſing from the union of the nitrous acid to the lead often appears cryſtallized upon the ſurface of the menſtruum in this experiment; and if, when the menſtruum is in that ſtate, a little freſh acid be added, the ſalt inſtantly cryſtallizes and falls down to the bottom of the

glaſs,

glaſs, the acid having abſorbed the water which held it in ſolution. When lead is diſſolved in the manner here mentioned, by a very diluted acid of nitre, there is no appearance of ſulphur upon the ſurface of the menſtruum, there is found at its bottom a black matter which is the ſulphur.

But though lead and ſulphur, a liquid and air, are unqueſtionably conſtituent parts of lead ore, I do not take upon me to ſay, that they are the only conſtituent parts: it is well known, that, during the ſmelting of lead ore, a third part or more of its weight is ſomehow or other loſt, ſince from one and twenty hundred weight of ore they ſeldom obtain above fourteen hundred weight of lead. What is loſt partly conſiſts of a ſcoria which floats upon the ſurface of the lead during the operation of ſmelting, and partly of what is ſublimed up the chimney and diſſipated in the air. The ſcoria, I apprehend, would be very little even from a ton of ore, if the ore was quite free from ſpar: it is the ſpar which is mixed with the ore that conſtitutes the main portion of the ſcoria. I have in my poſſeſſion a ſolid maſs of ſcoria, which accidentally flowed out from a ſmelting furnace, and which in colour and conſiſtency perfectly reſembles grey lime-ſtone; it receives a poliſh as fine as marble, and it might, perhaps, with advantage be caſt into molds for paving ſtones,

chimney pieces, and other matters. It arifes from the fpar mixed with the ore, and, by the addition of fufible fpar to the ore during its fufion, its quantity might be increafed at no great expence, in any proportion. That part of the ore which is fublimed and difperfed in the air, confifts partly of the fulphur which is confumed, and partly of lead; this fublimed lead attaches itfelf in part to the fides of the chimney of the fmelting furnace; the reft of it flies up into the air, from whence it falls upon the ground, poifoning the water and herbage upon which it fettles. This fublimed lead might be collected either by making it meet with water, or with the vapour of water during its afcent, or by making it pafs through an horizontal chimney of a fufficient length.

It is not eafy to determine with precifion the quantity of this fublimed lead; a general guefs, however, may throw fome light upon the fubject. They ufually at a fmelting houfe work off three tons, or fixty hundred weight, of lead ore every twenty-four hours; the fulphur contained in fixty hundred weight of ore we will fuppofe to be feven hundred weight, and the lead to be forty hundred weight; the air, liquid, fcoria, and fublimed lead, muft together, upon this fuppofition, amount to thirteen hundred weight; now, admitting three hundred weight of the thirteen to be fublimed lead, it is evident

evident that, could it be collected, there would be an annual ſaving at each ſmelting houſe of above fifty tons, which, ſuppoſing it to be worth four pounds *per* ton, would amount to above two hundred pounds a year. The price and quantity of lead ſublimate here aſſumed are probably both of them below the truth; but my end is anſwered in giving this hint to perſons engaged in the ſmelting buſineſs.

The following experiments, though upon a different ſubject, may not be unacceptable to the lovers of chemiſtry, as I do not remember to have any where met with them. I trouble the Society with a relation of them at this time, that I may not hereafter intrude upon their leiſure.

It is commonly known, that the ſurface of melted lead becomes covered with a pellicle of various colours. I undertook ſome experiments in the courſe of laſt winter, with a view to aſcertain the order in which the colours ſucceeded each other. The lead which lines the boxes in which tea is imported from China happening to be at hand, ſome of it was melted in an iron ladle; but I was much ſurprized to find that its ſurface, though it was preſently covered with a duſky pellicle, did not exhibit any colours. Imagining that the heat was not ſufficiently ſtrong to render the colours viſible, the fire was urged till the ladle became red-hot, the calcined pellicle

upon

upon the ſurface of the lead was red-hot alſo, but it was ſtill without colour. The ſame parcel of lead was boiled in a crucible for a conſiderable time; during the boiling a copious ſteam was diſcharged, and the ſurface of the lead, as is uſual, became covered with a half vitrified ſcoria. The lead which remained unvitrified was then examined, and it had acquired the property of forming a ſucceſſion of coloured pellicles during the whole time of continuing in a ſtate of fuſion.

Another portion of the ſame kind of lead was expoſed to a ſtrong calcining heat for a long time; the part which remained uncalcined did, at length, acquire the property of exhibiting colours ſufficiently vivid.

Theſe experiments induced me to conclude, that the Chineſe lead was mixed with ſome ſubſtance from which it was neceſſary to free it, either by ſublimation or calcination, before it would exhibit its colours. It would be uſeleſs to mention all the experiments which I made before I diſcovered the heterogeneous ſubſtance with which I ſuppoſed the Chineſe lead was mixed. At laſt I hit upon one which ſeems fully ſufficient to explain the phenomenon. Into a ladle full of melted Derbyſhire lead, which manifeſted a ſucceſſion of the moſt vivid colours, I put a ſmall portion of tin, and obſerved, that as ſoon as the tin was melted, and mixed with the lead, no

more

more colours were to be ſeen. I do not know preciſely the ſmalleſt poſſible quantity of tin, which will be ſufficient to deprive a given quantity of lead of its property of forming coloured pellicles, but I have reaſon to believe, that it does not exceed one five thouſandth part of weight of the lead.

Derbyſhire lead, which has loſt its property of exhibiting colours by being mixed with tin, acquires it again, as is mentioned of the Chineſe lead, by being expoſed to a calcining heat for a ſufficient time; the tin it is ſuppoſed being ſeparated from the lead by calcination before all the lead is reduced to a calx.

Some calcined Chineſe lead was reduced to its metallic form by burning ſome tallow over it. The reduced lead gave, when melted, coloured pellicles; the calx of tin, which we ſuppoſe to have been mixed with the calcined lead, not being ſo eaſily reducible as that of lead.

I find that zinc is another metallic ſubſtance which has the ſame property as tin with reſpect to the depriving lead of its power of forming coloured pellicles; but it does not, I think, poſſeſs this power in ſo eminent a degree as tin. I put ſmall portions of biſmuth alſo into melted lead, but the lead ſtill retained its quality of forming colours. I melted together ſome ſilver and lead, but the lead did not thereby loſe its power of forming colours.

 A little

A little tin added to a mixture of lead and bifmuth, or to a mixture of filver and lead, immediately takes away from the refpective mixtures the faculty of forming coloured pellicles.

This quality of tin has hitherto, as far as I know, been unobferved; but every new fact, relative to the actions of bodies one upon another, ought to be recorded. The change produced in lead by the admixture of a fmall portion of tin is much felt by the plumbers, as it makes the metal fo hard and harfh, that it is not without difficulty they can caft it into fheet lead. If their old lead does not work fo willingly, nor exhibit colours fo readily, as new lead, they may refer the difference to the fmall quantity of tin contained in the folder, from which old lead can feldom be thoroughly freed.

With refpect to the order in which the colours fucceed one another upon the furface of melted lead, it feems to be the following one; yellow, purple, blue, yellow, purple, green, pink, green, pink, green. Upon exhibiting the bright furface of melted lead to the air, I have often obferved thefe ten changes to follow one another in a more or lefs rapid fucceffion, according to the degree of heat prevailing in the lead. If the heat is but fmall, the fucceffion ftops before it has gone through all the changes; but with the greateft heat I did not obferve any

further

further variation. All the colours are very vivid, and each ſeems to go through all the ſhades belonging to it before it is changed into the next in order.

The formation of theſe colours may be explained from what has been advanced by Sir ISAAC NEWTON, and illuſtrated by the very ingenious experiments of Mr. DELAVAL, relative to the ſize of the particles conſtituting coloured bodies.

XL. *Description of a most effectual Method of securing Buildings against Fire, invented by* Charles *Lord Viscount* Mahon, *F. R. S.*

Read July 2, 1778.

§ 1. THE new and very simple method which I have discovered of securing every kind of building (even though constructed of timber) against all danger of fire, may very properly be divided into three parts; namely, *under-flooring*, *extra-lathing*, and *inter-securing*, which particular methods may be applied, in part or in whole, to different buildings, according to the various circumstances attending their construction, and according to the degree of accumulated fire, to which each of these buildings may be exposed, from the different uses to which they are meant to be appropriated.

§ 2. The method of *under-flooring* may be divided into two parts; *videlicet*, into *single* and *double under-flooring*.

The method of *single under-flooring* is as follows. A common strong lath, of about one quarter of an inch thick

(either of oak or fir) ſhould be nailed againſt each ſide of every joiſt, and of every main timber, which ſupports the floor intended to be ſecured. Other ſimilar laths ought then to be nailed the whole length of the joiſts, with their ends butting againſt each other: theſe are what I call the *fillets*. The top of each fillet ought to be at one inch and a half below the top of the joiſts or timbers againſt which they are nailed. Theſe fillets will then form, as it were, a ſort of ſmall ledge on each ſide of all the joiſts.

§ 3. When the fillets are going to be nailed on, ſome of the rough plaſter hereafter mentioned (§ 9.) muſt be ſpread with a trowel all along that ſide of each of the fillets, which is to lay next to the joiſts, in order that theſe fillets may be well bedded therein when they are nailed on, ſo that there ſhould not be any interval between the fillets and the joiſts.

§ 4. A great number of any common laths (either of oak or fir) muſt be cut nearly to the length of the width of the intervals between the joiſts.

Some of the rough plaſter referred to above (§ 3.) ought to be ſpread, with a trowel, ſucceſſively upon the top of all the fillets, and along the ſides of that part of the joiſts which is between the top of the fillets and the upper edge of the joiſts.

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The ſhort pieces of common laths juſt mentioned ought (in order to fill up the intervals between the joiſts that ſupport the floor) to be laid in the contrary direction to the joiſts, and cloſe together in a row, ſo as to touch one another, as much as the want of ſtraitneſs in the laths will poſſibly allow, without the laths lapping over each other; their ends muſt reſt upon the fillets ſpoken of above (§ 2.) and they ought to be well bedded in the rough plaſter. It is not proper to uſe any nails to faſten down either theſe ſhort pieces of laths, or thoſe ſhort pieces hereafter mentioned (§ 7.)

§ 5. Theſe ſhort pieces of laths ought then to be covered with one thick coat of the rough plaſter ſpoken of hereafter (§ 9.), which ſhould be ſpread all over them, and which ſhould be brought, with a trowel, to be about level with the tops of the joiſts, but not above them. This rough plaſter in a day or two ſhould be trowelled all over, cloſe home to the ſides of the joiſts; but the tops of the joiſts ought not to be any wiſe covered with it.

§ 6. The method of *double under-flooring* is, in the firſt part of it, exactly the ſame as the method juſt deſcribed. The fillets and the ſhort pieces of laths are applied in the ſame manner; but the coat of rough plaſter

ought to be little more than half as thick as the coat of rough plaſter applied in the method of *ſingle under-flooring*.

§ 7. In the method of *double under-flooring*, as faſt as this coat of rough plaſter is laid on, ſome more of the ſhort pieces of laths, cut as above directed (§ 4.), muſt be laid in the intervals between the joiſts upon the firſt coat of rough plaſter; and each of theſe ſhort laths muſt be, one after the other, bedded deep and quite ſound into this rough plaſter whilſt it is ſoft. Theſe ſhort pieces of laths ſhould be laid alſo as cloſe as poſſible to each other, and in the ſame direction as the firſt layer of ſhort laths.

§ 8. A coat of the ſame kind of rough plaſter ſhould then be ſpread over this ſecond layer of ſhort laths, as there was upon the firſt layer above deſcribed. This coat of rough plaſter ſhould (as above directed § 5. for the method of *ſingle under-flooring*) be trowelled level with the tops of the joiſts, but it ought not to riſe above them. The ſooner this ſecond coat of rough plaſter is ſpread upon the ſecond layer of ſhort laths juſt mentioned (§ 7.) the better.

What follows, as far as § 13. is common to the method of *ſingle* as well as to that of *double under-flooring*.

§ 9. Common coarſe lime and hair (ſuch as generally ſerves for the pricking-up-coat in plaſtering) may be uſed for all the purpoſes before or hereafter mentioned; but

but it is confiderably cheaper, and even much better, in all thefe cafes, to make ufe of *hay* inftead of *hair*, in order to prevent the plafter-work from cracking. The hay ought to be chopped to about three inches in length, but no fhorter.

One meafure of common rough *fand*, two meafures of flacked *lime*, and three meafures (but not lefs) of chopped *hay*, will prove, in general, a very good proportion, when fufficiently beat up together in the manner of common mortar. The hay muft be well dragged in this kind of rough plafter, and well intermixed with it; but the hay ought never to be put in, till the two other ingredients are well beat up together with water.

This rough plafter ought never to be made thin for any of the work mentioned in this paper. The ftiffer it is the better, provided it be not too dry to be fpread properly upon the laths.

If the flooring boards are required to be laid very foon, a fourth or a fifth part of *quick* [(a)] *lime* in powder, verywell mixed with this rough plafter juft before it is ufed, will caufe it to dry very faft.

(a) I have practiced this method in an extenfive work with great advantage. In *three weeks* this rough plafter grows perfectly *dry*. The rough plafter, fo made, may be applied at *all times of the year* with the greateft fuccefs. The eafieft method, by much, of reducing the *quick lime* to powder is, by dropping a *fmall* quantity of water on the lime-ftone, a little while before the powder is intended to be ufed: the lime will ftill retain a very fufficient degree of heat.

§ 10.

§ 10. When the rough plaſter-work between the joiſts has got thoroughly dry, it ought to be obſerved, whether or not, there be any ſmall cracks in it, particularly next to the joiſts. If there are any, they ought to be waſhed over with a bruſh, wet with *mortar-waſh*, which will effectually cloſe them; but there will never be any cracks at all, if the *chopped hay* and the *quick lime* be properly made uſe of.

§ 11. The mortar-waſh I make uſe of is merely this. About two meaſures of quick lime, and one meaſure of common ſand, ſhould be put into a pail, and ſhould be well ſtirred up with water, till the water grows very thick, ſo as to be almoſt of the conſiſtency of a thin jelly. This waſh, when uſed, will grow dry in a few minutes.

§ 12. Before the flooring boards are laid, a ſmall quantity of very dry common ſand ſhould be ſtrewed over the rough plaſter-work, but not over the tops of the joiſts. The ſand ſhould be ſtruck ſmooth with an hollow rule, which ought to be about the length of the diſtance from joiſt to joiſt, and of about one eighth of an inch curvature; which rule, paſſing over the ſand, in the ſame direction with the joiſts, will cauſe the ſand to lay rather rounding in the middle of the interval between each pair of joiſts.

The flooring boards may then be laid and faſtened down in the uſual manner; but very particular attention

 muſt

muſt be paid to the rough plaſter-work and to the ſand being moſt perfectly dry before the boards are laid, for fear of the *dry-rot*; of which however there is no kind of danger, when this precaution is made uſe of.

§ 13. The method of *under-flooring* I have alſo applied, with the utmoſt ſucceſs, to a wooden ſtair-caſe. It is made to follow the ſhape of the ſteps, but no ſand is laid upon the rough plaſter-work in this caſe.

§ 14. The method of *extra-lathing* may be applied to cieling joiſts, to ſloping roofs, and to wooden partitions. It is ſimply this:

As the laths are going to be nailed on, ſome of the above mentioned rough plaſter ought to be ſpread between theſe laths and the joiſts (or other timbers) againſt which theſe laths are to be nailed. The laths ought to be nailed very cloſe to each other.

When either of the ends of any of the laths laps over other laths, it ought to be attended to, that theſe ends be bedded ſound in ſome of the ſame kind of rough plaſter.

This attention is equally neceſſary for the ſecond layer of laths hereafter mentioned (§ 15.).

§ 15. This firſt layer of laths ought to be covered with a pretty thick coat of the ſame rough plaſter ſpoken of above (§ 9.). A ſecond layer of laths ought then to be nailed on, each lath being, as it is put on, well ſqueezed

and bedded ſound into the ſoft rough plaſter. For this reaſon, no more of this firſt coat of rough plaſter ought to be laid on at a time than what can be immediately followed with the ſecond layer of laths.

The laths of this ſecond layer ought to be laid as cloſe to each other as they can be, to allow of a proper clench for the rough plaſter.

The laths of the ſecond (b) layer may then be plaſtered over with a coat of the ſame kind of rough plaſter, or it may be plaſtered over in the uſual manner.

§ 16. The third method, which is that of *inter-ſecuring*, is very ſimilar, in moſt reſpects, to that of *under-flooring*; but no ſand is afterwards to be laid upon it. *Inter-ſecuring* is applicable to the ſame parts of a building as the method of *extra-lathing* juſt deſcribed; but it is not often neceſſary to be made uſe of.

§ 17. I have made a prodigious number of experiments upon every part of theſe different methods. I cauſed a wooden building to be conſtructed at Chevening, in Kent, in order to perform them in the moſt natural manner. The methods of *extra-lathing* and *double under-flooring* were the only ones made uſe of in that building.

(b) If a third layer of laths be immediately nailed on, and be covered with a third coat of rough plaſter, I then call the method *treble-lathing*; but this method of *treble-lathing* can almoſt, in no caſe, be required.

On the 26th of September laſt year I had the honour to repeat ſome of my experiments before the Preſident and ſome of the Fellows of the Royal Society, the Lord Mayor and Aldermen of the City of London, the Committee of City Lands, ſeveral of the foreign miniſters, and a great number of other perſons.

§ 18. The firſt experiment was to fill the lower room of the building (which room was about twenty-ſix feet long by ſixteen wide) full of ſhavings and faggots, mixed with combuſtibles, and to ſet them all on fire. The heat was ſo intenſe, that the glaſs of the windows was melted like ſo much common ſealing wax, and run down in drops, yet the flooring boards of that very room were not burnt through, nor was one of the ſide timbers, floor-joiſts, or cieling-joiſts, damaged in the ſmalleſt degree; and the perſons who went into the room immediately over the room filled with fire, did not perceive any ill effects from it whatever, even the floor of that room being perfectly cool during that enormous conflagration immediately underneath.

§ 19. I then cauſed a kind of wooden building (of full fifty feet in length, and of three ſtories high in the middle) to be erected, quite cloſe to one end of the ſecured wooden houſe. I filled and covered this building with above eleven hundred large kiln faggots, and ſeveral loads of dry ſhavings; and I ſet this pile on fire.

The height of the flame was no leſs than eighty-ſeven feet perpendicular from the ground, and the graſs upon a bank, at a hundred and fifty feet from the fire, was all ſcorched; yet the ſecured wooden building, quite contiguous to this vaſt heap of fire, was not at all damaged, except ſome parts of the outer coat of plaſter-work.

This experiment was intended to repreſent a wooden town on fire, and to ſhew how effectually even a wooden building, if ſecured according to my new method, would ſtop the progreſs of the flames on that ſide, without any aſſiſtance from fire-engines, &c.

§ 20. The laſt experiment I made that day, was the attempting to burn a wooden ſtair-caſe, ſecured according to my ſimple method of *under-flooring*. The under ſide of the ſtair-caſe was *extra-lathed*. Several very large kiln faggots were laid, and kindled, under the ſtair-caſe, round the ſtairs and upon the ſteps; this wooden ſtair-caſe notwithſtanding reſiſted, as if it had been of fire-ſtone, all the attempts that were made to conſume it.

I have ſince made five other ſtill ſtronger fires upon this ſame ſtair-caſe, without having repaired it, having, moreover, filled the ſmall place in which this ſtair-caſe is entirely with ſhavings and large faggots; but the ſtair-caſe is, however, ſtill ſtanding, and is but little damaged.

§ 21. In most houses it is necessary *only to secure the floors*; and that according to the method of *single under flooring* described above, in § 2, 3, 4, and 5. The extra-expence of it (all materials included) is only about nine pence *per* square yard, unless there should be particular difficulties attending the execution, in which case it will vary a little. When *quick lime* is made use of, the expence is a trifle more.

The extra-expence of the method of *extra-lathing* is no more than six pence *per* square yard for the timber side-walls and partitions; but for the cieling about nine pence *per* square yard. No *extra-lathing* is necessary in the generality of houses.

§ 22. I purpose giving to the world, before it is very long, a detailed account of many other experiments I have made upon this subject, and of the various advantages arising from my method, with several particulars relative to the different parts of each of the methods above described, and relative to their joint or separate application to different kinds of buildings, and to the different constituent parts of an house; to which I shall add a full explanation of the principles upon which they are founded, and the reasons for their certain and surprizing success. In the mean time I have taken the liberty of troubling the Society with this short account.

XLI. *A Method of finding, by the Help of Sir* Isaac Newton's *binomial Theorem, a near Value of the very slowly-converging infinite Series* $x + \frac{xx}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} +$ &c. *when* x *is very nearly equal to* 1. *By* Francis Maseres, *Esq. F. R. S. Cursitor Baron of the Exchequer.*

Read July 9, 1778.

ARTICLE I.

IF the capital letters A, B, C, D, E, &c. be put for the numeral co-efficients of the powers of x in the said series, so that A shall be $= 1$, $B = \frac{1}{2}$, $C = \frac{1}{3}$, $D = \frac{1}{4}$, $E = \frac{1}{5}$, and so on, we shall have $B = \frac{1}{2} \times A$, $C = \frac{2}{3} \times B$, $D = \frac{3}{4} \times C$, $E = \frac{4}{5} \times D$, &c. and the series $x + \frac{xx}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} +$ &c will be $= x + \frac{1}{2}Axx + \frac{2}{3}Bx^3 + \frac{3}{4}Cx^4 + \frac{4}{5}Dx^5 + \frac{5}{6}Ex^6 + \frac{6}{7}Fx^7 + \frac{7}{8}Gx^8$ + &c.; in which series the fractions $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$, $\frac{5}{6}$, $\frac{6}{7}$, $\frac{7}{8}$, &c. which generate the co-efficients of the powers of x in the several terms after the first term x, are derived from each other by the continual addition of 1 to both their numerators and their denominators.

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2. This obſervation ſuggeſts to us a method of finding a near value of the ſum of this ſeries by the help of Sir ISAAC NEWTON's binomial theorem, which may be explained as follows.

If m and n repreſent any two whole numbers, the reciprocal of the $\frac{m}{n}$th power of the binomial quantity $1-x$, or, according to Sir ISAAC NEWTON's notation of powers, the quantity $\overline{1-x}|^{\frac{-m}{n}}$, will , according to that celebrated theorem, be equal to the infinite ſeries

$$1 + \frac{m}{n} \times x$$
$$+ \frac{m}{n} \times \frac{m+n}{2n} \times xx + \frac{m}{n} \times \frac{m+n}{2n} \times \frac{m+2n}{3n} \times x^3$$
$$+ \frac{m}{n} \times \frac{m+n}{2n} \times \frac{m+2n}{3n} \times \frac{m+3n}{4n} \times x^4$$
$$+ \frac{m}{n} + \frac{m+n}{2n} \times \frac{m+2n}{3n} \times \frac{m+3n}{4n} \times \frac{m+4n}{5n} \times x^5 + \&c.$$

or $1 + \frac{m}{n} \text{A}x + \frac{m+n}{2n} \text{B}xx + \frac{m+2n}{3n} \text{C}x^3 + \frac{m+3n}{4n} \text{D}x^4 + \frac{m+4n}{5n} \text{E}x^5 + \frac{m+5n}{6n} \text{F}x^6 + \frac{m+6n}{7n} \text{G}x^7 + \frac{m+7n}{8n} \text{H}x^8 + \&c.$; in which ſeries the capital letters A, B, C, D, E, F, G, H, &c. ſtand for 1 and the co-efficients of x, xx, x^3, x^4, x^5, x^6, x^7, x^8, &c. Now it is evident, that the generating fractions $\frac{m+n}{2n}$, $\frac{m+2n}{3n}$, $\frac{m+3n}{4n}$, $\frac{m+4n}{5n}$, $\frac{m+5n}{6n}$, $\frac{m+6n}{7n}$, $\frac{m+7n}{8n}$, &c. are derived from $\frac{m}{n}$ and from each other by the continual addition of n to both their numerators and denominators. Therefore, though they are greater than they would be if m was ſubtracted from the numerator of

of each of them, that is, than the fractions $\frac{n}{2n}$, $\frac{2n}{3n}$, $\frac{3n}{4n}$, $\frac{4n}{5n}$, $\frac{5n}{6n}$, $\frac{6n}{7n}$, $\frac{7n}{8n}$, &c. and consequently, than the fractions $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$, $\frac{5}{6}$, $\frac{6}{7}$, $\frac{7}{8}$, &c. which are respectively equal to $\frac{n}{2n}$, $\frac{2n}{3n}$, $\frac{3n}{4n}$, $\frac{4n}{5n}$, $\frac{5n}{6n}$, $\frac{6n}{7n}$, $\frac{7n}{8n}$, &c. Yet, the further we go in the series, the less is the proportion in which they exceed the latter fractions; insomuch that, if we go far enough in the series, we may find terms in it whose proportion to the corresponding terms in the series $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$, $\frac{5}{6}$, $\frac{6}{7}$, $\frac{7}{8}$, &c. shall approach as near as we please to a proportion of equality. And, by taking n of a very great magnitude in comparison of m, we may even make the first terms of the series $\frac{m+n}{2n}$, $\frac{m+2n}{3n}$, $\frac{m+3n}{4n}$, $\frac{m+4n}{5n}$, $\frac{m+5n}{6n}$, $\frac{m+6n}{7n}$, $\frac{m+7n}{8n}$, &c. approach very nearly to an equality with the corresponding terms of the series $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$, $\frac{5}{6}$, $\frac{6}{7}$, $\frac{7}{8}$, &c. which are the generating fractions of the proposed series $x + \frac{xx}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} + \frac{x^7}{7} + \frac{x^8}{8} +$ &c. In order to this, let m be taken $= 1$, and $n = 1,000,000,000,000$, that is, $=$ a billion, or the square of a million, which, to avoid the frequent repetition of so many cyphers, we will call b.

Then will $\overline{1-x}|^{-\frac{1}{1,000,000,000,000}}$, or $\overline{1-x}|^{-\frac{1}{b}}$, or $\frac{1}{\overline{1-x}|^{\frac{1}{b}}}$, be

$$= 1 + \frac{1}{b}\,\mathrm{A}x + \frac{1+b}{2b}\,\mathrm{B}xx + \frac{1+2b}{3b}\,\mathrm{C}x^3 + \frac{1+3b}{4b}\,\mathrm{D}x^4 + \frac{1+4b}{5b}\,\mathrm{E}x^5$$

$+ \frac{1+5b}{6b}\,\mathrm{F}x^6 + \frac{1+6b}{7b}\,\mathrm{G}x^7 + \frac{1+7b}{8b}\,\mathrm{H}x^8 +$ &c. which, on account of

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the great magnitude of b, $2b$, $3b$, $4b$, $5b$, $6b$, $7b$, &c. in comparison of 1, will be almost equal to (though somewhat greater than) $1+\frac{1}{b}Ax+\frac{b}{2b}Bxx+\frac{2b}{3b}Cx^3+\frac{3b}{4b}Dx^4+\frac{4b}{5b}Ex^5+\frac{5b}{6b}Fx^6+\frac{6b}{7b}Gx^7+\frac{7b}{8b}Hx^8+$ &c. or $1+\frac{1}{b}Ax+\frac{1}{2}Bxx+\frac{2}{3}Cx^3+\frac{3}{4}Dx^4+\frac{4}{5}Ex^5+\frac{5}{6}Fx^6+\frac{6}{7}Gx^7+\frac{7}{8}Hx^8+$ &c. or $1+\frac{1}{b}\times 1\times x+\frac{1}{2}\times\frac{1}{b}\times 1\times xx+\frac{2}{3}\times\frac{1}{2}\times\frac{1}{b}\times 1\times x^3+\frac{3}{4}\times\frac{2}{3}\times\frac{1}{2}\times\frac{1}{b}\times 1\times x^4+$ &c. or $1+\frac{1}{b}x+\frac{1}{b}\times\frac{1}{2}xx+\frac{1}{b}\times\frac{1}{3}\times x^3+\frac{1}{b}\times\frac{1}{4}x^4+$ &c. or $1+\frac{x}{b}+\frac{xx}{2b}+\frac{x^3}{3b}+\frac{x^4}{4b}+\frac{x^5}{5b}+\frac{x^6}{6b}+\frac{x^7}{7b}+\frac{x^8}{8b}+$&c. Therefore, multiplying both sides by b, we shall have $b\times\frac{1}{\overline{1-x}|^{\frac{1}{b}}}$ nearly $=b+x+\frac{xx}{2}+\frac{x^3}{3}+\frac{x^4}{4}+\frac{x^5}{5}+\frac{x^6}{6}+\frac{x^7}{7}+\frac{x^8}{8}+$&c.; and, subtracting b from both sides, $x+\frac{xx}{2}+\frac{x^3}{3}+\frac{x^4}{4}+\frac{x^5}{5}+\frac{x^6}{6}+\frac{x^7}{7}+\frac{x^8}{8}+$ &c. nearly $=b\times\frac{1}{\overline{1-x}|^{\frac{1}{b}}}-b=b\times\overline{\frac{1}{1-x}}|^{\frac{1}{b}}-b$; that is, the proposed series $x+\frac{xx}{2}+\frac{x^3}{3}+\frac{x^4}{4}+\frac{x^5}{5}+$ &c. will be nearly equal to $b\times\overline{\frac{1}{1-x}}|^{\frac{1}{b}}-b$. We must therefore first subtract x from 1, and then divide 1 by the remainder, which will give us a quotient equal to $\frac{1}{1-x}$. And, having found this quotient, we must extract its bth, or 1,000,000,000,000th, root, and multiply the said root by b, or 1,000,000,000,000; and, lastly, from the product we must subtract b, or 1,000,000,000,000: and the

the remainder thereby obtained will be nearly equal to the propofed infinite feries $x + \frac{xx}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} + \frac{x^7}{7} +$ &c. Q. E. I.

An example of the foregoing method of fumming the faid infinite feries.

3. As an example of this method of finding the value of the feries $x + \frac{xx}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} +$ &c. let us fuppofe x to be equal to $\frac{9}{10}$.

Then we fhall have $1 - x = 1 - \frac{9}{10} = \frac{1}{10}$, and $\frac{1}{1-x} = 10$. Now, fince the logarithm of 10 in BRIGGS's Syftem of logarithms is 1, the logarithm of the 1,000,000,000,000th root of 10 muft be the 1,000,000,000,000th part of 1, or muft be = .000,000,000,001. This logarithm is too fmall to be found in the common tables of logarithms, which go only to feven places of figures; and therefore the number correfponding to it, that is, the 1,000,000,000,000th root of 10, cannot be found by the help of thofe tables; but it may be found in the manner following. The 1,000,000,000,000th root of 10 is a number that is fomewhat, and but a very little, greater than 1. That number, therefore, and 1 will reprefent two ordinates to the axis, or afymptote, of a logarithmick curve that are very nearly contiguous to each other: whence it follows, that the fub-tangent of the curve will

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bear very nearly the ſame proportion to the leſſer ordinate 1, as the abſciſs of the axis intercepted between the two ordinates, that is, as the logarithm of the ratio of the greater ordinate to the leſſer, or the logarithm .000,000,000,001, bears to the difference of the ſaid ordinates. Say therefore, as .434,294,481,9 (which is the ſub-tangent of the logarithmick curve in BRIGGS's Syſtem of Logarithms) is to 1 (or the leſſer of the two ordinates) ſo is .000,000,000,001 to a fourth number, which will be .000,000,000,002,302,585,093; and this fourth number will be the exceſs of the greater of the ſaid two ordinates above the leſſer, or of the billionth root of 10 above 1. Therefore the billionth root of 10 will be = 1.000,000,000,002,302,585,093; which, being multiplied by 1,000,000,000,000, will be = 1,000,000,000,002.302,585,093; from which if we ſubtract 1,000,000,000,000, the remainder will be 2.302,585,093. Therefore 2.302,585,093 is nearly equal to the infinite ſeries $x + \frac{xx}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} + \frac{x^7}{7}$ + &c. when x is $= \frac{9}{10}$. Q. E. I.

4. This number 2.302,585,093 gives the value of the ſeries $x + \frac{xx}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5}$ + &c. exact to nine places of figures, the error being only in the 10th figure 3, which ought to be a 2 inſtead of a 3, the more accurate value of that ſeries (which is equal to the logarithm of

the ratio of 1 to $1-x$ in NAPIER's Syſtem of Logarithms, that is, in the preſent example, to the logarithm of the ratio of 1 to $\frac{1}{10}$, or of 10 to 1, or to NAPIER's logarithm of 10) being 2.302,585,092,994,04.

5. I believe that ſimilar applications of the binomial theorem may be made for the ſummation of other ſlowly-converging infinite ſerieſes, whenever the generating fractions (by the multiplication of which the numeral co-efficients of the terms of ſuch ſerieſes are produced from each other) are formed by the addition of a given number to both their numerators and denominators. In our endeavours, therefore, to ſum ſuch ſerieſes it will be proper to attend to the law of the ſaid generating fractions.

XLII. *A Method of extending* Cardan's *Rule for resolving one Case of a Cubick Equation of this Form, $x^3 * - qx = r$, to the other Case of the same Equation, which it is not naturally fitted to solve, and which is therefore often called* the irreducible Case. *By* Francis Maseres, *Esq. F. R. S. Cursitor Baron of the Exchequer.*

Read July 9, 1778.

ARTICLE I.

IT is well known to all persons conversant with algebra, that CARDAN's rule for resolving the cubick equation $x^3 - qx = r$ is only fitted to resolve it when $\frac{rr}{4}$ is equal to, or greater than, $\frac{q^3}{27}$, or when r is equal to, or greater than, $\frac{2q\sqrt{q}}{3\sqrt{3}}$, and that it is of no use in the resolution of the other case of this equation, in which r is of any magnitude less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$. For in this case $\frac{rr}{4} - \frac{q^3}{27}$ becomes (according to the usual language of algebraists) a negative quantity, and consequently its square-root becomes

comes impossible, and the expression given by CARDAN's rule for the value of x (which is either $\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\frac{q}{3\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}}$ or $\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}$), involves in it the impossible quantity $\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$, and therefore is unintelligible and useless: or, according to what appears to me a more correct way of speaking (who never could form any idea of a negative quantity, and never understand by the sign $-$ any thing more than the subtraction of a lesser quantity from a greater), the quantity $\frac{rr}{4}-\frac{q^3}{27}$ becomes itself impossible, or the supposition that $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, (which is one of the foundations of CARDAN's rule), is no longer true, and consequently the rule itself, which is built upon it, can no longer take place.

2. Nevertheless it is possible, by the help of Sir ISAAC NEWTON's binomial theorem, to extend this rule to this latter case, in which $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, and which it is not of itself fitted to resolve; or, to speak with more accuracy, it is possible to derive from the expression of the value of x given by CARDAN's rule for the resolution of the equation $x^3-qx=r$ in the first case, in which $\frac{rr}{4}$ is

greater

greater than $\frac{q^3}{27}$, another expreſſion ſomewhat different from the former, that ſhall exhibit the true value of x in the ſecond caſe, in which $\frac{rr}{4}$ is leſs than $\frac{q^3}{27}$, provided it be not leſs than $\frac{q^3}{2 \times 27}$, or $\frac{q^3}{54}$: and this without any mention of either impoſſible, or negative, quantities. To ſhew how this may be effected, is the deſign of the following pages.

3. That the whole of this matter may be ſeen at one view, it will be convenient to ſet forth the foundation and inveſtigation of CARDAN's rule for reſolving the equation $x^3 - qx = r$, when $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$; which may be done as follows.

The Inveſtigation of Cardan's *Rule for reſolving the Cubick Equation* $x^3 * - qx = r$, *when* $\frac{rr}{4}$ *is greater than* $\frac{q^3}{27}$.

4. Previouſly to the inveſtigation of this rule, it will be proper to make the following obſervations.

OBS. 1. In the equation $x^3 - qx = r$ (which is a propoſition affirming that x^3 is greater than qx, and that the exceſs is equal to r) xx muſt always be greater than q, and x than $\sqrt{q}$.

OBS.

OBS. 2. While x increases from $\sqrt{q}$ *ad infinitum*, x^3 will increase continually from $q\sqrt{q}$ *ad infinitum*, and qx will increase continually from the same quantity $q\sqrt{q}$ *ad infinitum*.

OBS. 3. Also, while x increases from $\sqrt{q}$ *ad infinitum*, the excess of x^3 above qx will increase continually from nothing *ad infinitum*, without ever decreasing. For, if we put $\dot{x}$ to denote the increment which x receives in any given time, either small or great, $q\dot{x}$ will be the increment which qx will receive in the same time, and $3x^2\dot{x} + 3x\dot{x}^2 + \dot{x}^2$ will be the increment of x^3 in the same time. Now, since xx is always greater than q during the whole increase of x from being equal to $\sqrt{q}$ *ad infinitum*, $xx \times \dot{x}$ will be greater than $q\dot{x}$ during that whole increase. Therefore, *à fortiori*, $3x^2\dot{x} + 3x\dot{x}^2 + \dot{x}^3$ (which is more than triple of $xx \times \dot{x}$) will be greater than $q\dot{x}$; that is, the increment of x^3 will be greater than the contemporary increment of qx during all the increase of x. Consequently the excess of x^3 above qx, or the compound quantity $x^3 - qx$, will continually increase, without ever decreasing, while x increases from $\sqrt{q}$ to any greater magnitude.

OBS. 4. Since the compound quantity $x^3 - qx$ increases continually at the same time as x increases; and, when

 x is

x is equal to $\frac{2\sqrt{q}}{\sqrt{3}}$, $x^3 - qx$ is $\left(= \frac{8q\sqrt{q}}{3\sqrt{3}} - \frac{2q\sqrt{q}}{\sqrt{3}} = \frac{8q\sqrt{q}}{3\sqrt{3}} - \frac{6q\sqrt{q}}{3\sqrt{3}}\right) = \frac{2q\sqrt{q}}{3\sqrt{3}}$, it follows that, if x is greater than $\frac{2\sqrt{q}}{\sqrt{3}}$, the compound quantity $x^3 - qx$ will be greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, and, if x is lefs than $\frac{2\sqrt{q}}{\sqrt{3}}$, the faid compound quantity will be lefs than $\frac{2q\sqrt{q}}{3\sqrt{3}}$; and, *è converfo*, if the compound quantity $x^3 - qx$, or, its equal, the abfolute term r, is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, the value of x will be greater than $\frac{2\sqrt{q}}{\sqrt{3}}$; and, if $x^3 - qx$, or r, is lefs than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, the value of x will be lefs than $\frac{2\sqrt{q}}{\sqrt{3}}$; or, if $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, x will be greater than $\frac{2\sqrt{q}}{\sqrt{3}}$, and, if $\frac{rr}{4}$ is lefs than $\frac{q^3}{27}$, x will be lefs than $\frac{2\sqrt{q}}{\sqrt{3}}$.

Obs. 5. When r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, and confequently (by the laft obfervation) x is greater than $\frac{2\sqrt{q}}{\sqrt{3}}$, xx will be greater than $\frac{4q}{3}$, and $\frac{xx}{4}$ will be greater than $\frac{q}{3}$. But $\frac{xx}{4}$ is the fquare of $\frac{x}{2}$. Therefore when r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{{}^{3}}{27}$, the fquare of half x will be greater than $\frac{q}{3}$. But (by Euclid's Elements, Book II. Prop. V.) it is always poffible to divide a line, as x, into two unequal parts in fuch a pro-

a proportion that the rectangle under its parts ſhall be equal to any quantity that is leſs than the ſquare of its half. Therefore, when r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, it is poſſible to divide the line, or root, x into two unequal parts of ſuch magnitudes that their rectangle, or product, ſhall be equal to $\frac{q}{3}$. This obſervation is the foundation of CARDAN's rule for the reſolution of the equation $x^3-qx=r$ in the firſt caſe of that equation, or when r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$; the inveſtigation of which is as follows.

PROBLEM.

5. *To reſolve the Equation $x^3 * -qx=r$, when r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$.*

SOLUTION.

Since r is ſuppoſed to be greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, and conſequently (by Obſ. 5.) $\frac{xx}{4}$ is greater than $\frac{q}{3}$, it is poſſible for x to be divided into two unequal parts of ſuch magnitudes that their rectangle, or product, ſhall be equal to $\frac{q}{3}$. Let it be conceived to be ſo divided; and let the

greater of the two parts be called a, and the leſſer b. Then will ab be $=\frac{q}{3}$, and conſequently $3ab$ will be $=q$, and $3ab\times\overline{a+b}$ will be $=q\times\overline{a+b}$.

Now, ſince $a+b$ is equal to x, we ſhall have $a^3+3aab+3abb+b^3=x^3$, and $q\times\overline{a+b}=qx$. Therefore x^3-qx will be $=a^3+3aab+3abb+b^3-q\times\overline{a+b}=a^3+3ab\times\overline{a+b}+b^3-q\times\overline{a+b}$; that is (becauſe $3ab\times\overline{a+b}$ is $=q\times\overline{a+b}$) x^3-qx will be $=a^3+b^3$. Therefore r (which is $=x^3-qx$) will be $=a^3+b^3$.

But, ſince $3ab$ is $=q$, we ſhall have $b=\frac{q}{3a}$, and $b^3=\frac{q^3}{27a^3}$. Therefore a^3+b^3 is $=a^3+\frac{q^3}{27a^3}$, and r (which is $=a^3+b^3$) is $=a^3+\frac{q^3}{27a^3}$. Therefore ra^3 is $=a^6+\frac{q^3}{27}$, and ra^3-a^6 is $=\frac{q^3}{27}$.

But ra^3-a^6 is the product of the multiplication of $r-a^3$ into a^3, which are together equal to r. Therefore (by El. II. 5.) ra^3-a^6 muſt be leſs than the ſquare of half r, that is, than $\frac{rr}{4}$, and conſequently may be ſubtracted from it. Let it, and its equal $\frac{q^3}{27}$, be ſo ſubtracted. And we ſhall have $\frac{rr}{4}-ra^3+a^6=\frac{rr}{4}-\frac{q^3}{27}$. Therefore the ſquare-root of $\frac{rr}{4}-ra^3+a^6$ will be equal to $\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$. But the ſquare-root of $\frac{rr}{4}-ra^3+a^6$ is the difference of $\frac{r}{2}$ and a^3, that

that is, either $\frac{r}{2}-a^3$ or $a^3-\frac{r}{2}$, according as $\frac{r}{2}$ or a^3 is the greater quantity. But it has appeared above that a^3 and b^3 together are equal to r; and a is supposed to be greater than b, and consequently a^3 is greater than b^3. Therefore a^3 must be greater, and b^3 less, than $\frac{r}{2}$. Therefore $a^3-\frac{r}{2}$ is the difference of a^3 and $\frac{r}{2}$, and consequently is the square-root of the quantity $\frac{rr}{4}-ra^3+a^6$. Therefore $a^3-\frac{r}{2}$ is $=\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$, and a^3 is $=\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$. Consequently a is $=\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}$. But b has been shewn to be $=\frac{q}{3a}$. Therefore b is $=\dfrac{q}{3\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}}$; and consequently $a+b$, or x, is $=\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\dfrac{q}{3\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}}$.

Q. E. I.

6. This expression may be rendered more simple by substituting the single letter s in it instead of $\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$. For then it will be $\sqrt[3]{\frac{r}{2}+s}+\dfrac{q}{3\sqrt[3]{\frac{r}{2}+s}}$.

Synthetick Demonstration of the Truth of the foregoing Solution.

7. That this expression is equal to x in the equation

 x^3-

$x^3-qx=r$ will appear by ſubſtituting it inſtead of x in the compound quantity x^3-qx, which will thereby be ſeen to be equal to r, as it ought to be.

This may be done in the manner following.

Since x is $=\sqrt[3]{\frac{r}{2}+s}+\frac{q}{3\sqrt[3]{\frac{r}{2}+s}}$, or $\overline{\frac{r}{2}+s}|^{\frac{1}{3}}+\frac{q}{3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}}$, we ſhall have $x^3=\frac{r}{2}+s+3\times\overline{\frac{r}{2}+s}|^{\frac{2}{3}}\times\frac{q}{3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}}+3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}$ $\times\frac{qq}{9\times\overline{\frac{r}{2}+s}|^{\frac{2}{3}}}+\frac{q^3}{27\times\overline{\frac{r}{2}+s}}=\frac{r}{2}+s+q\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}+\frac{qq}{3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}}+$ $\frac{q^3}{\frac{27r}{2}+27s}$; and $qx=q\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}+\frac{qq}{3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}}$; and conſequently $x^3-qx=\frac{r}{2}+s+\frac{q^3}{\frac{27r}{2}+27s}=\frac{r}{2}+s+\frac{q^3}{\frac{27r}{2}+\frac{54s}{2}}=\frac{r}{2}+s+$ $\frac{q^3}{\frac{27r+54s}{2}}=\frac{r}{2}+s+\frac{2q^3}{27r+54s}$.

Now ss, or $\frac{rr}{4}-\frac{q^3}{27}$, is $\frac{27rr-4q^3}{108}=\frac{27rr-4q^3}{36\times 3}$; or, if we put $mm=27rr-4q^3$, we ſhall have ss, or $\frac{rr}{4}-\frac{q^3}{27}$, $=\frac{mm}{36\times 3}$, and $s=\frac{m}{6\sqrt{3}}$. Therefore $\frac{2q^3}{27r+54s}$ is $=\frac{2q^3}{27r+\frac{54\times m}{6\sqrt{3}}}=\frac{2q^3}{\frac{6\times 27\times\sqrt{3}\times r+54m}{6\sqrt{3}}}=$ $\frac{12\sqrt{3}\times q^3}{6\times 27\sqrt{3}\times r+54m}=\frac{2\sqrt{3}\times q^3}{27\sqrt{3}\times r+9m}$. Therefore $s+\frac{2q^3}{27r+54s}$ is $=\frac{m}{6\sqrt{3}}+$ $\frac{2\sqrt{3}\times q^3}{27\sqrt{3}\times r+9m}=\frac{27\sqrt{3}\times rm+9mm+36q^3}{6\times 27\times 3r+6\times 9\times\sqrt{3}\times m}=\frac{3\sqrt{3}\times rm+mm+4q^3}{54r+6\sqrt{3}\times m}=$ $\frac{3\sqrt{3}\times rm+27rr-4q^3+4q^3}{54r+6\sqrt{3}\times m}=\frac{3\sqrt{3}\times rm+27rr}{54r+6\sqrt{3}\times m}=\frac{\sqrt{3}\times rm+9rr}{18r+2\sqrt{3}\times m}$; and $\frac{r}{2}+$

$s+$

$s+\frac{2q^3}{27r+54s}$ is $=\frac{r}{2}+\frac{\sqrt{3}\times rm+9rr}{18r+2\sqrt{3}\times m}=\frac{18rr+2\sqrt{3}\times rm+2\sqrt{3}\times rm+18rr}{36r+4\sqrt{3}\times m}=\frac{36rr+4\sqrt{3}\times rm}{36r+4\sqrt{3}\times m}=r$. But we have before shewn that x^3-qx is $=\frac{r}{2}+s+\frac{2q^3}{27r+54s}$. Therefore x^3-qx is $=r$, and consequently $\sqrt[3]{\frac{r}{2}+s}+\frac{q}{3\sqrt[3]{\frac{r}{2}+s}}$ is the true value of x in the cubick equation $x^3-qx=r$. Q. E. D.

Two other Expressions for the Root of the foregoing Equation.

8. Two other expressions may be found for the root of this equation by resuming the investigation contained in Art. 5. The first of these expressions is $\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\frac{q}{3\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}}$, or (if we put ss, as before, $=\frac{rr}{4}-\frac{q^3}{27}$,) $\sqrt[3]{\frac{r}{2}-s}+\frac{q}{3\sqrt[3]{\frac{r}{2}-s}}$. The other expression is $\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}$, or $\sqrt[3]{\frac{r}{2}+s}+\sqrt[3]{\frac{r}{2}-s}$. These expressions are to be found in the following manner.

Investigation of the said Expressions.

9. In Art. 5. we supposed the line x to be divided in-

to

to two unequal parts a and b, of which a was ſuppoſed to be the greater; and we firſt found the value of the greater part a, and then determined that of the leſſer part b from its relation to a, which is expreſſed by the equation $3ab=q$. But we may with the ſame eaſe firſt determine the value of the leſſer part b, and then derive from it that of the greater part a; which would produce the firſt of the two expreſſions of the value of x mentioned in the laſt article. This may be done as follows.

Since it has been ſhewn in Art. 5. that r is $=a^3+b^3$, and $3ab$ is $=q$, and conſequently a is $=\frac{q}{3b}$, and a^3 to $\frac{q^3}{27b^3}$, it follows that r will be $=\frac{q^3}{27b^3}+b^3$. Therefore rb^3 is $=\frac{q^3}{27}+b^6$, and (ſubtracting b^6 from both ſides) rb^3-b^6 is $=\frac{q^3}{27}$. Therefore (ſubtracting both ſides from $\frac{rr}{4}$, than which they are evidently leſs), we ſhall have $\frac{rr}{4}-rb^3+b^6=\frac{rr}{4}-\frac{q^3}{27}$. Therefore the ſquare-root of $\frac{rr}{4}-rb^3+b^6$ will be $=\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$. But the ſquare-root of $\frac{rr}{4}-rb^3+b^6$ is the difference of the quantities $\frac{r}{2}$ and b^3, that is (becauſe b^3 is the leſſer part of a^3+b^3, or r, and conſequently is leſs than the half of it, or $\frac{r}{2}$), it is $=\frac{r}{2}-b^3$. Therefore $\frac{r}{2}-b^3$ is $=\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$. Therefore (adding b^3 to both ſides) $\frac{r}{2}$ will be $=b^3+$

✓

$\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$, and (subtracting $\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$ from both sides) b^3 will be $=\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$. Therefore b is $=\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}$, and a $(=\frac{q}{3b})$ is $=\frac{q}{3\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}}$, and consequently $b+a$, or $a+b$, or x, is $=\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\frac{q}{3\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}}$, or (if we put $ss=\frac{rr}{4}-\frac{q^3}{27}$) $\sqrt[3]{\frac{r}{2}-s}+\frac{q}{3\sqrt[3]{\frac{r}{2}-s}}$. Q. E. I.

Synthetick demonstration of the truth of the foregoing expression.

10. Here again we may demonstrate synthetically, that this expression is equal to the true value of x in the proposed equation $x^3-qx=r$, by substituting it for x in the left-hand side of that equation. For, if we make that substitution, we shall find that the value of x^3-qx thence arising will be equal to r. This may be done in the manner following.

If x is $=\sqrt[3]{\frac{r}{2}-s}+\frac{q}{3\sqrt[3]{\frac{r}{2}-s}}$, or $\overline{\frac{r}{2}-s}\big|^{\frac{1}{3}}+\frac{q}{3\times\overline{\frac{r}{2}-s}\big|^{\frac{1}{3}}}$, we shall have $x^3=\frac{r}{2}-s+3\times\overline{\frac{r}{2}-s}\big|^{\frac{2}{3}}\times\frac{q}{3\times\overline{\frac{r}{2}-s}\big|^{\frac{1}{3}}}+3\times\overline{\frac{r}{2}-s}\big|^{\frac{1}{3}}\times$

$\frac{qq}{9\times\overline{\frac{r}{2}-s}|^{\frac{2}{3}}}+\frac{q^3}{27\times\overline{\frac{r}{2}-s}}=\frac{r}{2}-s+q\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}+\frac{qq}{3\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}}+$ $\frac{q^3}{27\times\overline{\frac{r}{2}-s}}$, and $qx=q\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}+\frac{qq}{3\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}}$, and conſequently $x^3-qx=\frac{r}{2}-s+\frac{q^3}{27\times\overline{\frac{r}{2}-s}}=\frac{r}{2}-s+\frac{q^3}{\frac{27r}{2}-27s}=\frac{r}{2}-s$ $+\frac{q^3}{\frac{27r}{2}-\frac{54s}{2}}=\frac{r}{2}-s+\frac{2q^3}{27r-54s}$. Now ss, or $\frac{rr}{4}-\frac{q^3}{27}$ is $=\frac{27rr-4q^3}{108}=$ $\frac{27rr-4q^3}{36\times 3}$. Therefore if we put $mm=27rr-4q^3$, we ſhall have $ss=\frac{mm}{36\times 3}$, and $s=\frac{m}{6\sqrt{3}}$. Therefore $\frac{2q^3}{27r-54s}$ is $=$ $\frac{2q^3}{27r-\frac{54m}{6\sqrt{3}}}=\frac{2q^3}{\frac{6\times 27\times\sqrt{3}\times r-54m}{6\sqrt{3}}}=\frac{12\sqrt{3}\times q^3}{6\times 27\times\sqrt{3}\times r-54m}=\frac{2\sqrt{3}\times q^3}{27\times\sqrt{3}\times r-9m}$.

Therefore $\frac{r}{2}-s+\frac{2q^3}{27r-54s}$ is $=\frac{r}{2}-\frac{m}{6\sqrt{3}}+\frac{2\sqrt{3}\times q^3}{27\times\sqrt{3}\times r-9m}=\frac{r}{2}$ $\frac{-27\times\sqrt{3}\times rm+9mm+36q^3}{6\times 27\times 3r-54\sqrt{3}\times m}=\frac{r}{2}\frac{-3\sqrt{3}\times rm+mm+4q^3}{54r-6\sqrt{3}\times m}=\frac{r}{2}$ $\frac{-3\sqrt{3}\times rm+27rr-4q^3+4q^3}{54r-6\sqrt{3}\times m}=\frac{r}{2}\frac{-3\sqrt{3}\times rm+27rr}{54r-6\sqrt{3}\times m}=\frac{54rr-6\sqrt{3}\times rm-6\sqrt{3}\times rm+54rr}{108r-12\sqrt{3}\times m}$ $=\frac{108rr-12\sqrt{3}\times rm}{108r-12\sqrt{3}\times m}=r$. But it has been before ſhewn that x^3-qx is $=\frac{r}{2}-s+\frac{2q^3}{27r-54s}$. Therefore x^3-qx is $=r$; and $\sqrt[3]{\frac{r}{2}-s}+\frac{q}{3\sqrt[3]{\frac{r}{2}-s}}$ is the true value of x in the cubick equation $x^3-qx=r$. Q. E. D.

Inveſtigation

Investigation of the third expression of the value of the root x.

11. The third expreſſion for the value of x, or the laſt of the two mentioned in Art. 8. to wit, $\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}$, or $\sqrt[3]{\frac{r}{2}+s}+\sqrt[3]{\frac{r}{2}-s}$, may be obtained as follows.

Since a^3+b^3 is $=r$, it follows that b^3 will be $=r-a^3$. But a^3 is ſhewn in Art. 5. to be $=\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$. Therefore $r-a^3$ is $=r-\overline{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}=\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$. Conſequently b^3 is $=\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$, and b is $=\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}$, and $a+b$, or x, is $=\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}$, or (putting ss, as before, $=\frac{rr}{4}-\frac{q^3}{27}$,) $\sqrt[3]{\frac{r}{2}+s}+\sqrt[3]{\frac{r}{2}-s}$. Q. E. I.

Synthetick demonſtration of the truth of the ſaid third expreſſion.

12. Here again we may demonſtrate ſynthetically, that this expreſſion is equal to the true value of x in the equation $x^3-qx=r$, by ſubſtituting it for x in the left-hand ſide of the ſaid equation. For, if we make that ſubſtitution, we ſhall find that the value of x^3-qx,

5 X 2 thence

thence arising, will be equal to r. This may be done in the following manner.

If x is $=\sqrt[3]{\overline{\frac{r}{2}+s}}+\sqrt[3]{\overline{\frac{r}{2}-s}}$, or $\overline{\frac{r}{2}+s}|^{\frac{1}{3}}+\overline{\frac{r}{2}-s}|^{\frac{1}{3}}$, we shall have

$$x^3=\frac{r}{2}+s+3\times\overline{\frac{r}{2}+s}|^{\frac{2}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}+3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{2}{3}}+\frac{r}{2}-s=r+3\times\overline{\frac{r}{2}+s}|^{\frac{2}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}+3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{2}{3}}=r+$$

$$3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}+3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}=$$

$$r+3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}\times\overline{\frac{rr}{4}-ss}|^{\frac{1}{3}}+3\times\overline{\frac{rr}{4}-ss}|^{\frac{1}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}=r+3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}$$

$$\times\overline{\frac{rr}{4}-\frac{rr}{4}+\frac{q^3}{27}}|^{\frac{1}{3}}+3\times\overline{\frac{rr}{4}-\frac{rr}{4}+\frac{q^3}{27}}|^{\frac{1}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}=r+3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}\times\overline{\frac{q^3}{27}}|^{\frac{1}{3}}+$$

$$3\times\overline{\frac{q^3}{27}}|^{\frac{1}{3}}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}=r+3\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}\times\frac{q}{3}+3\times\frac{q}{3}\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}=r+q\times$$

$\overline{\frac{r}{2}+s}|^{\frac{1}{3}}+q\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}$. And qx will be $=q\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}+q\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}$. Therefore x^3-qx will be $=r+q\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}+q\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}-q\times\overline{\frac{r}{2}+s}|^{\frac{1}{3}}-q\times\overline{\frac{r}{2}-s}|^{\frac{1}{3}}=r$; and consequently $\overline{\frac{r}{2}+s}|^{\frac{1}{3}}+\overline{\frac{r}{2}-s}|^{\frac{1}{3}}$, or $\sqrt[3]{\overline{\frac{r}{2}+s}}+\sqrt[3]{\overline{\frac{r}{2}-s}}$, is the true value of x in the cubick equation $x^3-qx=r$.

13. N.B. I do not remember to have seen these substitutions, or synthetical demonstrations of the truth of the expressions given by CARDAN's rule, in any book of algebra.

14. *An*

14. *An Example of the Resolution of a Cubick Equation of the aforesaid Form, $x^3-qx=r$, by means of each of the three foregoing Expressions.*

I will here insert a single example of a numeral equation of the foregoing form, $x^3-qx=r$, resolved by each of the three expressions above-mentioned, in order to shew that they will all three bring out the same number for its root.

Let it therefore be required to find the value of x in the cubick equation $x^3-3x=18$.

15. In this equation q is $=3$, and r is $=18$. Therefore $\sqrt{q}$ is $=\sqrt{3}$, and $\frac{2q\sqrt{q}}{3\sqrt{3}}$ is $=\frac{2\times 3\sqrt{3}}{3\sqrt{3}}=2$, which is greatly less than 18, or r. Therefore this equation comes under the above-mentioned rule, and may be resolved by either of the foregoing expressions.

Resolution of the equation $x^3-3x=18$ by the first of the said expressions.

16. The first of those expressions is $\sqrt[3]{\frac{r}{2}+s}+\frac{q}{3\sqrt[3]{\frac{r}{2}+s}}$, in which s stands for $\sqrt{\frac{rr}{4}-\frac{q^3}{27}}$.

Now

Now, since q is $=3$, $\frac{q}{3}$ will be $=\frac{3}{3}=1$, and consequently $\frac{q^3}{27}$, or the cube of $\frac{q}{3}$, will also be $=1$. And, since r is $=18$, we shall have $\frac{r}{2}=9$, and $\frac{rr}{4}=81$, and consequently $\frac{rr}{4}-\frac{q^3}{27}=81-1=80$; that is, ss will be $=80$. Therefore s is $=\sqrt{80}=\sqrt{16}\times\sqrt{5}=4\sqrt{5}$; and $\frac{r}{2}+s$ is $=9+4\sqrt{5}=\frac{72+32\sqrt{5}}{8}=\frac{27+27\sqrt{5}+45+5\sqrt{5}}{8}$; and consequently $\sqrt[3]{\frac{r}{2}+s}$ is $=\frac{3+\sqrt{5}}{2}$. Therefore $3\times\sqrt[3]{\frac{r}{2}+s}$ is $=\frac{3\times\overline{3+\sqrt{5}}}{2}$, and $\frac{q}{3\sqrt[3]{\frac{r}{2}+s}}$ is $=3\times\frac{2}{3\times\overline{3+\sqrt{5}}}=\frac{2}{3+\sqrt{5}}$; and $\sqrt[3]{\frac{r}{2}+s}+\frac{q}{3\sqrt[3]{\frac{r}{2}+s}}$ is $=\frac{3+\sqrt{5}}{2}+\frac{2}{3+\sqrt{5}}=\frac{\overline{3+\sqrt{5}}\times\overline{3+\sqrt{5}}+4}{2\times\overline{3+\sqrt{5}}}=\frac{9+6\sqrt{5}+5+4}{2\times\overline{3+\sqrt{5}}}=\frac{18+6\sqrt{5}}{2\times\overline{3+\sqrt{5}}}=\frac{6\times\overline{3+\sqrt{5}}}{2\times\overline{3+\sqrt{5}}}=\frac{6}{2}=3$. Therefore 3 is the value of x in the equation $x^3-3x=18$. And so we shall find it to be upon trial: for, if x is taken $=3$, we shall have $x^3=27$, and $3x=3\times3=9$, and $x^3-3x=27-9=18$. And thus we see that the first of the three foregoing expressions, to wit, $\sqrt[3]{\frac{r}{2}+s}+\frac{q}{3\sqrt[3]{\frac{r}{2}+s}}$, has given us the true value of x in this equation.

Resolution

Resolution of the same equation by the second and third of the foregoing expressions.

17. We are now to resolve the same equation $x^3-3x=18$ by means of the two other expressions, to wit, $\sqrt[3]{\frac{r}{2}-s}+\frac{q}{3\sqrt[3]{\frac{r}{2}-s}}$, and $\sqrt[3]{\frac{r}{2}+s}+\sqrt[3]{\frac{r}{2}-s}$.

Now, since r is $=18$, and s has been shewn to be $=\sqrt{80}$, or $4\sqrt{5}$, we shall have $\frac{r}{2}-s=9-4\sqrt{5}=\frac{72-32\sqrt{5}}{8}=\frac{27-27\sqrt{5}+45-5\sqrt{5}}{8}$, and $\sqrt[3]{\frac{r}{2}-s}=\frac{3-\sqrt{5}}{2}$. Therefore $3\sqrt[3]{\frac{r}{2}-s}$ is $=\frac{3\times\overline{3-\sqrt{5}}}{2}$, and $\frac{q}{3\sqrt[3]{\frac{r}{2}-s}}$ is $=\frac{3}{\frac{3\times\overline{3-\sqrt{5}}}{2}}=3\times\frac{2}{3\times\overline{3-\sqrt{5}}}=\frac{2}{3-\sqrt{5}}$. Consequently $\sqrt[3]{\frac{r}{2}-s}+\frac{q}{3\sqrt[3]{\frac{r}{2}-s}}$ is $=\frac{3-\sqrt{5}}{2}+\frac{2}{3-\sqrt{5}}=\frac{\overline{3-\sqrt{5}}\times\overline{3-\sqrt{5}}+4}{2\times\overline{3-\sqrt{5}}}=\frac{9-6\sqrt{5}+5+4}{2\times\overline{3-\sqrt{5}}}=\frac{18-6\sqrt{5}}{2\times\overline{3-\sqrt{5}}}=\frac{9-3\sqrt{5}}{3-\sqrt{5}}=\frac{3\times\overline{3-\sqrt{5}}}{3-\sqrt{5}}=3$. Therefore x is $=3$, as it was found to be by the first expression.

18. The third expression $\sqrt[3]{\frac{r}{2}+s}+\sqrt[3]{\frac{r}{2}-s}$ is in the present case $=\frac{3+\sqrt{5}}{2}+\frac{3-\sqrt{5}}{2}=\frac{6}{2}=3$. Therefore by this expression, as well as by both the former, the value of x in the equation $x^3-3x=18$ comes out to be 3.

19. *Note.* The foregoing method of reſolving the cubick equation $x^3-qx=r$, when r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, and a like method of reſolving the cubick equation $x^3+qx=r$ (which holds good in all caſes, whatever be the magnitudes of q and r), are uſually known by the name of CARDAN'S *rules*, becauſe they were firſt publiſhed by him in his treatiſe of algebra, intitled, *Ars magna, quam vulgò Coſſam vocant, ſeu regulas Algebraicas*, in the year 1545, although, as he himſelf informs us, they were firſt found out by one SCIPIO FERREUS of Bononia. See WALLIS'S algebra, Chap. XIII.

Of the ſecond caſe of the cubick equation $x-qx=r$; in which r is leſs than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is leſs than $\frac{q^3}{27}$, and which cannot be reſolved by CARDAN'S *rule.*

20. The remaining caſe of the cubick equation $x^3-qx=r$, in which r is leſs than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is leſs than $\frac{q^3}{27}$, and which conſequently cannot be reſolved by the rules above-mentioned, has, upon that account, obtained amongſt algebraiſts the name of the *irreducible caſe:* at leaſt it is often called by the French writers of algebra *le cas irréductible.* The object of the remaining pages of

of this paper is to shew how, by the help of Sir ISAAC NEWTON's famous binomial theorem, the foregoing solution of the other, or first, case of this equation may be, as it were, extended to this latter case, or, rather, may be made the means of discovering, by a very peculiar train of reasoning, another solution, that shall be adapted to it.

21. By the binomial theorem it appears that the cube-root of the binomial quantity $a+b$ (in which a is supposed to be greater than b) is equal to the following infinite series, to wit, $a^{\frac{1}{3}} + \frac{a^{\frac{1}{3}}b}{3a} - \frac{a^{\frac{1}{3}}b^2}{9a^2} + \frac{5a^{\frac{1}{3}}b^3}{81a^3} - \frac{10a^{\frac{1}{3}}b^4}{243a^4} + \frac{22a^{\frac{1}{3}}b^5}{729a^5} - \frac{154a^{\frac{1}{3}}b^6}{6561a^6} + \frac{2618a^{\frac{1}{3}}b^7}{137,781a^7} -$ &c. or to $a^{\frac{1}{3}} \times$ the infinite series $1 + \frac{b}{3a} - \frac{b^2}{9a^2} + \frac{5b^3}{81a^3} - \frac{10b^4}{243a^4} + \frac{22b^5}{729a^5} - \frac{154b^5}{6561a^6} + \frac{2618b^7}{137,781a^7} -$ &c. or (if we put the capital letters A, B, C, D, E, F, G, H, &c. for the several numeral coefficients, $1, \frac{1}{3}, \frac{1}{9}, \frac{5}{81}, \frac{10}{243}, \frac{22}{729}, \frac{154}{6561}, \frac{2618}{137,781}$, &c. of the terms of the series, respectively,) $a^{\frac{1}{3}} \times$ the infinite series $1 + \frac{1Ab}{3a} - \frac{2Bb^2}{6a^2} + \frac{5Cb^3}{9a^3} - \frac{8Db^4}{12a^4} + \frac{11Eb^5}{15a^5} - \frac{14Fb^6}{18a^6} + \frac{17Gb^7}{21a^7} -$ &c. in which series both the numerators and the denominators of the generating fractions, $\frac{2}{6}, \frac{5}{9}, \frac{8}{12}, \frac{11}{15}, \frac{14}{18}, \frac{17}{21}$, &c. following the second term, increase continually by 3, so that it will be easy for any one to continue the series to as many terms as he shall think proper.

22. In like manner the cube-root of the residual quantity $a-b$ is found by the same binomial theorem to

be equal to the infinite series $a^{\frac{1}{3}} - \frac{a^{\frac{1}{3}}b}{3a} - \frac{a^{\frac{1}{3}}b^2}{9a^2} - \frac{5a^{\frac{1}{3}}b^3}{81a^3} - \frac{10a^{\frac{1}{3}}b^4}{243a^4} - \frac{22a^{\frac{1}{3}}b^5}{729a^5} - \frac{154a^{\frac{1}{3}}b^6}{6561a^6} - \frac{2618a^{\frac{1}{3}}b^7}{137,781a^7} -$ &c. or to $a^{\frac{1}{3}} \times$ the infinite series $1 - \frac{b}{3a} - \frac{b^2}{9a^2} - \frac{5b^3}{81a^3} - \frac{10b^4}{243a^4} - \frac{22b^5}{729a^5} - \frac{154b^6}{6561a^6} - \frac{2618b^7}{137,781a^7} -$ &c. or $a^{\frac{1}{3}} \times$ the infinite series $1 - \frac{1Ab}{3a} - \frac{2Bb^2}{6a^2} - \frac{5Cb^3}{9a^3} - \frac{8Db^4}{12a^4} - \frac{11Eb^5}{15a^5} - \frac{14Fb^6}{18a^6} - \frac{17Gb^7}{21a^7} -$ &c. in which series the numeral coefficients of the several terms are the same as in the series that expresses the cube-root of $a+b$, but the terms which involve the odd powers of b (which in that series are marked with the sign +, or all added to the first term,) are in this latter series marked with the sign −, and are all to be subtracted from the first term, as well as the terms which involve the even powers of b, which are to be subtracted from the first term in both serieses.

PROBLEM.

23. *Let it now be required to resolve the first case of the cubick equation $x^3 - qx = r$, in which r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, by means of an infinite series derived from the expressions given by* CARDAN'S *rule.*

SOLUTION.

We have seen in Art. 11. that, if ss be put $= \frac{rr}{4} - \frac{q^3}{27}$, the value of x in this equation will be $= \sqrt[3]{\frac{r}{2} + s} +$

$\sqrt[3]{\frac{r}{2} - s}$. For the sake of avoiding fractions, let e be put $= \frac{r}{2}$. And we shall have $x = \sqrt[3]{e+s} + \sqrt[3]{e-s}$. But (by Art. 21.) $\sqrt[3]{e+s}$ is $= e^{\frac{1}{3}} \times$ the infinite series $1 + \frac{s}{3e} - \frac{ss}{9ee} + \frac{5s^3}{81e^3} - \frac{10s^4}{243e^4} + \frac{22s^5}{729e^5} - \frac{154s^6}{6561e^6} + \frac{2618s^7}{137,781e^7} -$ &c; and, by Art. 22. $\sqrt[3]{e-s}$ is $= e^{\frac{1}{3}} \times$ the infinite series $1 - \frac{s}{3e} - \frac{ss}{9ee} - \frac{5s^3}{81e^3} - \frac{10s^4}{243e^4} - \frac{22s^5}{729e^5} - \frac{154s^6}{6561e^6} - \frac{2618s^7}{137,781e^7} -$ &c. Therefore $\sqrt[3]{e+s} + \sqrt[3]{e-s}$ is equal to $e^{\frac{1}{3}} \times$ the sum of these two serieses, that is, to $e^{\frac{1}{3}} \times$ the infinite series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6} -$ &c; and consequently the root of the equation $x^3 - qx = r$ is $= e^{\frac{1}{3}} \times$ the infinite series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6} -$ &c. *ad infinitum.* Q. E. F.

24. *Note.* This series must always converge, because ss, or $\frac{rr}{4} - \frac{q^3}{27}$, is always less than $\frac{rr}{4}$, or ee. And, when ss is considerably less than ee, or $\frac{rr}{4} - \frac{q^3}{27}$ is considerably less than $\frac{rr}{4}$, or $\frac{rr}{4}$ is very little greater than $\frac{q^3}{27}$, the convergency of the terms of this series will be sufficient to make it useful. But in other cases, when $\frac{rr}{4}$ is much greater than $\frac{q^3}{27}$, (as when it is triple, quadruple or quintuple of it, or still greater,) the terms of this series will converge so slowly as to render it very unfit for practice. And indeed in the most favourable cases it will, as I believe, be less convenient in practice than the expression

$\sqrt[3]{e+s}+\sqrt[3]{e-s}$, or $\sqrt[3]{\frac{r}{2}+s}+\sqrt[3]{\frac{r}{2}-s}$, from which it was derived. However, that it may appear that this series will exhibit the root of the equation $x^3-qx=r$ truly, if we will take the neceſſary pains of computing it, I will here ſubjoin one example, and no more, of the reſolution of a cubick equation of that form by means of it, having taken care to chuſe ſuch numbers for q and r as ſhall make $\frac{rr}{4}$ be but little greater than $\frac{q^3}{27}$, and conſequently ſhall give us only a ſmall number for the fraction $\frac{ss}{ee}$, by the continual multiplication of which the terms of the ſeries are generated.

An example of the reſolution of a cubick equation of the aforeſaid form, $x^3-qx=r$, *in the firſt caſe of it, in which* r *is greater than* $\frac{2q\sqrt{q}}{3\sqrt{3}}$, *or* $\frac{rr}{4}$ *is greater than* $\frac{q^3}{27}$, *by means of the expreſſion* $e^{\frac{1}{3}}\times$ *the infinite ſeries* $2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}$ *– &c. obtained in Art.* 23.

25. Let it be required to reſolve the equation $x^3-300x=2108$ by means of the infinite ſeries $e^{\frac{1}{3}}\times\overline{2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}-\&c.}$ obtained in Art. 23. by the help of Sir ISAAC NEWTON's binomial theorem.

Here

Here q is $= 300$, and r is $= 2108$. Therefore $\frac{2q\sqrt{q}}{3\sqrt{3}}$ is $= \frac{2 \times 300 \times \sqrt{300}}{3 \times \sqrt{3}} = \frac{2 \times 100 \times \sqrt{300}}{\sqrt{3}} = \frac{2 \times 100 \times 10\sqrt{3}}{\sqrt{3}} = 2 \times 100 \times 10 = 2000$, which is lefs than 2108, or r. Therefore this equation comes under the cafe of CARDAN's rule, and confequently may be refolved by means of the infinite feries $e^{\frac{1}{3}} \times \overline{|2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6}} -$ &c. if that feries has been juftly derived from the third expreffion of the value of x given by CARDAN's rule.

26. Now, fince r is $= 2108$, $\frac{r}{2}$, or e, will be $= 1054$, and $\frac{rr}{4}$, or ee, will be $= 1,110,916$. And, fince q is $= 300$, $\frac{q}{3}$ will be $= 100$, and $\frac{q^3}{27}$, or the cube of $\frac{q}{3}$, will be $= 1000,000$; and confequently ss, or $\frac{rr}{4} - \frac{q^3}{27}$, will be $(= 1,110,916 - 1000,000) = 110,916$. Therefore, the fraction $\frac{ss}{ee}$ is $= \frac{110,916}{1,110,916} = .0998$. Therefore $\frac{s^4}{e^4}$ is $= \overline{.0998}|^2 = .009,950$, and $\frac{s^6}{e^6}$ is $= \overline{.0998}|^3 = .000,992$; and $\frac{2ss}{9ee}$ is $= \frac{2}{9} \times .0998 = \frac{.1996}{9} = .022,177$; and $\frac{20s^4}{243e^4}$ is $= \frac{20}{243} \times .009,950 = \frac{.199,000}{243} = .000,818$; and $\frac{308s^6}{6561e^6}$ is $= \frac{308}{6561} \times .000,992 = \frac{.305,536}{6561} = .000,046$; and confequently, $\frac{2ss}{9ee} + \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6}$ is $= .022,177 + .000,818, + .000,046 = .023,041$; and $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6}$ is $= 2 - .023,041$

=

$= 1.976,959$. But e is $= 1054$. Consequently, $e^{\frac{1}{3}}$, or $\sqrt[3]{e}$, is $= \sqrt[3]{1054} = 10.1768$. Therefore $e^{\frac{1}{3}} \times$ the series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6}$ – &c. is $= 10.1768 \times 1.976,959 = 20.119,116$. Therefore the root of the equation $x^3 - 300x = 2108$ is $= 20.119,116$. Q. E. I.

27. This value of x is true to five places of figures, the more accurate value of it being 20.119,053, as will easily appear by prosecuting it to three or four more places of figures by Mr. RAPHSON's method of approximation.

28. That 20.119 is very nearly equal to, but somewhat less than, the true value of x in the equation $x^3 - 300x = 2108$, will appear by substituting it instead of x in the left-hand side of that equation. For, if we take $x = 20.119$, we shall have $xx = 404.774,161$, and $x^3 = 8143.651,345,159$, and $300x = 6035.700$; and consequently, $x^3 - 300x = 8143.651,345,159, - 6035.700 = 2107.951,345,159$, which is somewhat less than 2108, or the accurate value of $x^3 - 300x$ in the proposed equation $x^3 - 300x = 2108$. Therefore, 20.119 must be nearly equal to, but somewhat less than, the accurate value of x in that equation.

29. It appears therefore from this example, that this expression, $e^{\frac{1}{3}} \times$ the infinite series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6}$ – &c. does truly exhibit the root of the equation

4 x^3

$x^3-qx=r$ in that caſe of it which falls under CARDAN's rule, or in which r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$.

30. I now proceed to conſider the problem which is the principal object of this paper, which is to ſhew how from the ſeries $e^{\frac{1}{3}} \times \sqrt{2-\frac{2ss}{9ee}-\frac{20s^4}{243s^4}-\frac{308s^6}{6561s^6}-\&c.}$ we may derive another ſeries, differing from it only in the ſigns of ſome of the terms, by which the equation $x^3-qx=r$ may be reſolved in that other caſe of it which does not come under CARDAN's rule, and in which r is leſs than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is leſs than $\frac{q^3}{27}$: and this without any mention of either impoſſible or negative quantities.

P R O B L E M.

To reſolve, by means of an infinite ſeries derived from the infinite ſeries $e^{\frac{1}{3}} \times \sqrt{2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}-\&c.}$ *the ſecond caſe of the cubick equation* $x^3-qx=r$, *in which* r *is leſs than* $\frac{2q\sqrt{q}}{3\sqrt{3}}$, *or* $\frac{rr}{4}$ *is leſs than* $\frac{q^3}{27}$.

S O L U T I O N.

31. We have ſeen that in the firſt caſe of the equation $x^3-qx=r$, in which $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, the pro-

duct

duct of $e^{\frac{1}{3}}$ into the series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6}$ − &c. *ad infinitum*, is equal to the root x. Now there are two different ways of computing this series, which (though not equally short and convenient in practice) are nevertheless equally just and true: and therefore they must both produce the same result for the value of the series. The first way of computing it is the common one, which consists of the following processes; to wit, first, to compute the quantities $\frac{rr}{4}$ and $\frac{q^3}{27}$, as was done in the foregoing example, art. 26, where $\frac{rr}{4}$ was found to be $= 1,110,916$, and $\frac{q^3}{27}$ to be 1000,000; 2dly, to subtract $\frac{q^3}{27}$ from $\frac{rr}{4}$, in order to get the quantity ss, which is equal to their difference, and which in the foregoing example was 110,916; 3dly, to divide ss by ee, so as to obtain the value of the fraction $\frac{ss}{ee}$; as in the foregoing example we found the fraction $\frac{110,916}{1,110,916}$ to be $= .0998$; 4thly, to compute the powers of the value found for the fraction $\frac{ss}{ee}$; as in the foregoing example we computed those of .0998, and found its square to be .009,950, and its cube to be .000,992; 5thly, to multiply $\frac{ss}{ee}$, and its powers $\frac{s^4}{e^4}$, $\frac{s^6}{e^6}$, &c. into the co-efficients $\frac{2}{9}$, $\frac{20}{243}$, $\frac{308}{6561}$, &c. respectively,

respectively, as in the foregoing example we multiplied .0998 into $\frac{2}{9}$, and .009,950 into $\frac{20}{243}$, and .000,992 into $\frac{308}{6561}$, and found the products to be .022,117, .000,818, and .000,046; and 6thly, to subtract all the products so obtained from 2 the first term of the series. This is the common and the proper way of computing the series $2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}-$ &c. when we want to make use of it in practice. But it may also be computed in another manner, which may be described as as follows.

Instead of ss insert the compound quantity $\frac{rr}{4}-\frac{q^3}{27}$ itself, to which ss is equal, in all the terms of it. And it will be thereby converted into the following series, to wit, $2-\frac{2}{9ee}\times\overline{\frac{rr}{4}-\frac{q^3}{27}}-\frac{20}{243e^4}\times\overline{\frac{rr}{4}-\frac{q^3}{27}}|^2-\frac{308}{6561e^6}\times\overline{\frac{rr}{4}-\frac{q^3}{27}}|^3-$ &c. or (because ee is $=\frac{rr}{4}$, and consequently $e^4=\frac{r^4}{16}$, and $e^6=\frac{r^6}{64}$) $2-\frac{8}{9rr}\times\overline{\frac{rr}{4}-\frac{q^3}{27}}-\frac{320}{243r^4}\times\overline{\frac{rr}{4}-\frac{q^3}{27}}|^2-\frac{19712}{6561r^6}\times\overline{\frac{rr}{4}-\frac{q^3}{27}}|^3-$ &c. or $2-\frac{8}{9rr}\times\overline{\frac{rr}{4}-\frac{q^3}{27}}-\frac{320}{243r^4}\times\overline{\frac{r^4}{16}-\frac{2rrq^3}{4\times27}+\frac{q^6}{27\times27}}-\frac{19712}{6561r^6}\times\overline{\frac{r^6}{64}-\frac{3r^4q^3}{16\times27}+\frac{3rrq^6}{4\times27\times27}-\frac{q^9}{27\times27\times27}}-$ &c. or (putting the Greek letters α, β, γ, &c. for the numeral co-efficients $\frac{8}{9}$, $\frac{320}{243}$, $\frac{19712}{6561}$, &c. respectively) $2-\frac{\alpha}{rr}\times\overline{\frac{rr}{4}-\frac{q^3}{27}}-\frac{\beta}{r^4}\times\overline{\frac{r^4}{16}-\frac{2rrq^3}{4\times27}+\frac{q^6}{27\times27}}-\frac{\gamma}{r^6}\times\overline{\frac{r^6}{64}-\frac{3r^4q^3}{16\times27}+\frac{3rrq^6}{4\times27\times27}-\frac{q^9}{27\times27\times27}}-$ &c.

&c. or $2 - \frac{\alpha}{4} + \frac{\alpha q^3}{27rr} - \frac{\beta}{16} + \frac{2\beta q^3}{4 \times 27rr} - \frac{\beta q^6}{27 \times 27 \times r^4} - \frac{\gamma}{64} + \frac{3\gamma q^3}{16 \times 27rr} - \frac{3\gamma q^6}{4 \times 27 \times 27r^4} + \frac{\gamma q^9}{27 \times 27 \times 27r^6}$ − &c. which consists of a much greater number of terms than the series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6}$ − &c. from which it is derived, and in which many of the terms are much more complicated than in that former series. Nevertheless, since the compound quantity $\frac{rr}{4} - \frac{q^3}{27}$ is equal to ss, the insertion of it instead of ss in the terms of that former series cannot alter its real value, though it will make it much more difficult to compute. It must therefore be true of the new and complicated series $2 - \frac{\alpha}{4} + \frac{\alpha q^3}{27rr} - \frac{\beta}{16} + \frac{2\beta q^3}{4 \times 27rr} - \frac{\beta q^6}{27 \times 27r^4} - \frac{\gamma}{64} + \frac{3\gamma q^3}{16 \times 27rr} - \frac{3\gamma q^6}{4 \times 27 \times 27r^4} + \frac{\gamma q^9}{27 \times 27 \times 27r^6}$ − &c. as well as of the former series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6}$ − &c. that, if it be multiplied into $e^{\frac{1}{3}}$, or $\sqrt[3]{e}$, or $\sqrt[3]{\frac{r}{2}}$, the series thereby produced will be equal to the value of x in the equation $x^3 - qx = r$, or that, if the said series be cubed, and also multiplied into q, and from its cube the product of its multiplication into q be subtracted, the remainder will be equal to r, or rather will approximate to the value of r, because, as the quantity substituted for x in the compound quantity $x^3 - qx$ is only a part of an infinite series

that is equal to x, it is impossible that the said remainder (which is produced by that substitution) should be accurately equal to the whole value of r.

32. By the help of this observation we may from the foregoing series $2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}-$ &c. which, being multiplied into $e^{\frac{1}{3}}$, or the cube-root of $\frac{r}{2}$, expresses the value of x in the equation $x^3-qx=r$, in the first case of that equation, when $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, deduce another series resembling the former in the composition of its terms, but differing from it in the signs to be prefixed to some of them, that will likewise (if multiplied into $e^{\frac{1}{3}}$, or the cube-root of $\frac{r}{2}$) express the value of x in the second case of the same equation, in which $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, and which cannot be resolved by CARDAN's rules. This may be done as follows.

33. If in this second case of the equation $x^3-qx=r$ we subtract $\frac{rr}{4}$ from $\frac{q^3}{27}$, and call the remainder ss (as we before put ss for the opposite difference $\frac{rr}{4}-\frac{q^3}{27}$) and then raise the powers of ss, to wit, s^4, s^6, s^8, s^{10}, and also the correspondent powers of its value $\frac{q^3}{27}-\frac{r^4}{4}$, to wit, $\left|\frac{q^3}{27}-\frac{rr}{4}\right|^2$, $\left|\frac{q^3}{27}-\frac{rr}{4}\right|^3$, $\left|\frac{q^3}{27}-\frac{rr}{4}\right|^4$, $\left|\frac{q^3}{27}-\frac{rr}{4}\right|^5$, &c. the even

 powers

powers of the difference $\frac{q^3}{27} - \frac{rr}{4}$, to wit, $\overline{\frac{q^3}{27} - \frac{rr}{4}}\Big|^2$, $\overline{\frac{q^3}{27} - \frac{rr}{4}}\Big|^4$, &c. will conſiſt of the very ſame terms, or the ſame powers, products, and multiples of the two original quantities $\frac{rr}{4}$ and $\frac{q^3}{27}$, and with the ſame ſigns + and − prefixed to them, as were before contained in the even powers of the oppoſite difference $\frac{rr}{4} - \frac{q^3}{27}$, when $\frac{rr}{4}$ was greater than $\frac{q^3}{27}$. Thus, for example, the ſquare of $\frac{rr}{4} - \frac{q^3}{27}$ in the former caſe was $\frac{r^4}{16} - \frac{2rrq^3}{4 \times 27} + \frac{q^6}{27 \times 27}$; and in the preſent caſe the ſquare of $\frac{q^3}{27} - \frac{rr}{4}$ is $\frac{q^6}{27 \times 27} - \frac{2q^3rr}{27 \times 4} + \frac{r^4}{16}$, which conſiſts of the ſame terms, and with the ſame ſigns prefixed to them, as were contained in the ſquare of $\frac{rr}{4} - \frac{q^3}{27}$, and differs from it only in the order in which the extreme terms $\frac{r^4}{16}$ and $\frac{q^6}{27 \times 27}$ are placed. And the ſame obſervation is true concerning all the other even powers of the oppoſite differences $\frac{rr}{4} - \frac{q^3}{27}$ and $\frac{q^3}{27} - \frac{rr}{4}$.

Alſo the odd powers of the difference $\frac{q^3}{27} - \frac{rr}{4}$, to wit, $\frac{q^3}{27} - \frac{rr}{4}$ itſelf, and $\overline{\frac{q^3}{27} - \frac{rr}{4}}\Big|^3$, $\overline{\frac{q^3}{27} - \frac{rr}{4}}\Big|^5$, &c. will conſiſt of the ſame terms, or of the ſame powers, products, and multiples of the two original quantities $\frac{rr}{4}$ and $\frac{q^3}{27}$, as were contained in the ſame odd powers of the oppoſite difference

rr

$\frac{rr}{4} - \frac{q^3}{27}$, when $\frac{rr}{4}$ was greater than $\frac{q^3}{27}$. But the signs prefixed to the said terms will be contrary to those which were prefixed to them in the former case. Thus, the cube of $\frac{rr}{4} - \frac{q^3}{27}$ in the former case was $\frac{r^6}{64} - \frac{3r^4q^3}{16 \times 27} + \frac{3rrq^6}{4 \times 27 \times 27} - \frac{q^9}{27 \times 27 \times 27}$; and the cube of $\frac{q^3}{27} - \frac{rr}{4}$ in the present case is $\frac{q^9}{27 \times 27 \times 27} - \frac{3q^6rr}{27 \times 27 \times 4} + \frac{3q^3r^4}{27 \times 16} - \frac{r^6}{64}$, which consists of the same terms as are contained in the cube of $\frac{rr}{4} - \frac{q^3}{27}$: but they are placed in a contrary order to that in which they stood in the former case; and the signs that are prefixed to them are contrary in every term to what they were before. And the same is true of all the other odd powers of these opposite differences of $\frac{rr}{4}$ and $\frac{q^3}{27}$.

34. It follows, therefore, that if ss be put for $\frac{q^3}{27} - \frac{rr}{4}$ in this latter case of the equation $x^3 - qx = r$, in which $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, the even powers of ss, to wit, s^4, s^8, s^{12}, s^{16}, &c. will represent, or be equal to, the same powers, products, and multiples of the two original quantities $\frac{rr}{4}$ and $\frac{q^3}{27}$ in the present case as they represented in the former case, when $\frac{rr}{4}$ was greater than $\frac{q^3}{27}$, and ss was made to stand for $\frac{rr}{4} - \frac{q^3}{27}$; and the several terms represented by the said even powers of ss will have the same signs

signs + and − prefixed to them respectively in this second case as in the first case. And it likewise follows that the odd powers of ss, to wit, ss, s^6, s^{10}, s^{14}, &c. will also represent, or be equal to, the same powers, products, and multiples of the two original quantities $\frac{rr}{4}$ and $\frac{q^3}{27}$, as in the former case; but the signs + and − prefixed to the several terms represented by the said odd powers of ss will be contrary to what they were before.

35. If therefore in this second case of the equation $x^3-qx=r$, in which $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, we put $ss=\frac{q^3}{27}-\frac{rr}{4}$, the series $2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}-$ &c. will represent, or be equal to, a system of terms, derived from the two original quantities $\frac{rr}{4}$ and $\frac{q^3}{27}$, that will be the very same in point of composition, that is, will be the very same powers, products, and multiples of $\frac{rr}{4}$ and $\frac{q^3}{27}$, as the terms that were represented by it in the former case, in which $\frac{rr}{4}$ was greater than $\frac{q^3}{27}$: but the terms so represented will not *all* have the same signs + and − prefixed to them, as they had before; but those terms in the said system, which are represented by the terms of the series $2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}-$ &c. which involve the even powers of ss, to wit, $\frac{20s^4}{243e^4}$, &c. will have the same signs

prefixed

prefixed to them as they had before when ss stood for $\frac{rr}{4} - \frac{q^3}{27}$; and those terms of the said system which are represented by the terms of the said series which involve the odd powers of ss, to wit, $\frac{2ss}{9ee}$ and $\frac{308s^6}{6561e^6}$, &c. will have contrary signs to those they had before. Consequently, if we change the signs of those terms in the series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6} -$ &c. which involve the odd powers of ss, to wit, the terms $\frac{2ss}{9ee}$ and $\frac{308s^6}{6561e^6}$ &c. the new series thereby produced, to wit, $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. will represent, or be equal to, a system of terms which will not only be the very same in point of composition (or will be the same powers, products, and multiples of the two original quantities $\frac{rr}{4}$ and $\frac{q^3}{27}$), as those which were represented by the series $2 - \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6} -$ &c. in the former case, but will also be connected with each other in exactly the same manner by the signs + and − : that is, by Art. 31. the said new series will represent, or be equal to, the following system of terms, to wit, $2 - \frac{\alpha}{4} + \frac{\alpha q^3}{27rr} - \frac{\beta}{16} + \frac{2\beta q^3}{4 \times 27rr} - \frac{\beta q^6}{27 \times 27r^4} - \frac{\gamma}{64} + \frac{3\gamma q^3}{16 \times 27rr} - \frac{3\gamma q^6}{4 \times 27 \times 27r^4} + \frac{\gamma q^9}{27 \times 27 \times 27r^6} -$ &c.

But

But it has been ſhewn (in Art. 31.) that, if this ſyſtem of terms be multiplied into $e^{\frac{1}{3}}$, or the cube-root of $\frac{r}{2}$, and the ſeries thence produced be cubed, and alſo multiplied into q, and from its cube the product of its multiplication into q be ſubtracted, the remainder thereby obtained will be (nearly) equal to r. Therefore, if the ſeries $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.$ (which repreſents, or is equal to, the ſaid ſyſtem of terms, when $\frac{rr}{4}$ is leſs than $\frac{q^3}{27}$, and ss is made $= \frac{q^3}{27} - \frac{rr}{4}$,) be multiplied by $e^{\frac{1}{3}}$, or the cube-root of $\frac{r}{2}$, and the ſeries thence produced be cubed, and alſo multiplied into q, and from the cube of the ſaid ſeries the product of its multiplication into q be ſubtracted, it will follow that the remainder thereby obtained will be (nearly) equal to r; that is, the product of the multiplication of $e^{\frac{1}{3}}$, or the cube-root of $\frac{r}{2}$, into the infinite ſeries $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.$ is equal to the value of x in the equation $x^3 - qx = r$ in the ſecond caſe of it, when $\frac{rr}{4}$ is leſs than $\frac{q^3}{27}$. Q. E. I.

36. This ſeries $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.$ does not always converge, but only when ss is leſs than ee, or $\frac{q^3}{27} - \frac{rr}{4}$ is leſs than $\frac{rr}{4}$, or $\frac{q^3}{27}$ is leſs than $\frac{2rr}{4}$, or $\frac{rr}{4}$ is greater than

half

half $\frac{q^3}{27}$, or than $\frac{q^3}{54}$, though less than $\frac{q^3}{27}$. And the nearer $\frac{rr}{4}$ approaches to $\frac{q^3}{27}$, the greater will be the swiftness with which this series will converge.

37. I will now add a few examples of the resolution of cubick equations of the aforesaid form, $x^3-qx=r$, in the second case of those equations, in which r is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, by means of the infinite series $e^{\frac{1}{3}} \times \overline{2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -}$ &c. found in Art. 35. in order to confirm the truth of the reasonings by which that series was obtained.

EXAMPLE I.

38. *Let it be required to resolve the equation* $x^3-50x=120$ *by means of the said infinite series.*

Here q is $=50$; r is $=120$; $\frac{r}{2}$ or e, is $=60$; $\frac{rr}{4}$, or ee, is $=3600$; q^3 is $=125,000$; and $\frac{q^3}{27}$ is $=\frac{125,000}{27}=$ 4629.629,629,629, &c. which is greater than 3600, or $\frac{rr}{4}$. Therefore this equation cannot be resolved by CARDAN's rule, but may by the expression $e^{\frac{1}{3}} \times$ the series $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. provided that series converges. Now, since $\frac{q^3}{27}$ is $=4629.629,629,629$, &c.

 and

and $\frac{rr}{4}$ is $= 3600$, we ſhall have $ss = \frac{q^3}{27} - \frac{rr}{4} = 4629.629$, 629, 629, &c. $- 3600 = 1029.629, 629, 629$, &c. which is conſiderably leſs than 3600, or ee; and conſequently the ſeries will converge.

39. We ſhall therefore have $\frac{ss}{ee} = \frac{1029.629,629,629}{3600}$ &c. $=.286,00$; and $\frac{s^4}{e^4} = .081,796$; and $\frac{s^6}{e^6} = .023,393$; and conſequently $\frac{2ss}{9ee} = \frac{2 \times .286}{9} = \frac{.572}{9} = .063,55$; and $\frac{20s^4}{243e^4} = \frac{20 \times .081,796}{243} = \frac{1.635,92}{243} = .006,73$; and $\frac{308s^6}{6561e^6} = \frac{308 \times .023,393}{6561} = \frac{7.205,944}{6561} = .001,098$. Therefore $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6}$ is $= 2 + .063,55 - .006,73 + .001,09 = 2.064,64 - .006,73 = 2.057,91$. And $e^{\frac{1}{3}}$, or $\sqrt[3]{e}$, is $= \sqrt[3]{60} = 3.914,867$. Therefore $e^{\frac{1}{3}} \times$ the ſeries $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. is $= 3.914,867 \times 2.057,91 = 8.0564$; that is, the root of the propoſed equation $x^3 - 50x = 120$ is 8.0564; which is true in three places of figures, the error being in the fourth place of figures, or third place of decimal fractions, where the figure ought to be a 5 inſtead of a 6, the more accurate value of x in that equation being 8.055,810,345,702, as may eaſily be found by Mr. RAPHSON's method of approximation. But 8.0564, the value of x found by the foregoing proceſs, is ſufficiently near to its more accurate value 8.055,810, &c.

to shew the truth of the foregoing reasonings. Their difference is only $\frac{6}{10,000}$ parts of an unit, which is only the 13426th part of 8.055,810, &c. or the true value of x.

40. N. B. This equation $x^3-50x=120$ expresses the relation between the diameter of a circle and three chords in it that lie contiguous to each other, and together take up a semicircle, and form a trapezium of which the diameter of the circle is the fourth side. For if the three chords are called b, k and t, and the diameter of the circle is called x, the relation between them will be expressed by the cubick equation $x^3 \left.\begin{matrix}-bb\\-kk\\-tt\end{matrix}\right\} \times x = 2bkt$, which, if the numbers 3, 4 and 5 are substituted instead of the letters b, k, and t, will become $x^3-50x=120$. See Sir ISAAC NEWTON'S Arithmetica Universalis, Edit. 2d. 1722, page 101.

EXAMPLE II.

41. *Let it be required to find by means of the same series the root of the equation* $x^3-x=\frac{1}{3}$.

Now in this equation q is $=1$, r is $=\frac{1}{3}$, $\frac{r}{2}$ is $=\frac{1}{6}$, $\frac{rr}{4}$ or ee, is $=\frac{1}{36}$, and $\frac{q^3}{27}$ is $=\frac{1}{27}$, which is greater than $\frac{1}{36}$

 or

or $\frac{rr}{4}$. Therefore this equation cannot be resolved by CARDAN'S rule, but may by the series $e^{\frac{1}{3}} \times \overline{2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} - \frac{308s^6}{6561e^6} - \&c.}$ in case that series is a converging one.

Now, since $\frac{q^3}{27}$ is $= \frac{1}{27}$, and $\frac{rr}{4}$ is $= \frac{1}{36}$, we shall have ss, or $\frac{q^3}{27} - \frac{rr}{4}$, $= \frac{1}{27} - \frac{1}{36} = \frac{36-27}{27 \times 36} = \frac{9}{27 \times 36} = \frac{1}{3 \times 36}$, which is less than $\frac{1}{36}$, or ee, in the proportion of 1 to 3. Consequently the series $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.$ and the series $e^{\frac{1}{3}} \times \overline{2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.}$ will converge. Therefore the equation $x^3 - x = \frac{1}{3}$ may be resolved by the means of it as follows.

42. Since ss is $= \frac{1}{3 \times 36}$, and $\frac{rr}{4}$, or ee, is $= \frac{1}{36}$, we shall have $\frac{ss}{ee} = \frac{1}{3} = .333,333$, and $\frac{s^4}{e^4} = \frac{1}{9} = .111,111$, and $\frac{s^6}{e^6} = \frac{1}{27} = .037,037$, and consequently $\frac{2ss}{9ee} = \frac{2 \times .333333}{9} = \frac{.666666}{9} = .074,074$, and $\frac{20s^4}{243e^4} = \frac{20 \times .111,111}{243} = \frac{2.222,222}{243} = .009,144$, and $\frac{308s^6}{6561e^6} = \frac{308 \times .037,037}{6561} = \frac{11.407,396}{6561} = .001,738$. Therefore $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.$ is $= 2 + .074,074 - .009,144 + .001,738 = 2.075,812 - .009,144 = 2.066,668$. And $\sqrt[3]{e}$ is $= \sqrt[3]{\frac{1}{6}} = \frac{1}{\sqrt[3]{6}} = \frac{1}{1.817,121}$. Therefore $e^{\frac{1}{3}} \times$ the series $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.$ is $= \frac{1}{1.817,121} \times 2.066,668 =$

1.13733;

1.13733; that is, the root of the propofed equation $x^3-x=\frac{1}{3}$ is 1.137,33; which is true to four places of figures, the error being in the fifth place of figures, or the fourth place of decimal fractions, where the figure ought to be an unit inftead of a 3, the more accurate value of x being 1.137,158,164, which differs from the value of it here found by lefs than .00017, or $\frac{17}{100,000^{th}}$ parts of an unit, which is lefs than the 6689th part of 1.137,158,164, or the true value of x.

EXAMPLE III.

43. *Let it be required to find the root of the equation*

$$x^3 - 5x = 4.$$

Here q is $= 5$; r is $= 4$; $\frac{r}{2}$, or e, is $= 2$; $\frac{rr}{4}$, or ee, is $= 4$; q^3 is $= 125$, and $\frac{q^3}{27}$ is $= \frac{125}{27} = 4.629,629,629$, &c. which is greater than 4, or $\frac{rr}{4}$. Therefore this equation cannot be refolved by CARDAN's rule, but may by the infinite feries $e^{\frac{1}{3}} \times \overline{2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.}$ in cafe that feries is a converging one.

Now, fince $\frac{q^3}{27}$ is 4.629,629,629, &c. and $\frac{rr}{4}$ is $= 4$, we fhall have $\frac{q^3}{27} - \frac{rr}{4}$, or ss, $= .629,629,629$, &c. which

is

is leſs than 4, or ee, in the proportion of about 6 to 40, which is a pretty large proportion of minority, and much larger than the proportion of ss to ee in either of the former examples. Conſequently the ſeries $e^{\frac{1}{3}} \times \overline{2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.}$ will converge with a greater degree of ſwiftneſs than in either of thoſe examples. Therefore the equation $x^3 - 5x = r$ may be reſolved by it as follows.

44. Here $\frac{ss}{ee}$ is $= \frac{.629,629, \&c.}{4} = .157,407$; and conſequently $\frac{s^4}{e^4}$ is $= .024,777$, and $\frac{s^6}{e^6}$ is $= .003,900$. Therefore $\frac{2ss}{9ee}$ is $= \frac{2 \times .157,407}{9} = \frac{.314,814}{9} = .034,979$, and $\frac{20s^4}{243e^4}$ is $= \frac{20 \times .024,777}{243} = \frac{.495,540}{243} = 002,039$, and $\frac{308s^6}{6561e^6}$ is $= \frac{308 \times .003,900}{6561} = \frac{1.201,200}{6561} = .000,182$, and conſequently $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6}$ is $= 2 + .034,979 - .002,039 + .000,182 = 2.035,161 - .002,039 = 2.033,122$. And $e^{\frac{1}{3}}$, or $\sqrt[3]{e}$, is $= \sqrt[3]{2} = 1.259,921$. Therefore $e^{\frac{1}{3}} \times$ the ſeries $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.$ is $= 1.259,921 \times 2.033,122 = 2.561,573$; that is, the root of the propoſed equation $x^3 - 5x = 4$ is 2.561,573; which is true to five places of figures, the error being in the ſixth place of figures, or the fifth place of decimal fractions, where the figure ought to

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be a 5 inftead of a 7. For the accurate value of x in this equation is $\frac{1+\sqrt{17}}{2}$, or $\frac{1+4.123,106}{2}$, or $\frac{5.123,106}{2}$, or 2.561,553; which differs from 2.561,573, or the value of x found by the foregoing feries, by only $\frac{20}{1000,000\text{th}}$, or $\frac{2}{100,000\text{th}}$, parts of an unit, or lefs than the 128,000th part of 2.561,553, or the value of x itfelf; which is a great degree of exactnefs.

45. *Note*. That x, or the root of the equation $x^3-5x=r$, is accurately equal to $\frac{1+\sqrt{17}}{2}$, will appear by fubftituting $\frac{1+\sqrt{17}}{2}$ inftead of x in the compound quantity x^3-5x, and obferving that it will make that quantity become equal to 4. For, if x is $=\frac{1+\sqrt{17}}{2}$, we fhall have $x^3=\frac{1+3\times\sqrt{17}+3\times17+17\times\sqrt{17}}{8}=\frac{52+20\times\sqrt{17}}{8}=\frac{13+5\sqrt{17}}{2}$, and $5x=\frac{5+5\sqrt{17}}{2}$, and confequently $x^3-5x=\frac{13+5\sqrt{17}}{2}-\frac{5-5\sqrt{17}}{2}=\frac{8}{2}=4$. Therefore $\frac{1+\sqrt{17}}{2}$ is $=x$. Q. E. D.

46. Thefe examples fufficiently prove that the expreffion $e^{\frac{1}{3}}\times$ the feries $2+\frac{2ss}{9ee}-\frac{20s^4}{243e^4}+\frac{308s^6}{6561e^6}-$ &c. (which we derived from the other feries $e^{\frac{1}{3}}\times\overline{\left|2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}-\text{\&c.}\right.}$ by the peculiar train of reafoning ufed in Art. 33, 34, and 35,) gives the true root of the cubick equation x^3-

$qx=r$

$qx = r$ in the second case of it, in which r is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, and which therefore cannot be resolved by CARDAN's rule.

I will, however, subjoin one more example to the same purpose; which shall be that of the equation $x^3 - 63x = 162$, which both Dr. WALLIS and Mr. DE MOIVRE have resolved by extracting what they call the impossible cube-roots of the impossible binomial quantities $81 + \sqrt{-2700}$ and $81 - \sqrt{-2700}$. Now this equation may be resolved by the foregoing expression $e^{\frac{1}{3}} \times$ the series $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. in the manner following.

EXAMPLE 4.

47. *Let it be required to find the root of the equation*

$$x^3 - 63x = 162.$$

Here q is $= 63$; r is $= 162$; $\frac{r}{2}$, or e, is $= 81$; $\frac{rr}{4}$, or ee, is $= 6561$; $\frac{q}{3}$ is $= 21$; and $\frac{q^3}{27}$ is $= 9261$, which is greater than 6561, or $\frac{rr}{4}$. Therefore this equation cannot be resolved by CARDAN's rule, but may by the infinite series $e^{\frac{1}{3}} \times \overline{2 + \frac{2ss}{9ee} - \frac{24s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.}$ in case that series is a converging one.

Now,

Now, since $\frac{q^3}{27}$ is $=9261$, and $\frac{rr}{4}$ is $=6561$, we shall have $\frac{q^3}{27}-\frac{rr}{4}$, or ss, $=2700$, which is less than 6561, or ee, in the proportion of 100 to 243. Consequently the series $2+\frac{2ss}{9ee}-\frac{20s^4}{243e^4}+\frac{308s^6}{6461e^6}-$ &c. and the product of that series multiplied into $e^{\frac{1}{3}}$, or the series $e^{\frac{1}{3}}\times \overline{2+\frac{2ss}{9ee}-\frac{20s^4}{243e^4}+\frac{308s^6}{6561e^6}-\text{\&c.}}$ will converge. Therefore the equation $x^3-63x=162$ may be resolved by it as follows.

48. Since ss is $=2700$, and $\frac{rr}{4}$, or ee, is $=6561$, we shall have $\frac{ss}{ee}=\frac{2700}{6561}=\frac{100}{243}=.411,522$, and $\frac{s^4}{e^4}=.169,350$, and $\frac{s^6}{e^6}=.069,691$, and consequently $\frac{2ss}{9ee}=\frac{2\times .411,522}{9}=\frac{.823,044}{9}=.091,449$, and $\frac{20s^4}{243e^4}=\frac{20\times .169,350}{243}=\frac{3.387,0}{243}=.013,938$, and $\frac{308s^6}{6561e^6}=\frac{308\times .069,691}{6561}=\frac{21.464,828}{6561}=.003,271$. Therefore $2+\frac{2ss}{9ee}-\frac{20s^4}{243e^4}+\frac{308s^6}{6561e^6}-$ &c. is $=2+.091,449,-.013,938,+.003,271-$ &c. $=2.094,720,-.013,938-$ &c. $=2.080,782,-$ &c. And $e^{\frac{1}{3}}$, or $\sqrt[3]{e}$, is $=\sqrt[3]{81}=4.326,749$. Therefore $e^{\frac{1}{3}}\times$ the series $2+\frac{2ss}{9ee}-\frac{20s^4}{243e^4}+\frac{308s^6}{6561e^6}-$ &c. is $=4.326,749\times 2.080,782,-$ &c. $=9.003,021-$ &c; that is, the root of the proposed equation $x^3-63x=162$ is $=9.003,021,-$ &c. or somewhat less than $9.003,021$; which is true to three places

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of figures, the error being in the fourth place of figures, or the third place of decimal fractions, where there ought to be a cypher inſtead of a 3, becauſe the accurate value of x in this equation is 9, as will appear upon trial: for, if x be taken $=9$, we ſhall have $x^3=729$, and $63x=567$, and conſequently x^3-63x $(=729-567)=162$.

SCHOLIUM.

49. This reſolution of the equation $x^3-63x=162$ anſwers to Dr. WALLIS's reſolution of it by extracting the cube-roots of the impoſſible binomial quantities $81+\sqrt{-2700}$ and $81-\sqrt{-2700}$, inaſmuch as both reſolutions are originally derived from CARDAN's rule. But the difference between them is, that the method here delivered is intelligible in every ſtep of it, whereas Dr. WALLIS's method treats of impoſſible quantities, or quantities of which no clear idea can be formed, in the whole courſe of the proceſs, though it concludes with a reſult that is intelligible, by means of the equality of the impoſſible members of the two ultimate quantities $\frac{9}{2}+\frac{1}{2}\sqrt{-3}$ and $\frac{9}{2}-\frac{1}{2}\sqrt{-3}$ (whoſe ſum is equal to the value of x), and the contrariety of the ſigns + and −, which are prefixed to them. The doctor's method of finding $\frac{9}{2}+\frac{1}{2}\sqrt{-3}$ and $\frac{9}{2}-\frac{1}{2}\sqrt{-3}$ to be the cube-roots

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of the impoffible binomial quantities $81 + \sqrt{-2700}$ and $81 - \sqrt{-2700}$ is only tentative. But Mr. DE MOIVRE has given a *certain* method of finding the cube-roots of fuch quantities in all cafes; but not without the trifection of an angle, or finding (by the help of a table of fines, or otherwife) the cofine of the third part of a circular arc whofe cofine is given; by means of which trifection it is well known (independently of CARDAN's rule, or Mr. DE MOIVRE's procefs) that the fecond cafe of the cubick equation $x^3 - qx = r$ (in which $\frac{rr}{4}$ is lefs than $\frac{q^3}{27}$), may be refolved. So that Mr. DE MOIVRE's method of doing this bufinefs, though more perfect than Dr. WALLIS's, does not feem to be of much ufe in the refolution of thefe equations. And both methods are equally liable to the objection above-mentioned, of exhibiting to our eyes, during the whole courfe of the proceffes, a parcel of algebraick quantities, of which our underftandings cannot form any idea; though, by means of the ultimate exclufion of thofe quantities, the refults become intelligible and true. It is by the introduction of fuch needlefs difficulties and myfteries into algebra (which, for the moft part, take their rife from the fuppofition of the exiftence of negative quantities,

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or quantities less than nothing, or of the possibility of subtracting a greater quantity from a lesser), that the otherwise clear and elegant science of algebra has been clouded and obscured, and rendered disgusting to numbers of men of a just taste for reasoning; who are apt to complain of it, and despise it, on that account. And, doubtless, they have too much reason to do so, and to say, in the words of the famous *Monsieur* DES CARTES in his dissertation *De Methodo*, page 11, *Algebram verò, ut solet doceri, animadverti certis regulis et numerandi formulis ita esse contentam, ut videatur potiùs ars quædam confusa, cujus usu ingenium quodammodò turbatur et obscuratur, quam scientia, quâ excolatur et perspicacius reddatur.* If this complaint was just in DES CARTES's time, there is certainly much more reason for it now.

50. The passage above alluded to in Dr. WALLIS's algebra, is in the 48th chapter, pages 179, 180, of the folio edition at London in 1685. And Mr. DE MOIVRE's method of extracting the cube-root of an impossible binomial quantity, as $81+\sqrt{-2700}$, or $a+\sqrt{-b}$, is published in the appendix to the second volume of professor SAUNDERSON's algebra, pages 744, 745, 746, 747. It is very ingenious, and shews that author's great skill in the use and management of algebraick

gebraick quantities. See alſo on this ſubject CLAIRAUT'S Elémens d'Algébre, Part V. Section 9. pages 286, 287, 288; and a paper of Monſieur NICOLE in the memoirs of the French Academy of Sciences for the year 1738, pages 99 and 100. See alſo MACLAURIN'S algebra, Part I. the ſupplement to the 14th Chapter, pages 127, 128, 129, 130; and the Philoſophical Tranſactions, N°. 451.

51. If any gentleman ſhould be inclined to compute the ſeries $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. to more than four terms, he will find the firſt eight terms of it to be as follows, to wit, $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \frac{1870s^8}{59049e^8} + \frac{111,826s^{10}}{4,782,969e^{10}} - \frac{2,358,512s^{12}}{129,140,163e^{12}} + \frac{120,646,960s^{14}}{8,135,830,269e^{14}}$.

XLIII. *Account of the Advantages of a newly-invented Machine much varied in its Effects, and very useful for determining the perfect Proportion between different Moveables acting by Levers and Wheel and Pinion. By Mr.* Le Cerf, *Watch-maker at* Geneva; *Communicated by Lord Viscount* Mahon, *F. R. S.*

Read July 9, 1779.

TO SAMUEL HORSLEY, LL.D. S. R. S.

S I R,

Harley Street,
March 19, 1778.

HAVING received from Mr. LE CERF, watch-maker at Geneva, two instruments of his invention, with the annexed paper; and being desired by him to present them to the Royal Society, I take the liberty of sending them to you, as secretary of that learned Body. I have perused this interesting treatise, which contains some new and valuable thoughts upon the relative size of wheels and pinions working together; and this being one of the things of the most general use, with respect to the improvement and easy execution of clocks, watches,

watches, and all kind of wheel machines, this paper appears to me to be very worthy of the attention of the Society. It is to be regretted, that the author does not enter into a greater and more minute detail upon the shape of the working-teeth, &c. as this, in my opinion, will, in several cases, materially affect the very simple, general rules which he has elegantly laid down.

I am, SIR, with true regard,

Your very obedient, humble servant,

MAHON.

§ I. THIS machine may be called a compass of geometrical and mechanical proportion; its property is to resolve a great number of problems analogous to the theory and practice of watch-making, in a manner which is at the same time very evident and infinitely more

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Description d'une Machine de nouvelle Invention, aussi variée dans ses Effets que nécessaire pour déterminer les parfaits rapports entre les differens mobiles agissans par Leviers et par Engrenages. Par Monsieur Le Cerf, *Horloger à* Geneve.

§ I. ON peut appeller cette machine, compas de proportion géométrique et méchanique, puis qu'il a la proprieté de resoudre une quantité de problémes analogues à la theorie et à la pratique de l'horlogerie d'une maniére très

more convenient than the mode of arithmetical calculations.

§ 2. The compaſs of proportion here treated of not only ſhews the proportion of the diameters between the wheels and the pinions, but ſerves to determine every ſpecies of proportion of the calibers, of the ſize of pivots, width of pallets, bigneſs of cylinders, and in general of whatever is an object of dimenſion. In every caſe it affords a product, either in unity or fractions, as perfect as the application of it to the mechaniſm we are conſidering is eaſy.

§ 3. Nothing therefore can more conduce to the perfection of watch-making than an inſtrument which immediately determines all the dimenſions and proportions required; dimenſions and proportions which could not heretofore be obtained but by means ſo long, ſo laborious,

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très évidente et infiniment plus commode que par les opérations du calcul.

§ 2. Ce compas de proportion dont ce mémoire fait l'objet, décide non ſeulement du rapport des diamêtres entre les rouës et les pignons, mais encore il ſert à déterminer toutes ſortes de proportions, ſoit des calibres, groſſeurs des pivots, largeurs des palettes, groſſeurs des cilindres, et généralement de tout ce qui fait objet de dimenſion; il donne dans tous les cas ſoit par unités ou par fractions un reſultat auſſi parfait que facile à appliquer au mechaniſme en queſtion.

§ 3. Rien ne nous paroit donc plus intéreſſant pour la perfection de l'horlogerie qu'un ſemblable inſtrument qui détermine ſur le champ toutes les dimenſions et proportions requiſes que l'on ne pouvoit juſqu'à préſent obtenir que par des

rious, and so imperfect, that it is they which have probably given rise to a general relaxation with regard to the true principles, and to the adoption of rules and measures arbitrary, vague, and which produce very considerable errors: such, for instance, is the error of taking a little more than three points of a tooth in order to determine the size of the pinions of six, without regard to the revolutions which the different numbers of the teeth of the wheels produce. The consequence is, that an equal measure being taken upon a wheel of 18 and upon one of 72 teeth produces a variation of one entire revolution, as will be shewn hereafter.

§ 4. For instance, the pinion is the divisor of the wheel as well as that of the circle, each tooth of which is to raise its required proportion of degrees. A pinion of six is to raise 60° *per* tooth because $6 \times 60 = 360$; one of

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des moyens aussi longs que pénibles et imparfaits, et qui vraisemblablement ont donné lieu à un relachement général sur les vrais principes, en suivant des régles ou mesures aussi vagues qu'arbitraires et qui mênent à des erreurs très considérables; telles sont celles de prendre un peu plus que trois pointes de dent pour déterminer la grosseur des pignons de six sans distinction des revolutions que produisent les différens nombres de dents des rouës. Par conséquent la même mésure prise sur une rouë de 18 comme sur celle de 72 dents, produit une écart d'une revolution entiére, ainsi que l'on l'expliquera ci après.

§ 4. Par exemple, le pignon est le diviseur de la rouë aussi bien que celui du cercle, dont chaque aile doit lever sa portion de dégrés requise; un pignon de 6 doit

of ſeven is to raiſe 51° 25′$\frac{2}{3}$ and ſome ſeconds; one of eight is to raiſe 45°; one of ten is to raiſe 36°: finally, one of twelve muſt inconteſtably raiſe 30° *per* tooth, ſince 12 × 30 = 360.

§ 5. Amidſt various methods that have been uſed for the ſolution of this important problem, I ſhall mention only the moſt ſimple one which may be underſtood by thoſe who have the very firſt principles of arithmetic.

§ 6. I ſet in motion two wheels, each of twelve teeth. By making one wheel turn the other, it is evident that theſe two diameters muſt be perfectly equal between themſelves, allowing for the neceſſary ſhake between the teeth.[a]

(a) This ſhake is very inconſiderable, eſpecially when the pinion and the teeth of the wheel are properly opened; for, according to my experiments, the deduction to be made on this account is reduced to the 96th part of the circumference of a pinion

6 doit lever 60° par aile, parce que 6 × 60 = 360; un de 7 doit lever 51° 25$\frac{2}{3}$′ et quelques ſecondes; un de 8, 45°; celui de 10, 36°; enfin un pignon de 12, doit inconteſtablement lever 30° par aile, puiſque 12 × 30 = 360.

§ 5. *Entre diverſes régles* que l'on a employées pour reſoudre cet important problême; on ſera mention ſeulement dela plus ſimple et qui ſe trouvera à la portée de ceux qui auront les premieres notions des elemens de l'arithmétique.

§ 6. Si je fais fonctionner par engrenage deux mobiles du même nombre, que je ſuppoſe de 12 dents chacun, il eſt clair que ces deux diamètres doivent être parfaitement égaux entr'eux (au lochement près qu' exige tout engrenage libre *).

* Lochement qui ſe reduit à très peu de choſe, ſur tout quand le pignons et la denture ſont vuidés à leur point, car ſuivant l'expérience que j'en ai faite, cette conſidération ſe reduit à toute rigeur à un 96e dela circonférence d'un pignon de 12, qui produit la ſomme d'une 8e de dent

§ 7. It will follow from hence, that theſe two movers, which we will ſuppoſe wheel and pinion, muſt reciprocally raiſe the 30° required; but if you increaſe one of theſe ſo as to make a revolution more than the other, which is fixed at 12 teeth, and at one line diameter, this laſt will indeed have 24 teeth; but inſtead of two lines of diameter it will have only 23 and $\frac{1}{12}$th relatively to that of the pinion; or if we give it exactly the double diameter, it will have 25 teeth inſtead of 24; conſequently upon three revolutions, or 36 teeth, one muſt ſubtract from the wheel 2 and $\frac{7}{12}$th of a line, and ſo on as far as 12 or rather 11 effective revolutions (for the firſt, being ſuppoſed to be in equilibrio with its pinion, ought

pinion of 12 which produces the ſum of the 8th part of a tooth upon the wheel let its number be what it will.

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§ 7. Il ſuit delà que ces deux mobiles que nous ſuppoſerons rouë et pignon doivent lever reciproquement les 30° requis; mais ſi l'on augmente l'un de ces mobiles d'une revolution de l'autre qui eſt fixée à 12 dents et une ligne de diamêtre, ce dernier aura bien 24 dents, mais au lieu de deux lignes de diamétre il n'aura que [illegible] relativement à celui du pignon, ou bien ſi on lui donne exactement le double en diamètre il aura 25 dents au lieu de 24, parconſéquent ſur trois révolutions ſoit 36 dents, il en faudra retrancher de la rouë $\frac{1}{8}$ de ligne, ainſi de ſuite juſqu'à 12 ou bien 11 révolutions effectives) parce que la premiére étant

dent ſur la rouë de tel nombre qu'elle ſoit; néantmoins pour rendre les juſtes rapports d'autant plus complets, j'ai fait entrer tout d'un tems cette petite compenſation en faveur du mobile qui mêne dans les dimenſions de mon compas en queſtion, pour tous les nombres et grandeurs des rouës et pignons quelconques, &c.

cenſée

not to be reckoned). The wheel will then indeed have 144 teeth, or 12×12; but it will only have 11 times as much in diameter, that is, 11 lines instead of 12, in order that the angles of these two movers may always be in the same perfect proportion to one another, of which the precise raising of 30° *per* tooth is one of the most convincing proofs.

§ 8. In order to make this more intelligible, I say that, as the primitive radii ought to be equal between these movers, a pinion of six wings, being a line in diameter, requires the taking off of one sixth, to take from it what is useless in its catch; this is what forms the apparent diameter. This deduction will reduce the primitive radius of this pinion to $\frac{5}{6}$ths of a line; for as its revolutions about a wheel of 12 teeth are as two to one, what is to make the radius of the wheel for the first revolution will be equal

censée faire équilibre avec son pignon ne doit point entrer en ligne de compte) la rouë aura bien 144 dents ou 12×12. Mais elle n'aura qu'onze fois autant en diamêtre, c'est à dire onze lignes au lieu de douze, pour que les angles de ces deux mobiles restent toûjours dans le même et parfait rapport, dont la juste levée de 30° par aile est une des preuves la plus convaincante.

§ 8. Pour rendre ceci plus intelligible, nous disons que comme les rayons primitifs doivent être égaux entre ces mobiles; un pignon de six ailes qui aura une ligne de diamêtre exige le retranchement d'un sixiéme pour lui ôter ce qu'il y a d'inutile dans son engrenage, c'est ce qui forme le diamêtre apparent; ce retranchement reduira le rayon primitif de ce pignon à $\frac{10}{12}$ à soit à $\frac{5}{6}$ de ligne, car comme ses révolutions autour d'une rouë de 12 deuts sont comme 2 à 1, et ce qui

equal to this in $\frac{12}{12}$ths, or one line; but for 2, 3, and 4 revolutions, and ſo on, one muſt add $\frac{10}{12}$ths of a line to the radius already laid down, for as many revolutions as the pinion muſt make more than the wheel, a wheel of 12 teeth will conſequently have $\frac{22}{12}$ths of a line radius or diameter; one of 18 teeth and $\frac{32}{12}$ths; one of 24 teeth $\frac{42}{12}$ths, and ſo on.

§ 9. The ſame thing holds true in all the other pinions, let the number of them be what it will, where the ſame rule is to be obſerved. Thus upon 10 revolutions of a pinion of 10 one muſt deduct from the diameter of the wheel one diameter of the pinion; as many upon eight revolutions of a pinion of eight; upon ſeven of a pinion of ſeven as many; upon ſix of a pinion of ſix

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qui doit faire le rayon dela rouë pour la premiére révolution ſera egal à celui ci en 12 douziémes ſoit une ligne mais pour 2, 3, et 4 révolutions ainſi de ſuite on ajoutera 10 douziémes de ligne au rayon déja poſé, pour autant de révolution que devra faire le pignon de plus que la rouë; ainſi une rouë de 12 dents aura de rayon ou de diamêtre 22 douziémes de ligne, celle de 18 dents 32 douziémes; de 24, 42 douziémes, &c.

§ 9. Il en eſt demême de tous les autres pignons, de tel nombre que ce ſoit, où il faut obſerver la même régle, ainſi ſur 10 révolutions d'un pignon de 10 on retranchera du diamêtre dela rouë une fois celui du pignon; ſur 8 révolutions d'un pignon de 8 autant; ſur ſept d'un pignon de 7 autant; ſur ſix d'un pignon de

fix as many; and so on upon all the movers acting by revolutions and teeth.

§ 10. This constant variation of the diameters in the proportion of the revolutions of the pinion upon the wheel, is a discovery so much the more important towards determining, with greater ease, the due proportion between the wheels and the pinions, that it follows gradually, in the most perfect diminutive order, all the numbers and magnitudes of any diameter whatever; a singularity of effect which shall be accounted for in another place.

§ 11. The diameter of the wheel must of necessity increase in a ratio of the actual revolutions of the pinion, and not in that of its apparent diameter; since we are considering the working parts of wheels and pinions, where the angles, relatively to the change of the curves and

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de six autant; ainsi du reste de tous les mobiles agissans par révolutions et engrenages.

§ 10. Cette constante varieté des diamêtres, en raison des révolutions du pignon sur la rouë, est une découverte d'autant plus importante pour déterminer avec la plus grande facilité et précision le juste rapport entre les rouë et les pignons, qu'elle suit graduellement dans l'ordre diminutif le plus parfait tous les nombres et grandeurs de quelques diamêtres que ce soit; singularité d'effets dont nous rendrons compte ailleurs.

§ 11. Le diamêtre de la rouë doit nécessairement augmenter en raison des révolutions effectives du pignon et non en raison de son diamêtre apparent puis qu'il

and the circumference, muſt be reciprocally in the ſame proportion to operate conſtantly the degrees of raiſing which are required.

§ 12. One may eaſily conceive, that the teeth of a wheel become conſtantly more parallel to each other, and approach to the ſtraight line, in proportion as the number of them increaſes, the depth in that caſe need not be ſo great, and the curve being ſhorter is more favourable to the uniformity of the frictions than upon wheels which are few in number; this is what moſt commonly happens to pinions of ſix, the numbers of whoſe wheels hardly ever exceed 60 or 72 teeth. For if in the uſual method of uſing a pinion-gage one was to take upon a wheel of 12 or 18 teeth a little more than the three points (which points from the nature of their angles would

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qu'il s'agit d'un engrenage où les angles relativement au changement des courbes et de la circonférence doivent être réciproquement dans le même rapport pour opérer conſtamment les degrés de levées requis.

§ 12. Il eſt facile de comprendre que les dents d'une roue deviennent toûjours plus paralelles entr'elles et s'approchent de la ligne droite a meſure que le nombre en eſt augmenté, et moins alors l'engrenage exige de pénétration, la courbe étant plus courte devient dans ce cas plus avantageuſe en faveur de l'uniformité des frottemens, que ſur des roues peu nombreuſes, c'eſt ce qui d'ordinaire arrive aux pignons de ſix dont les nombres des roues ne paſſent guéres 60 et 72 dents, &c. Car ſi par la méthode ordinaire avec un calibre à pignon on prenoit ſur une roue de 12 dents où de 18 un peu plus que les trois pointes (que par la nature de leurs

would hitch into the three teeth at one and the ſame time) ſuppoſed to be in due proportion with a pinion of ſix, and that afterwards one was to make uſe of the ſame meaſure upon a wheel of 42 teeth, the natural conſequence would be, that this pinion would be by $\frac{3}{6}$ths, or half its diameter, too big.

§ 13. A pinion of ſeven, ſuppoſed to be of a proper bigneſs, with a wheel of 21 teeth, would according to the ſame meaſure of three full teeth taken upon a wheel of 70, be too big by its whole diameter, inſomuch, that the wheel inſtead of 70 teeth ought to have but 63, &c. Though the ſuperior angles, that is, the circumference of the wheels from 12 to 120, and above *ad infinitum*, relatively to the ſize of a pinion given according to our rule, are and ought to be invariably the ſame; the lower angles,

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leurs angles embraſſeroient tout d'un tems les trois dents) que l'on ſuppoſe en juſte rapport avec un pignon de ſix, et que l'on vouluſſe faire uſage de la même meſure ſur une rouë de 42 dents, il en reſulteroit évidemment que ce pignon ſe trouveroit de $\frac{3}{6}$ ſoit dela moitié de ſon diamêtre trop gros.

§ 13. Un pignon de ſept qui ſeroit par ſuppoſition de juſte groſſeur avec une rouë de 21 dents ſe trouveroit, ſelon la même meſure de trois dents pleines, priſes ſur une rouë de 70 trop gros de ſon diamêtre entier, de ſorte que la rouë au lieu de 70 dents n'en devroit avoir que 63, &c. Non obſtant que les angles ſuperieurs, ſoit la circonférence des rouës depuis 12 à 120 et au deſſus juſques à l'infini; relativement à la groſſeur d'un pignon donné ſelon nôtre régle,

 ſoient

angles, that is, the thickneſs of the teeth, are yet extremely different in every wheel.

§ 14. For inſtance, all the wheels from 12 to 120 being ſloped and rounded with the ſame file, the teeth of the wheel of 48 are half as thick again as thoſe of 12, &c. In proportion as the paralleliſm of the angles increaſes, the teeth of courſe become larger and fuller, and each tooth conſequently always exceeds the intermediate ſpace in the ſame proportion, and gradually as far as the right line. This, in my opinion, determines the proper ſpace between the teeth of each wheel, whence follows the curve, equally conſtant and advantageous in every reſpect, both with regard to the execution and to the uniformity in its lead on a pinion ſuch as this, the thickneſs of the teeth of which is a ſixth part of its diameter,

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ſoient et doivent être invariablement les mêmes, les angles inférieurs ſoit l'epaiſſeur des dents, ſont cependant à chaque rouë très differens.

§ 14. Par exemple, toutes les rouës depuis 12 juſques à 120 étant eſlanquées et arrondies avec la même lune ou fraiſe, les dents de la rouë de 48 ſont la moitié plus groſſes ou plus épaiſſes que celle de 12, &c. à meſure que le paralleliſme des angles augmente, les dents deviennent naturellement plus groſſes ou plus étoffées (et le plein par conſéquent excèdant toûjours le vuide dans la même proportion et graduellement juſqu'à la ligne droite). C'eſt là qu' eſt à mon avis le vrai point de vuidage graduel de chaque rouë, d'où ſuit naturellement la courbe auſſi conſtante qu'elle me paroit la plus advantageuſe à tous égards, ſoit dans l'execution, ſoit dans l'uniformité, à peu de choſe près, de la menée ſur un pignon tel que celui ci, dont l'epaiſſeur des ailes eſt la ſixiéme partie de ſon diamêtre, tout comme l'excedant

meter, juſt as the exceſs of the primitive radius or true diameter is the ſixth part of its apparent diameter.

§ 15. According to this demonſtration, founded upon experiments, and which I am able to make out clearly, it is eaſy to ſee what errors muſt have crept into clock-making by the old method of taking for the pinion of ſix three or a little more than three points of teeth; for pinion of ſeven, the three full teeth when finiſhed; for pinion of eight, the three full teeth, and the void ſpace as far as the fourth; for pinion of ten, four full teeth ſo as the wheel comes out of the engine (the making of which has hitherto been truſted to women, children, and ſervants); finally, for a pinion of twelve, five points of rather ſtrong teeth, with the ſame arbitrary formality. Theſe methods, I muſt repeat it, are only fit to perpetuate miſun-

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l'excedant du rayon primitif ſoit diamêtre vrai, eſt la ſixiéme partie de ſon diamêtre apparent.

§ 15. Selon cette démonſtration fondée ſur des principes d'experience et dont on peut donner des preuves évidentes, il eſt aiſé de voir et de ſentir a quel extrême erreur ces uſages ont expoſé l'horlogerie juſques à préſent où l'on a ſuivi conſtamment l'ancienne méthode de prendre pour pignon de ſix, les uns un peu plus que les trois pointes de dents, les autres les trois pointes juſtes; pour pignon de 7 les trois dents pleines la denture finie; pour pignon de 8 les trois dents pleines et le vuide juſques à la quatrieme; pour pignon de 10 quatre dents pleines telles que la rouë ſort du fendage, lequel juſques à preſent a été confié aux femmes, aux enfans et même aux ſervantes, &c. Enfin pour pignon de 12, cinq pointes de dents un peu fortes avec la même formalité arbitraire, méthodes nous

miſunderſtanding and diviſion upon the manner of fixing and aſcertaining the firſt principles of watch-making.

§ 16. Nothing therefore ought to make us more ſenſible of the want there is of our compaſſes of proportion, than the conſideration of meaſures as ridiculous in themſelves as they are impoſſible to fix; there is no proportion which can be fixed (except it be by mere chance) between theſe movers not even within ſo much as eight or ten degrees of riſe *per* tooth; whereas by our machines, whoſe utility and the manner of making uſe of them is comprehended at firſt ſight, one may come within a quarter or even an eighth of a riſe *per* tooth, or nearly ſo.

§ 17.

le répétons tout à fait propres à perpetuer le ſchiſme, la més-intelligence et l'erreur ſur la maniére propre à fixer les vrais et premiers principes de l'horlogerie.

§ 16. Rien ne devroit donc mieux faire ſentir la néceſſité de nos compas de proportion que des méthodes de meſures auſſi ridicules par elles mêmes qu'impropres à fixer aucun juſte rapport entre les mobiles en queſtion, pas même à 8 et 10 degrés de levées près par aile, ſi non par l'effet du pûr hazard, tandis que l'on peut avec autant de précision que de facilité par nos ſuſdites machines aller juſqu'à $\frac{1}{4}$ et même $\frac{1}{8}$ ſoit $7\frac{1}{2}$ minute en degré de levée par aile près, et leur uſage et la maniére de s'en ſervir eſt au premier coup d'œil ſi aiſée à comprendre qu'il ſeroit fort inutile d'entrer dans un plus grand detail à ce ſujet.

 § 17.

§ 17. It is not ſo eaſy to adapt theſe principles to our machines for all ſorts of pinions, numbers of teeth, and ſizes of wheels, and generally to whatever acts by lever, or wheel and pinion, or whatever is ſuſceptible of relation, dimenſion, proportion, and compenſation, from the firſt to the laſt movers of a watch or clock; for the leaſt fault, either in the ſyſtems or geometrical principles, or the mechanical execution, is as fatal as would be the putting down a cypher too much or too little in the ſolution of a problem; and the more fatal the more complicated the machine.

§ 18.

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§ 17. Il n'en eſt pas de même ſur la maniére d'adapter les ſuſdits principes à nos machines pour toutes ſortes de pignons, de nombres et grandeurs de rouës, et généralement ſur ce qui agit par levier et engrenage, ainſi que ſur tout ce qui eſt ſuſceptible de rapport, dimenſion, proportion, et compenſation reſpectives depuis les premiers juſques aux derniers mobiles d'une montre et pendule; nous ſavons par expérience combien de difference il y a entre le maniement de la plume et du papier à celui des inſtrumens mechaniques dont la conſtruction, ſur tout pour de ſemblables objets, exige la plus haute combinaiſon dans ſes parties, auſſi bien que la plus grande precifion dans l'application des principes théoriques; car la plus légére faute, ſoit dans les ſyſtêmes ou principes géométriques, ſoit dans l'execution méchanique, eſt auſſi grave que ſi l'on poſoit un chiffre de plus ou de moins dans des ſolutions problêmatiques, à cette différence près, que la derniére ſera bien manquer le juſte reſultat, tout comme la premiére, mais que de plus celle ci induira en erreur, ſuivant la complication des objets, juſques à l'infini.

§ 18.

§ 18. The conſideration of theſe difficulties has reduced me hitherto to keep ſilence upon the general conſtruction of theſe inſtruments; inſtruments, in my opinion, abſolutely neceſſary for the conſtructing of movements, ſets of wheels, and motions, either rough or finiſhed, according to the juſt proportions.

§ 19. But I make bold to ſet down, as a fundamental propoſition, that the true ſize or relation between a wheel and its pinion is abſolutely fixed; and in no caſe whatſoever arbitrary, as thoſe who are not watch-makers might pretend: for all the reaſons which can be aſſigned in favour of this arbitrarineſs would be as vague and deſtitute of foundation as if one was to conſider arbitrarily the true gradation of a ſteel-yard relatively to its weight, or the length of a pendulum which ſwings ſeconds, whereas the

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§ 18. La conſidération de ces difficultés nous a reduits juſques à préſent à garder le ſilence ſur la conſtruction générale des ces machines devenues ſelon nous indiſpenſables pour diriger et conſtruire des mouvemens, rouages et cadiactures bruts ou finis ſelon les vraies proportions requiſes.

19. Nous ôſons donc établir pour principe fondamental, que la juſte groſſeur ſoit le parſait rapport entre une roue et ſon pignon eſt inconteſtablement fixe, abſolu, et non arbitraire dans aucun cas, ainſi que tous autres que des horlogers pourroient le prétendre; car toutes les raiſons que l'on pourroient alléguer en faveur de cet arbitraire, ſeroient auſſi vagues et deſtituées de fondement que ſi l'on enviſageoit arbitrairement la juſte graduation d'une Romaine ſoit Levrot relativement à ſon poid, ainſi que la longueur du pendul à ſeconde, tandis que ce dernier;

the latter is fixed by nature and the general laws of gravitation, and its vibrations are in the inverſe ratio of the ſquare roots of the lengths of the pendulums, which ſhould be neither more nor leſs than 36 inches 8 and $\frac{4}{7}$ths of lines from the point of its ſuſpenſion to the center of the lens, &c.

§ 20. I have already ſaid, that each pinion, of what number ſoever, ought to raiſe its preciſe portion of degrees, in order to operate in the moſt uniform manner poſſible its required revolutions; conſequently, each tooth of a pinion of ſix muſt raiſe the 60° which form the aperture of its angles.

§ 21. Let us ſuppoſe this pinion to have one line diameter, and the wheel which turns it to have 10 lines and 72 teeth; then if, inſtead of a line which is the due ſize

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dernier eſt fixé par la nature ſoit par la loi dela péſanteur, et que ſes vibrations s'exécutent dans la rapport invers des racines quarrées des longueurs du pendul, qui doit n'avoir ni plus ni moins que 36 pouces 8 et $\frac{4}{7}$es de lignes depuis le point de ſa ſuſpenſion juſques au centre de la lentille, &c.

§ 20. Nous avons déja dit que chaque pignon, de tel nombre que ce ſoit, doit lever ſa portion préciſe de dégrés pour opérer le plus uniformément poſſible ſes révolutions requiſes, par conſéquent chaque aile d'un pignon de 6 doit lever les 60° qui forment l'ouverture de ſes angles.

§ 21. Suppoſons ce pignon d'une ligne de diamêtre et la rouë qui forme ſon engrenage de 10 lignes et de 72 dents, alors ſi au lieu d'une ligne qui eſt la juſte

ſize of the pinion, we give it the twelfth of a line more (which, according to the rule of common meaſures, would be imperceptible, ſince there is often much more diſtance between the extremities of two oppoſite ſyſtems) it is evident, that upon every twelve revolutions there would be one wrong, conſequently ſuch a wheel of 12 revolutions would, according to the right rule, inſtead of 72 teeth require only 66.

§ 22. If then a pinion of ſix, inſtead of raiſing 60°, ſhould raiſe 65°, there will not only be 30° in every revolution of a pinion entirely loſt with reſpect to the expence of the moving force, but the extraneous frictions go on in a continued progreſſion, *ad infinitum*, till a ſtoppage enſues, and this ſometimes after a long ſeries of irregularity from the very beginning; a thing not at all ſurprizing,

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juſte groſſeur de ce pignon, on lui donne une douziéme de ligne de plus (qui ſelon la régle des communes meſures ſeroit imperceptible puis-qu' entre les deux extremités de diverſes opinions et ſyſtêmes, il y a beaucoup plus d' écart encore) il eſt évident que ſur 12 révolutions il y en auroit une de mécompte, ſoit une fois de ſon diamêtre, par conſequent une telle rouë de 12 révolutions au lieu de 72 dents, n'en exigeroit par la bonne régle que 66.

§ 22. Si donc un pignon de 6 au lieu de lever les 60° en leve 65°, il y aura 30° par chaque révolution du pignon, non ſeulement en pure perte quand à la dépenſe de la force motrice, mais les frottemens étrangers ſoit arcboutemens s'augmentent toûjours par progreſſion à l'infini juſques à ce que l'arrêt s'en ſuive, et quelque fois après une longue ſuite d'irrégularités depuis ſa naiſſance; on ne s'en etonnrea

prizing, if one confiders the defectuofity of the principles upon which the greater part of thefe machines are conftructed.

§ 23. Let us reckon only 2° more of rife *per* tooth, we fhall find between the four pinions (that of the center of 10, and the three of 6) 8780° *per* hour, which in 30 hours form a fum of loft labour for one complete hour.

§ 24. Who does not fee that fuch an irregularity in the combination of a machine intended to meafure time muft be the more prejudicial, from the impoffibility there is of eftablifhing any fixed rule with refpect to a juft and indifpenfable relation between the movers and their reciprocal actions.

§ 25. A pinion which is too fmall in proportion of the diameter and divifion of the wheel will not be able to raife

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etonnrea pas, fi l'on confidére la défectuofité des principes fuivant lefquels la plûpart de ces machines d'horlogerie font conftruites.

§ 22. Que l'on compte feulement deux degrés de levée par aile de plus, on trouvera entre les 4 pignons (celui du centre de 10 et les 3 autres de 6) 8780° par heure, ce qui forme dans 30 heures une fomme de peines perdues, pour une heure complette.

§ 24. Qui ne fent qu'un tel écart dans la combinaifon d'une machine deftinée pour mefurer le temps, doit être d'autant plus préjudiciable que l'on ne fauroit jamais établir aucune régle fixe, en faveur d'un jufte et indifpenfable rapport, entre les mobiles et leurs actions reciproques.

§ 25. Un pignon trop petit en raifon du diamêtre et dela divifion de la roue ne

raise the required degrees, because the aperture of its angle does not answer to that of the teeth of the wheel: for instance, a pinion of 6, which is $\frac{7}{12}$ths of a line smaller than the true rule makes it, will raise only 55°, when the tooth of the wheel has carried the tooth of the pinion as far as its extremity on the aperture of its angle allows, it is forced to leave it before the tooth which follows is come to the same place from whence the other sat off, consequently it falls 5° which are wanting to its just rise; this is what occasions the precipitations of the balance, which are so disagreeable to the ear, that one is in much greater haste to correct them than to prevent the retardations which, no doubt, arise from the other extremity; that is being obliged to make a forced rise, or a greater one than the true proportion of the diameters requires,

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ne pourra lever les degrés requis, parce que l'ouverture de son angle ne repond point à celui des dents de la rouë; par exemple, un pignon de 6 de $\frac{7}{12}$e de ligne plus petit que la véritable régle le fixe, ne levera que 55°, la dent de la rouë ayant mené l'aile du pignon aussi loin que son extrêmité ou l'ouverture de son angle le permet, est obligé de l'abandonner avant que l'aile suivante arrive à la même place d'où l'autre est parti soit de la ligne des centres, il fait par conséquent une chûte de 5° qui manquent à sa juste levée, c'est ce qui cause les précipitations du balancier qui choquent si fort l'oreille. C'est pourquoi on est beaucoup plus empressé à les corriger qu'à prévenir les lenteurs, qui sans doute proviennent de l'autre extrêmité, parce qu'étant obligé de faire une levée forcée ou plus grande que le juste rapport des diamêtres exige, ils éprouvent un contrast ou

quires, they meet with an oppoſition occaſioning this ſlowneſs infinitely more prejudicial than the precipitations.

§ 26. The difference then which there is between a pinion that is too large, and one that is too ſmall, is, that the firſt will occaſion irregularities, but never a ſtoppage; whereas the latter will produce both the one and the other: conſequently, if one had no choice between theſe extremities of error, it would be better to declare in favour of the ſmalleſt.

§ 27. Might it not with juſtice (as an ingenious clock-maker obſerves) be objected to us, how uſeleſs a thing it is to determine the right proportion in the relation of the diameters, whilſt the ſame defect of proportion ſubſiſts in a much higher degree in the general inequality of

arcboutement qui occaſionne cette lenteur, infiniment plus préjudiciable que les précipitations.

§ 26. La différence donc qu'il y a entre un pignon trop petit ou trop gros eſt telle, que le premier cauſera bien des irrégularités mais jamais d'arrêts, au lieu que le dernier produira l'un et l'autre, par conſéquent ſi l'on n'avoit d'autres choix que ces deux extrêmités de vices, il vaudroit mieux ſe déclarer en faveur du plus petit.

§ 27. Ne feroit on pas fondé (comme le remarque un horloger de génie) à nous faire la légitime objection, de l'inutilité qu'il y a à déterminer la juſte proportion dans le rapport des diamêtres, tandis que le même vice de la diſproportion ſubſiſte à un plus haut point encore, dans l'irrégularité générale de la diviſion des

2 rouës,

of the diviſion of the wheels which in our day has been carried to the heighth? I anſwer, that to form to one's ſelf a right idea of the important conſequences of a true or falſe diviſion of the wheels, it will be neceſſary firſt to enter into rather a greater diſcuſſion.

§ 28. The true and juſt diviſion of wheels and pinions is, that all the teeth ſhould be at perfectly equal diſtances from each other, or at leaſt divided into ſo many perfectly equal parts, which is the ſame thing.

§ 29. Conſequently, a wheel of 60 teeth forms 60 angles of 6° of overture each; if then, for inſtance, theſe 60 angles are open at different degrees, that is to ſay, one half only at 4 or 5°, it is neceſſary that the other half ſhould be intermixed or opened at 7 or 8°; we muſt remember that each angle of the wheel, no matter what number

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rouës, qui aujourd'huy eſt portée à ſon comble; nous répondons que pour ſe former une juſte idée des conſéquences importantes de la vraie ou fauſſe diviſion des rouës il conviendroit au-paravant d'entrer dans un détail un peu plus conſidérable.

§ 28. La vraie et juſte diviſion des rouës et des pignons eſt que toutes les dents ou ailes doivent être à des diſtances parfaitement égales l'une de l'autre, ou bien diviſées en autant de parties parfaitement égales, ce qui eſt la même choſe.

§ 29. Une rouë de 60 dents, forme conſéquemment 60 angles de ſix degrés d'ouverture chacun, ſi donc par exemple ces 60 angles ſont ouverts à différens degrés, c'eſt à dire la moitié ſeulement à 4 et 5°, il faut néceſſairement que l'autre moitié ſoit entremêlée ou de ſuite ouverte à 7 or 8°; l'on doit ſe rappeller que

number upon a pinion of ſix, muſt operate in a mutual proportion the 60° of riſe by which the angle of the pinion is open; from whence it happens, that the angles of the wheel, open only at 5°, will raiſe only 60° above thoſe of the pinion; but, if they are open at 7°, the riſe will be of 70° inſtead of 60°.

§ 30. There are likewiſe a great number of defects which are increaſed two and three-fold by the uſual conſequences attending wheel-engines, the ſmall tooth of which is conſtantly expoſed to become ſhorter than the large ones, in proportion of its ſize. This happens in the following manner; the file, which is to round off, working on each ſide equally, does not take more from the ſide of the large tooth than it does from the ſmall; conſequently, the ſmall one is ſharp whilſt the large one ſtill

que chaque angle de la rouë n'importe dequel nombre ſur pignon de ſix doit opérer en mutuelle proportion les 60° de levées do n l'angle du pignon eſt ouvert; d'où ſuit que les angles de la rouë ſeulement ouverts à 5°, n'opereront que 50° ſur ceux du pignon, mais que s'ils ſont ouverts à 7° la levée ſera de 70° au lieu de 60°.

§ 30. *Je* ne parlerai pas d'un concours de vices qui ſe double et ſe triple par l'effet ordinaire des outils aux dentures, ou la petite dent eſt expoſée à devenir toûjours plus courte que les groſſes, en raiſon de ſa petiteſſe; cela arrive de cette maniere, la lime à arrondir mordant des deux côtés également; n'emporte par plus d'etoffe du côté de la groſſe que de la petite dent, par conſéquent il y a encore du plat ſur la groſſe que la petite eſt déja pointue, il faut néceſſairement

 raccourcir

ftill continues flat, one muft therefore fhorten the fmall tooth in proportion to the fize of the larger.

§ 31. When the teeth were ufed to be finifhed by hand, there was great care taken to keep all the teeth of the fame length, even when it was impoffible in irely to correct or take off their inequality: in confequence of this care, the wheel at leaft ufed to be always perfectly round; whereas when the tool is ufed, the wheels are unavoidably as ill-rounded as they are unequal in their divifions, fo that the teeth ought never to be finifhed with the tool, unlefs they are thoroughly equally cut.

§ 32. It is therefore eafy to conceive, that the pinion becomes fometimes too large, and fometimes too fmall, upon the fame unequal wheel, and that the angle of the tooth

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raccourcir la petite dent dans la même proportion qu'il y a plus de matiére à la groffe.

§ 31. Lors qu'on finiffoit les dentures à la main on s'attachoit foigneufement à tenir toutes les dents de la même longueur, même lors qu'on ne pouvoit corriger ou remedier éfficacement à leur inégalité; la rouë par cette precaution fe trouvoit du moins toûjours parfaitement ronde, au lieu qu'à l'outil les rouës deviennent inévitablement auffi mal rondes qu'elles font inégales dans leurs divifions; en forte que l'on ne devroit jamais finir les dentures à l'outil à moins qu'elles ne fuffent fendues parfaitement égales.

§ 32. Il eft donc aifé de comprendre que le pignon devient tantôt trop gros et tantôt trop petit fur la même rouë inégale et très fouvent à l'excès et que l'angle de

tooth of 60°, being more or leſs open, produces an error of 10 degrees on the true riſe of the pinion.

§ 33. Let the arbitrary general practice be what it will, certain it is, that neither the degree of depth of the wheel and pinion, nor the fulneſs, nor the ſize of the wheel, can eſtabliſh any principle analogous to the harmony and perfect agreement of the movers we are talking of, to wit, the true ſize of the pinion; even the moſt perfect geometrical curve, ſuppoſing it practicable, could not make the pinion raiſe a minute more or leſs in degrees, without a juſt proportion of their reciprocal diameters.

§ 34. It is therefore impoſſible to uſe too much attention in conſidering the relations and proportions of the different movers which act by levers and wheel and pinion, ſince

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de la denture de 60 ouvert d'une degré de plus ou de moins, produit un écart de 10° ſur la juſte levée du pignon.

§ 33. Quoi qu'il en ſoit du ſyſtême arbitraire et de la routine générale, il n'y a ni le fort ni le foible des engrenages, ni le plein ni le vuide, qui puiſſe établir aucun principe analogue à l'harmonie et au parfait accord des différens mobiles en queſtion, ſoit la juſte groſſeur d'un pignon, la courbe géometrique même la plus parfaite (quand elle feroit praticable) ne ſauroit non plus faire lever au pignon une ſeule minute en degrés de plus ou de moins ſans le juſte rapport de leurs diamêtres reciproques.

§ 34. On ne ſauroit donc aſſés prendre en conſidération la partie des rapports et des proportions des différens mobiles agiſſans par leviers et par engrenages; puis

ſince theſe are the moſt eſſential objects of clock-making.

§ 35. This rule ariſing from the center of the move-ables themſelves, for the which it is principally uſed, it is right to deduct one revolution of a pinion, in conſequence of the juſt balance or perfect equilibrio between them; whence it follows, that the retrenching a complete revo-lution of the pinion will take place only at the ſeventh, or at forty-two, which ſeventh too ſtrictly ſpeaking is only the ſixth, ſince the firſt revolution ought not in fact to be reckoned, being ſuppoſed to be in equilibrio with its aſſociate, the wheel of the ſame number and ſame dia-meter; for the pinion making ſeven revolutions for one of the wheel, one may ſay with reaſon, that one active revolution of the wheel will make ſeven paſſive ones of the pinion, and deducting that of the wheel, the effec-tual

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puis que ce ſont les objets les plus eſſentiels de l'horlogerie.

§ 35. Cette régle ſortant du centre des mobiles mêmes qui font le principal objet de ſon uſage, il eſt de l'ordre de déduire une révolution du pignon en raiſon de la juſte balance, ou du parfait equilibre entr'eux, d'où il ſuit que le retranche-ment d'une révolution complette du pignon n'aura lieu qu'à la ſeptiéme ſoit à 42 qui cependant à bien compter n'eſt que la 6ᵉ puis que la premiére révolution ne doit point entrer en ligne de compte, étant cenſée faire équilibre avec ſa con-ſorte la rouë du même nombre et du même diamêtre, car le pignon faiſant ſept révolutions pour une de la rouë, on peut dire avec raiſon qu'une révolution active de la rouë en opére ſept paſſives du pignon, et en déduiſant celle de la rouë, les révolutions

tual revolutions of the pinion remain at ſix inſtead of ſeven, or elſe we muſt conſider the revolutions of the wheel with thoſe of the pinion as one to ſeven, of which the juſt reſult is ſix.

§ 36. If then one wiſhes to know what the juſt deduction required amounts to in every caſe, one need only divide the apparent diameter of the pinion by the number of its teeth, and the quotient will be the juſt portion for each of its revolutions, which conſequently upon ſix, makes the ſum of the entire diameter of the pinion of ſix, as is proved by the annexed table.

§ 37. In caſe of neceſſity one might conſider the revolutions under two points of view; the firſt of them as active, and the other as paſſive.

The

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révolutions éffectives du pignon reſtent à 6 au lieu de 7, ou bien il faut conſidérer les révolutions de la rouë avec ceux du pignon comme un à ſept et dont le juſte reſultat eſt ſix.

§ 36. Si donc on veut ſavoir à quoi ſe monte dans tous les cas la juſte déduction requiſe, il ne s'agit que de diviſer le diamêtre apparent du pignon par le nombre de ſes ailes et le quotient ſera la juſte portion pour chacune de ſes révolutions, qui par conſéquent ſur ſix fait la ſomme du diamêtre entier dont le pignon de ſix ſe trouve revêtu, ainſi que le calcul de la table cy jointe en fait foy.

§ 37. Au beſoin on pourroit enviſager les révolutions ſous deux points de vuë; ſçavoir les premiéres comme paſſives et les ſecondes comme actives.

1°. Les

The former ſuch as regard the duration of the motion or quantity of the vibrations.

The others, whoſe object is ſolely the conſtant proportion of the diameters of the movements in agitation.

§ 38. In the firſt caſe one reckons for a revolution of the pinion as many times as it is contained in the number of the teeth of the wheel; this mode of reckoning it is well known is abſolutely neceſſary for obtaining the required concluſion.

§ 39. The ſame is not true with reſpect to the latter caſe, in which the revolutions of the pinion are only reckoned to fix the true proportions of their reſpective diameters.

§ 40. The harmony required between the two movers is the ſame as if they had contracted a perpetual alliance,

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1°. Les révolutions qui ont pour objet la durée du mouvement ou la quantité des vibrations.

2°. Celles qui ont uniquement pour objet les conſtants rapports des diamêtres entre les mobiles en queſtion.

§ 38. Dans le premier cas l'on compte pour revolution du pignon autant de fois qu'il eſt contenu dans le nombre des dents de la roué; cette maniére de compter eſt comme l'on ſçait indiſpenſable pour obtenir le reſultat qu'on demande.

§ 39. Mais il n'en eſt pas de même du dernier cas où les révolutions du pignon ne ſe comptent que pour fixer les juſtes rapports de leurs diamêtres reciproques.

§ 40. L'harmonie requiſe entre les deux mobiles en queſtion eſt telle que s'ils avoient contractés une alliance perpétuelle à ſe maintenir mutuellement dans le même

ance, to hold conftantly the fame proportion towards each other, juft as if they were always in perfect equilibrio.

§ 41. However it be that this centrifugal law feems to adapt itfelf to the ufe one defires to make of it, it is not the lefs fixed and permanent; nor can it ever occafion the leaft error, provided only that the wheel pays its periodical tribute of deduction in favour of the pinion; in that cafe it will be equally in the fame proportion at the end of one revolution and of a hundred thoufand.

§ 42. In a word, the whole myftery of this golden rule is comprifed within the true and the apparent diameter of the pinion; whence it follows, that the deduction pofitively required is exactly the excefs of the apparent diameter above the true one; a deduction which for a pinion

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même rapport (moyennant un certain retranchement) tout comme s'ils étoient toûjours dans leurs parfaits équilibres.

§ 41. Quoi que cette régle centrifuge paroiffe s'accommoder à l'ufage que l'on en veut faire, elle n'eft pas moins droite et permanente et ne peut jamais induire dans la moindre erreur, pourvû toutefois que la rouë paye fon tribut périodique (foit de déduction) en faveur du pignon, elle fe trouvera alors auffi bien dans le même rapport au bout de cent mille révolutions qu'elle l'étoit à la premiére.

§ 42. Enfin l'on faura que tout le miftére de cette régle d'or, confifte entre le diamêtre vraï et le diamêtre apparent du pignon, d'où il fuit que cette déduction abfolument requife eft pofitivement la portion dont le diamêtre apparent excède du

a pinion of six is a sixth part of its diameter; for one of seven, a seventh; for one of eight, an eighth; for ten, a tenth; for twelve, a twelfth, and so on of all the moveables serving for pinions. What is meant by a pinion in watch-making is that moveable which is set in action by another of a greater number of teeth, and communicates its motion successively to the last of them; hence all the pinions relatively to the shape of the teeth of the wheels and pinions require an addition of the true diameter, and that is what is properly called the apparent diameter.

§ 43. We must, however, except the rochet pinions, because the whole primitive radius nearly as far as the extremity of the tooth is run over by the point of the tooth, which is of a rochet form, as well as the tooth of the pinion. But whatever advantage these rochet

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du vrai, déduction qui pour pignon de six est la sixiéme partie de son diamêtre, pour pignon de 7 la 7ᵉ, pour 8 la 8ᵉ, pour 10 la 10ᵉ, pour 12 la 12ᵉ; ainsi du reste de tous les mobiles servant de pignon; l'on entend dans l'horlogerie par le mot de pignon, ce mobile qui est actionné par un autre plus nombreux, et qui communique successivement le mouvement, jusqu'au dernier des mobiles; ce qui fait que tous les pignons, relativement à la nature des engrenages, exigent un excédent du diamêtre vrai, que l'on peut nommer à juste titre, le diamêtre apparent.

§ 43. Toutes fois, il enfaut excepter les pignons à rochet, parce que tout le rayon primitif, jusqu'à l'extrêmité de l'aile a très peu de chose près est parcouru par la pointe de la dent également formée en rochet tout comme l'aile du pignon; mais

rochet pinions ſeem to offer themſelves with, in conſequence of a theory founded upon the advantage of the uniformity of the frictions, experience teaches that, independently of the difficulty there is in the execution, this ſort of pinion is much more vicious than the other; it is conſequently quite out of uſe.

§ 44. I think it not at all requiſite to anſwer the vague objections which the friends of the arbitrary method may ſupport their ſyſtem by: it is enough for me, that almoſt all watch-makers are agreed upon the fulneſs of different pinions, and on the form of their teeth, to leave the neceſſary ſtrength and conſiſtence, which, for a pinion of ſix, comes to nearly a ſixth part of the diameter; for one of ſeven, to a ſeventh; one of eight, to an eighth; one

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mais avec quelque avantage que ces pignons à rochet ſe préſentent à nôtre idée fondée même ſur une théorie bien raiſonnée en faveur de l'uniformité des frottemens, l'expérience nous démontre, outre la grande difficulté dans l'exécution, que cette ſorte de pignon eſt beaucoup plus vicieuſe que les autres; auſſi n'en fait on preſque plus d'uſage.

§ 44. Nous nous eſtimons très diſpenſé de repondre ou de combattre toutes les objections vagues, que les partiſans de l'arbitraire pourroient alléguer en faveur de leur ſiſtême; il ſuffit que preſque tous les horlogers ſont d'accord ſur le point de vuidage des differens pignons et ſur la forme de leurs ailes, pour en laiſſer la force et conſiſtence néceſſaire qui ſe reduit pour pignon de 6 à très peu de choſe près à la 6e partie de ſon diamêtre; pour pignon de 7 à la 7e; pour 8 à la 8e; pour

one of ten, to a tenth, &c. The moſt ordinary workman is able to judge at firſt ſight, whether the pinion is deep and open to the center as it ought to be, and if the wheels are of a proper flatneſs, &c.

§ 45. And though there ſhould be ſome little variation as of ſix to ſix and a half, and from ſix to five and a half *per* tooth of a pinion of ſix; this difference, though very ſtriking to the ſight, would not either increaſe or diminiſh the apparent diameter by even a forty-eighth, whence ſtrictly ſpeaking there would only be a twenty-fourth part of a tooth, more or leſs, which would raiſe about two-thirds of a degree *per* tooth, ſo that one may in fact look upon all objections of this nature as being as vague as they are groundleſs.

§ 46.

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pour 10 à la 10ᵉ, &c. Vû que le plus ſimple ouvrier en ce genre eſt aſſés habile pour juger du premier coup d'œil, ſi un pignon eſt enfoncé et vuidé à ſon point, et ſi les ailes ſont bien formée en planches, &c.

§ 45. Quand il y auroit même quelque petit écart comme de 6 à $6\frac{1}{2}$ et de 6 à $5\frac{1}{2}$ par aile, d'un pignon de ſix, cette difference, quoi que très frappante à la vuë, n'augmenteroit ni ne diminueroit ſon diamêtre apparent, pas ſeulement d'un 48ᵉ d'où il reſulteroit à toute rigueur ſeulement la 24ᵉ partie d'une dent de plus ou de moins qui opéreroit environ $\frac{2}{3}$ de degré de levée par aile; de ſorte que l'on pourroit judicieuſement enviſager les objections de cette nature comme auſſi vagues qu'inconſéquentes.

6 § 46.

§ 46. I ſhall not ſtop to give leſſons upon the fixing of the true point of the depth, becauſe every workman or finiſher muſt know from the firſt elements of this theory, that the right depth muſt begin upon the tooth which divides the line of center; that is, that in every caſe, whether of two ſmall or two large pinions, one muſt look for the middle of the two extremities, and fix the point of their depths, when theſe two movers run the moſt eaſy. Let only, I ſay, this ſame method be purſued with reſpect to pinions of juſt ſize, by carrying on the depthening a little too far till one perceives a ſmall fall, and then carrying it to a diſtance again to make this fall diſappear, all the requiſites for a good and perfect depthening will be found united; for the pinion being in the due proportion with its tooth, the lead or depth can

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§ 46. Je ne m'arrêterai point ici pour donner des leçons ſur la fixation du vrai point des engrenages, puiſque chaque ouvrier ſoit finiſſeur doit ſavoir ce la par les prémiers élémens de cette théorie que l'attouchement ou la menée doit commencer ſur l'aile partagéant la ligne des centres, en ſuivant même leurs marottes ordinaires; c'eſt à dire qu'on doit chercher dans tous les cas ſoit pour pignons trop petits, le milieu des deux extrêmités et fixer leurs points d'engrenages ou ces deux mobiles roulent le plus doux; que l'on ſuive dis-je, ſeulement la même méthode à légard des pignons de juſtes groſſeurs en pouſſant l'engrenage un peu trop en avant juſques à ce que l'on s'apperçoive d'une petite chûte, et en l'eloignant enſuite pour faire diſparoitre cette chûte; toutes les conditions d'un bon et parfait engrenage s'y trouveront réunies, car le pignon étant dans ſon vrai rapport, avec la

can only begin ſucceſſively upon the central line, as likewiſe the operation of the riſe in ſo many required degrees between theſe two moveables, the proof of which may be evidently demonſtrated by the inſtrument made with two parts of a circle, in which the arc of the riſe, run over by each of the movers, is diſtinguiſhable to one half, one quarter, and even one eighth of a degree. It would certainly be ſuperfluous to point out the manner of operating with ſuch a machine, ſince all thoſe who have any notion of the method of uſing mechanical and mathematical inſtruments will immediately ſee what is meant.

§ 47. But ſhould any body doubt of the reality, the utility, or the importance of this rule, the means of ſatisfaction are offered by the experiment of the degrees of riſe

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a roue, l'attouchement, ou la menée ne peut du moins que commencer ſucceſſivement ſur la ligne centrale, ainſi que l'opération de la levée en autant de degrés requis, entre ces deux mobiles, dont la preuve ſe peut démontrer d'une maniére evidente par l'inſtrument à deux portions de cercle, ou l'arc de levée parcouru par chacun des mobiles, ſe diſtingue juſqu'à $\frac{1}{2}$, $\frac{1}{4}$, et même a $\frac{1}{8}$e; d'un degré près; il ſeroit ſans doute très ſuperflu d'indiquer la maniére d'opérer ſur la dite machine, car toutes les perſonnes qui auront quelques connoiſſance et quelque pratique des inſtrumens de mechanique ou mathematique verront au premier coup d'œil de quoi il s'agit.

§ 47. Au reſte ſi quelqu'un vouloit revoquer en doute la réalité, l'utilite, et l'importance de cette régle, on offre les moyens à tous ceux qui le déſireront, d'en

 faire

riſe required. This will convince every body, as it has convinced the author, who both looks upon this operation as the beſt touch-ſtone, and conſiders it as an axiom equally new and eaſy for the reſolution of a problem which has been an object of debate amongſt watchmakers for above a century.

§ 48. The author thinks too, that this rule is applicable to all pinions whatſoever. The pinion of ſix has been choſen for the experiments, as inconteſtibly the moſt vicious; ſo vicious that, in my opinion, it deſerves the fate of the pinion of five, eſpecially for very good watches, which perhaps will be ſtill better made out when one comes to know the gradual difference there is between

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faire l'experience par les degrés de levées requis; pour s'en convaincre indubitablement tout comme l'auteur, qui eſtime à juſte titre cette operation comme la meilleure pierre de touche et même qui la regarde comme un axiome auſſi facile que nouveau, pour reſoudre ſans replique un problême qui depuis plus d'un ſiécle a fait un objet de débat et de conteſtation dans l'horlogerie; par ce que l'incertitude des vrais principes a été une ſuite preſque génerale juſques à nos jours 1777; ainſi que l'expérience le démontre évidemment.

§ 48 L'auteur penſe au reſte que cette régle eſt applicable à tous les nombres et pignons quelconques; et pour plus grande ſureté de preuve dans les différentes expériences, l'on a choiſi le pignon de ſix comme étant ſans contredit le plus vicieux, ou le moins avantageux de tous les autres; auſſi à mon avis mériteroit il le même ſort que le pignon de cinq, ſur tout pour des montres de qualité, ce qui ſe verra peut-être dans ſon temps quand on connôitra la différence graduelle qui ſe

between each pinion, which will be amply proved by ſome experimental obſervations of ſome friends of mine on the theory and practice of watch-making in general.

§ 49. I will not, however, pretend that my inſtrument is not ſtill capable of ſeveral uſeful improvements.

§ 50. But I ſubmit the rule, together with the compaſs of proportion, to all ingenious and well-informed perſons, either for approbation or full confutation of my ſentiments.

§ 51.

ſe trouve entre chaque pignon et qui ſera amplement détaillée dans les obſervations expérimentales de MM. L.C. et P. ſur la théorie et ſur la pratique de l'horlogerie en général.

§ 49. Malgré des preuves auſſi authentiques et auſſi convainquantes de la réalité de mon ſuſdit expoſé, je n'oſerois cependant pas dire que la machine ne ſoit pas ſuſceptible d'une plus grande perfection et de diverſes additions avantageuſes.

§ 50. En attendant on ſoumet, avec une ſinc`re confiance cette régle ainſi que le compas de proportion, qui dans tous les cas lui ſert de démonſtrateur et de rapporteur fidéle, à l'examen des perſonns éclairées qui peuvent en être juges compétens, pour en obtenir l'approbation en faveur des vrais principes, et pour exciter l'emulation ſi néceſſaire dans l'horlogerie; Si au contraire cette approbation n'a pas lieu, on demande une reſutation en bonne et duë forme.

§ 51. And though the ufe of the compafs extends to a great multiplicity of objects, I have only mentioned the moft interefting ones with regard to watch-making. Obferve that each pinion has its particular divifion upon the branches of the inftrument, and that each column is directed by the number of pinions from fix to twelve.

§ 52. The line or moving branch, which croffes the two branches of the compafs, is divided according to the French foot in inches, lines, &c. to a 48th or 36th of a line; fo that one may know at one and the fame time both the true proportion of the different diameters, and their precife fizes. If one wifhes to know the fize of a wheel for a pinion of a given thicknefs, one puts the pinion

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§ 51. Quoi que l'ufage de nôtre fufdit compas s'étende fur une multitude d'objets, on a feulement cité les plus intéreffans relativement à l'horlogerie; on obfervera que chaque pignon a fa divifion particuliére fur les branches de la machine et que chaque colomne eft dirigée par le nombre des pignons depuis 6 jufques à 12.

§ 52. La ligne ou branche mouvante qui croife les deux branches du compas porte la divifion du pied de Roi, en pouces, lignes, &c. jufques à un 48e ou 36 douziémes de ligne, car une ligne à la machoire donne un pouce fur la dite divifion du pied; en forte que l'on peut favoir tout d'un tems et le jufte rapport des différens diamêtres et leurs grandeurs précifes, &c.; fi l'on veut favoir la grandeur d'une rouë pour pignon d'une groffeur donnée: on fait entrer le pignon jufte entre les deux

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nion between the jaw, and one carries the divider to the number of teeth which the wheel muſt have and the diviſion of ſuch a pinion indicates, then putting the divider of the other branch facing the aperture of this angle will indicate the juſt ſize of the wheel: for inſtance, if I have a pinion of ſix, and want to know what muſt be the thickneſs of a wheel of ſixty teeth, relatively to the proportion of the ſaid pinion, I take the juſt meaſure of the pinion by the jaws, and put the divider upon 60 of the column or diviſion of the pinion of ſix, and the other divider facing, then the aperture between the points of the divider, is the ſize of the wheel. The ſame operation takes place in all the other pinions, according to their numbers, with this proviſo, that you muſt do the reverſe when

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deux machoires ou pinces et on méne le coulant ſur le nombre des dents que la rouë doit avoir et que la diviſion d'un tel pignon indique; et en mettant le coulant de l'autre branche vis à vis, l'ouverture de cet angle indiquera la juſte grandeur de la rouë; par exemple, ſi j'ai un pignon de ſix et que je veuille ſavoir de quelle groſſeur une rouë de 60 dents doit être relativement au rapport du dit pignon, je prends la méſure juſte du pignon, par les machoires, et je mets le coulant ſur 60 de la colonne ſoit de la diviſion du pignon de ſix, et l'autre coulant vis à vis, alors l'ouverture entre les deux becs des coulans eſt la grandeur de la rouë; la même opération a lieu pour tous les autres pignons ſelon leurs nombres; bien entendu qu'il faut faire l'oppoſé lorſqu'on a une grandeur de rouë donnée et

when the ſize of the wheel is given, and you want to know the thickneſs of the pinion. The ſame thing holds for the meaſure of the vargeous palets, the balance wheel, the cylinder and its wheel, the fuſee and barrel, the pivots, and, generally ſpeaking, whatever requires dimenſion.

§ 53. In order to find immediately the true proportion between the ſlide wheel and the curb, you muſt put the ſlide wheel upon the number you wiſh it to have of the diviſion of the pinion of ſix; afterwards (without moving the head of the compaſs) you ſeparate the moveable dividers till the wheel of the curb is exactly upon it, and it is on the number of the diviſion of the pinion of ſix, indicated by the divider, that the curb muſt be ſlit.

§ 54. One muſt obſerve, that the diviſions of the pinions of ten and twelve is the ſame upon the two branches;

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que l'on veut ſavoir la groſſeur du pignon; il en eſt de même pour la méſure des palettes de verges, de la rouë de rencontre, du cilindre et de ſa rouë, de la fuſée et barillet et des pivots, et généralement de tout ce qui exige dimenſion.

§ 53. Pour trouver auſſi tôt le juſte rapport entre la rouë de roſette ſur le rateau ſoit la couliſſerie, il faut mettre la rouë de roſette ſur le nombre que l'on ſouhaite qu'elle aie, de la diviſion du pignon de ſix, enſuite (ſans bouger la tête du compas) l'on eloigne les coulants juſques à ce que la rouë de rateau s'y trouve juſte, et c'eſt ſur ce nombre de la diviſion du pignon de ſix que le coulant indique, que le rateau doit être fendu.

§ 54. Il faut obſerver que la diviſion pour pignon de 10 et de 12 eſt de même ſur

branches; this is done the more eaſily to put the two ſliders facing each other in all caſes.

When one wiſhes to meaſure pieces which are larger than the aperture of the jaws allows, one puts the ſliders on the edge of the ſmall points when there is a hole in the middle of the branches where the aperture of the angle is only a ſixth part of the diviſion of a foot, that is, if the angle upon the diviſion of the foot is ſix inches open, that of the middle will be only one inch and ſo on.

§ 55. As to the other machine of braſs, with two portions of a circle divided as far as half a quarter of a degree, one may with the greateſt preciſion verify upon it the true proportion or thickneſs of the pinions by the degrees of riſe required by making the different wheels from

ſur les deux branches, et cela pour mettre plus facilement dans tous les cas les coulans parfaitement vis à vis.

Lorſqu'on voudroit meſurer quelques piéces plus grande que l'ouverture des machoires ne le permet, on met les coulans ſur le bord des petits points ou il y a un trou au milieu des branches ou l'ouverture de ſon angle n'eſt que la ſixiéme partie de celui de la diviſion du pied; c'eſt à dire ſi l'angle ſur la diviſion du pied eſt ouvert à ſix pouces, celui du milieu ne le ſera qu'à un pouce, ainſi du reſte.

§ 55. Quant à l'autre machine en leton à deux portions de cercle diviſée juſques à un demi quart de degré; l'on peut y vérifier avec la plus grande préciſion, le juſte rapport des pignons ſoit groſſeur par les degrés de levées requiſes, et

from 12 to 120 operate upon the fame pinion the degrees of rife required. Thefe wheels exactly follow the dimenfions of the rule we are fpeaking of. Nobody will doubt but that the fame experiments upon all the other pinions will be equally demonftrative and convincing as thofe upon the pinion of fix.

§ 56. This machine may be likewife ufeful in determining with great precifion the figure of the curve of the teeth of the wheels, that is, by placing the additional femi-circle upon the center of the wheel, and putting a needle upon the axis, one will obferve, if the needle of the pinion of fix goes regularly through the ten degrees whilft that of the wheel is going through one only; if not, one will fee very diftinctly the irregularity of the curve,

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et principalement pour fe convaincre de la réalité de nôtre fufdite régle, en faifant opérer les différentes rouës depuis le nombre de 12 jufques à 120 fur le même pignon les degrés de levées requis; ces rouës fuivent exactement les dimenfions de la régle en queftion; on ne doutera fans doute pas que les mêmes expériences fur tous les autres pignons ne feroient pas moins demonftratives et convaincantes que celles fur le pignon de fix.

§ 56. Cette machine peut encore fervir pour déterminer avec une grande précifion la vraie figure de la courbe des dents de la roué fçavoir en pofant le demi cercle poftiche fur le centre de la rouë, et en mettant une aiguille fur l'axe ou l'arbre; on obfervera fi l'aiguille du pignon de fix parcourt reguliérement les dix degrés pendant que celle de la rouë n'en parcourt qu'un, fi non on verra trés diftinctement la difformité de la courbe pour lui donner la forme requife, pour opérer

curve, in order to the giving it the requisite form uniformly to operate the degrees of rise upon the teeth of the pinion, which comes very nearly to the geometrical curve, the operations of which are not to be understood by all watch-makers as this instrument may be.

§ 57. Every body sees that the curve of a wheel of 120 teeth must naturally be one half shorter than that of one of 60, since it goes only through three degrees whilst the tooth of the pinion goes through 60. The same holds in every case in which the curve of the tooth must be figured, according to the degrees it goes over in its axis, to operate upon the wheel of any pinion whatever that arc of the rise the most uniformly possible, and in order to join these advantages to others still more considerable, it will be useful to make use of wheels and pinions

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opérer avec uniformité les degrés de levées sur l'aile du pignon, ce qui revient à peu de chose près à la courbe géométrique dont les opérations ne sont pas de la compétence de tous les horlogers comme le seroit cet instrument.

§ 57. On sent bien que la courbe d'une rouë de 120 dents doit être naturellement plus courte de la moitié que celle de 60 puis qu'elle ne parcourt que 3° pendant que l'aile du pignon en parcourt 60, &c. Il en est de mêmes dans tous les cas où la courbe de la dent doit être figurée selon les degrés qu'elle parcourt sur son axe pour opérer sur l'aile d'un pignon quelconque, l'arc de levée le plus uniformément possible; et pour réunir ces derniers avantages à d'autres plus considerables encore, il convient de faire usage des pignons et rouës aussi nombreuses que

nions with as great a number of teeth as the greatneſs of the caliber permits. By this means one not only comes nearer the true geometrical curve; but one reduces the reſiſtance and preſſure upon the axis of the moveable to the leaſt term poſſible.

§ 58. As to the true point of the opening of the teeth, this is my rule. I take rather a ſoft file, in thickneſs ſix and a quarter of its diameter for a pinion of ſix, ſeven and a quarter for one of ſeven, eight and a quarter for one of eight, ten and a quarter for one of ten, twelve and a quarter for one of twelve, and ſo on; and I believe that this rule may be the eaſieſt for the preſervation both of the beſt figure of the curves and the neceſſary force of their teeth. Juſt as the thickneſs of the teeth of a pinion of ſix muſt be the ſixth part of its diameter; that of

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que la grandeur du calibre le permet; par ce moyen on s'approche non ſeulement de la vraie courbe géometrique, mais on réduit encore la réſiſtance et la preſſion ſur l'axe des mobiles au moindre terme poſſible.

§ 58. Quant au juſte point de vuidage de la denture des roués de tel nombre et grandeur quelconque, voici ma régle: je prends une lime ou fraiſe pas trop rude de l'epaiſſeur pour pignon de ſix, la 6^e et $\frac{1}{4}$ de ſon diamêtre; pour 7 la 7^e et $\frac{1}{4}$; pour 8 la 8^e $\frac{1}{4}$; pour 10 la 10^e $\frac{1}{4}$; pour 12 la 12^e $\frac{1}{4}$; ainſi du reſte pour tous les autres; et je crois que cette régle pourroit être la plus facile et la plus analogue pour la conſervation et de la meilleure figure des courbes et de la force néceſſaire de la denture, tout comme l'epaiſſeur des ailes d'un pignon de 6 doit

5 être

of the pinion of ſeven, a ſeventh; one of eight, an eighth; and ſo on, for any number of pinions whatever. Juſt in the ſame manner as the length of the teeth of the wheel, which ſhall be exactly the aperture of their angles, or (which comes to the ſame) half the diameter of its pinion of ſix, three ſevenths for a pinion of ſeven, three eighths for one of eight, three tenths for one of ten, three twelfths, or a quarter, for a pinion of twelve, and ſo on.

§ 59. To obſerve a ſtill more regular order, it would be likewiſe extremely proper that all the pinions of a watch or repeater, except the centre one, ſhould be of the ſame thickneſs; hence will ariſe many advantages both in the execution of the rough and finiſhed work, which I ſhall ſpeak more fully of upon ſome other occaſion.

être la 6ᵉ partie de ſon diamêtre; pour pignon de 7 la 7ᵉ; pour 8 la 8ᵉ, ainſi de ſuite pour tous les nombres de pignons que ce ſoit. Auſſi bien que la longueur des dents de la rouë qui ſera poſitivement de l'ouverture de leurs angles, ou ce qui eſt la même choſe, la moitié du diamêtre de ſon pignon de ſix les $\frac{3}{7}$ᵉˢ pour pignon de 7, les $\frac{3}{8}$ᵉˢ pour 8, les $\frac{3}{10}$ᵉˢ pour 10, les $\frac{3}{12}$ᵉˢ ſoit le quart pour pignon de douze, ainſi de ſuite.

§ 59. Pour obſerver d'autant mieux et à tous égards une ſuite d'ordre, il feroit auſſi très à propos, que tous les pignons d'une montre ou répétition (excepté celui du centre) fuſſent de la même groſſeur; d'où reſulte divers avantages ſoit dans l'exécution du brut, ſoit dans celui du finiſſage, dont on parlera plus amplement à la premiére occaſion.

TABLE DE DEDUCTIONS.

Les deux mobiles en parfait equilibre. Pignons et Rouës. 6	et 6	Diam. apparent. 1 ligne.	Diam. vrai. 1 ligne.	Ligne chacun de diamêtre.
2	12	24	22	2 douziémes de ligne par revolution à déduire.
3	18	36	32	4
4	24	48	42	6
5	30	60	52	8
6	36	72	62	10
7	42	84	72	—— 12 douziémes.
8	48	96	82	
9	54	108	92	
10	60	120	102	
11	66	132	112	
12	72	144	122	
13	78	156	132	—— 24 douziémes.
14	84	168	142	
15	90	180	152	
16	96	192	162	
17	102	204	172	
18	108	216	182	
19	114	228	192	—— 36 douziémes.
20	120	240	202	
21	126	252	212	
22	132	264	222	
23	138	276	232	
24	144	288	242	
25	150	300	252	—— 48 douziémes de ligne à retrancher fur une rouë de 150 dents foit 24 révolutions effectives d'un pignon de fix.
			48	
		300	300	

Révolutions paffives du pignons. Dents de la roue.

Pour

Pour Pignons de Sept.

Les deux mobiles en parfait equilibre. — Pignons et Rouës. Révolutions paſſives du pignon.	Dents de la roue :	Diam. apparent.	Diam. vrai.	
7	et 7	$10\frac{1}{2}$	$10\frac{1}{2}$	Douz. de ligne de diam. chacun.
2	14	21	$19\frac{1}{2}$	$1\frac{1}{2}$ douziémes de ligne à déduire par revolution.
3	21	$31\frac{1}{2}$	$28\frac{1}{2}$	3
4	28	42	$37\frac{1}{2}$	$4\frac{1}{2}$
5	35	$52\frac{1}{2}$	$46\frac{1}{2}$	6
6	42	63	$55\frac{1}{2}$	$7\frac{1}{2}$
7	49	$73\frac{1}{2}$	$64\frac{1}{2}$	9
8	56	84	$73\frac{1}{2}$	$10\frac{1}{2}$ douziémes.
9	63	$94\frac{1}{2}$	$82\frac{1}{2}$	
10	70	105	$91\frac{1}{2}$	
11	77	$115\frac{1}{2}$	$100\frac{1}{2}$	
12	84	126	$109\frac{1}{2}$	
13	91	$136\frac{1}{2}$	$118\frac{1}{2}$	
14	98	147	$127\frac{1}{2}$	
15	105	$157\frac{1}{2}$	$136\frac{1}{2}$	21 douziémes.
16	112	168	$145\frac{1}{2}$	
17	119	$178\frac{1}{2}$	$154\frac{1}{2}$	
18	126	189	$163\frac{1}{2}$	
19	133	$199\frac{1}{2}$	$172\frac{1}{2}$	
20	140	210	$181\frac{1}{2}$	
21	147	$220\frac{1}{2}$	$190\frac{1}{2}$	
22	154	231	$199\frac{1}{2}$	$31\frac{1}{2}$ douziémes.
23	161	$241\frac{1}{2}$	$208\frac{1}{2}$	
24	168	252	$217\frac{1}{2}$	
25	175	$262\frac{1}{2}$	$226\frac{1}{2}$	
26	182	273	$235\frac{1}{2}$	
27	189	$283\frac{1}{2}$	$244\frac{1}{2}$	
28	196	294	$253\frac{1}{2}$	
29	203	$304\frac{1}{2}$	$262\frac{1}{2}$	42 douziémes de ligne à retrancher ſur une roue de 203 dents ſoit 28 révolutions effectives d'un pignon de ſept.
			42	
		$304\frac{1}{2}$	$304\frac{1}{2}$	

Pour Pignons de Huit.

Les deux mobiles en parfait equilibre. Pignons et Roués. 8	et 8	Diam. apparent.	Diam. vrâi.	
Révolutions passives du pignon.	Dents de la roue:	1 ligne.	1 ligne.	de diamêtre chacun.
2	16	24	$22\frac{1}{2}$	$1\frac{1}{2}$ douziémes de ligne à déduire par révolution.
3	24	36	33	3
4	32	48	$43\frac{1}{2}$	$4\frac{1}{2}$
5	40	60	54	6
6	48	72	$64\frac{1}{2}$	$7\frac{1}{2}$
7	56	84	75	9
8	64	96	$85\frac{1}{2}$	$10\frac{1}{2}$
9	72	108	96	—— 12 douziémes.
10	80	120	$106\frac{1}{2}$	
11	88	132	117	
12	96	144	$127\frac{1}{2}$	
13	104	156	138	
14	112	168	$148\frac{1}{2}$	
15	120	180	159	
16	128	192	$169\frac{1}{2}$	
17	136	204	180	—— 24 douziémes.
18	144	216	$190\frac{1}{2}$	
19	152	228	201	
20	160	240	$211\frac{1}{2}$	
21	168	252	222	
22	176	264	$232\frac{1}{2}$	
23	184	276	243	
24	192	288	$253\frac{1}{2}$	
25	200	300	264	—— 36 douziémes de ligne a retrancher sur une roue de 200 dents soit 24 révolutions effectives d'un pignon de huit.
			36	
		300	300	

Pour Pignons de Dix.

Les deux mobiles en parfait equilibre.		Diam. apparent.	Diam. vrai.	
Pignons et Rouës.				
10	et 10	15	15	de diamêtre chacun.
2	20	30	28½	1½ douziémes de ligne à déduire par révolution.
3	30	45	42	3
4	40	60	55½	4½
5	50	75	69	6
6	60	90	82½	7½
7	70	105	96	9
8	80	120	109½	10½
9	90	135	123	12
10	100	150	136½	13½
11	110	165	150	—— 15 douziémes.
12	120	180	163½	
13	130	195	177	
14	140	210	190½	
15	150	225	204	
16	160	240	217½	
17	170	255	231	
18	180	270	244½	
19	190	285	258	
20	200	300	271½	
21	210	315	285	—— 30 douziémes de ligne a retrancher fur une rouë de 210 dents foit 20 révolutions effectives d'un pignon de dix.
			30	
		315	315	

Révolutions paffives du pignon. — Dents de la rouë.

Pour

Pour Pignons de Douze.

Les deux mobiles en parfait equilibre.		Diam. apparent.	Diam. vrai.	
Pignons et Rouës.				
12	et 12	18	18	Douz. de ligne de diam. chacun.
Révolutions passives du pignon.	Dents de la rouë.			
2	24	36	34½	1½ douziémes de ligne à retrancher par révolution.
3	36	54	51	3
4	48	72	67½	4½
5	60	90	84	6
6	72	108	100½	7½
7	84	126	117	9
8	96	144	133½	10½
9	108	162	150	12
10.	120	180	166½	13½
11	132	198	183	15
12	144	216	199½	16½
13	156	234	216	—— 18 douziémes.
14	168	252	232½	
15	180	270	249	
16	192	288	265½	
17	204	306	282	
18	216	324	298½	
19	228	342	315	
20	240	360	331½	
21	252	378	347	
22	264	396	363½	
23	276	414	380	
24	288	432	396½	
25	300	450	414	—— 36 douziémes de ligne à retrancher fur une rouë de 300 dents foit 24 révolutions effectives d'un pignon de 12.
			36	
		450	450	

XLIV. *New Experiments upon the* Leyden *Phial, respecting the Termination of Conductors. By* Benjamin Wilson, *Esq. F. R. S.*

Read July 9, 1778.

IN the LXIVth volume of the Philosophical Transactions there is a paper of Mr. HENLY's upon the subject of conductors, wherein are contained several experiments, intended to shew that pointed terminations are preferable to spherical ones for securing buildings, &c. from accidents by lightning.

Upon those experiments I made some observations, and particularly upon the fifth, where a point and ball were placed at the same distance from a sphere of copper, so as to make part of the circuit in the Leyden experiment[(a)]. In the description of that experiment I objected to the two chains employed therein, because the metallic communication was, by that method, considerably interrupted, on account of a want of contact between the several links composing the chains. I did not then repeat the experiment because the particular circumstances attending the Leyden phial appeared, in my

(a) Farther Observations upon Lightning, by B. WILSON, published in Phil. Trans. for the year 1774.

judge-

judgement, very unlike what happens in nature; and therefore I contented myſelf with pointing out the ſeveral circumſtances in which they differed; and in obſerving that, according to Mr. HENLY'S account, the point did not protect the rounded end from being ſtruck, which it ought to have done, if Dr. FRANKLIN'S philoſophy was well-founded.

Since that time an occaſion has offered which made it neceſſary to try this particular experiment. The occaſion alluded to aroſe from a late inveſtigation of Mr. NAIRNE'S experiments by Dr. MUSGRAVE, who was deſirous of having that experiment repeated; becauſe (as it ſtood in Mr. HENLY'S account) it ſeemed to contradict a conſiderable part of the doctor's reaſoning.

Not being furniſhed with an apparatus to make the experiment, I requeſted the favour of Mr. CAVALLO to aſſiſt me with his; and though it was not ſo compleat for the purpoſe as could be wiſhed, yet it anſwered ſufficiently well to ſhew, that an attention to the circumſtance of a perfect communication in this experiment was very material to diſcover the truth; and that the want of it had, probably, occaſioned the ball being ſtruck in preference to the point, as related by Mr. HENLY: for upon employing a wire inſtead of the chains, the point was ſtruck at more than *three times* the diſtance of the ball.

Seeing

Seeing ſo great a difference between the two experiments, I ordered ſuch an apparatus to be made as I thought would be the leaſt exceptionable for the purpoſes of determining the fact upon which theſe different appearances ſeemed to depend; namely, a perfect and an imperfect circuit of communication with the Leyden phial.

In the contriving of this inſtrument it appeared material, in order to have it anſwer the ſame end as Mr. LANE's electrometer, that the ſeveral experiments to be tried with it might be compared with each other in a more accurate manner.

The circuit of communication was divided into two parts. A bent rod of braſs, with a ball of the ſame metal, three quarters of an inch in diameter, ſcrewed on to the upper extremity of it, and a copper ball, five inches in diameter, ſcrewed on to the lower end, formed one of the parts. This part was ſupported by a ſtand of wood that had a cap of braſs at the top, into which the braſs rod was occaſionally ſcrewed.

The other part of the circuit conſiſted of a braſs rod alſo, one end of which branched out in the form of a fork with two prongs, that pointed towards the center of the copper ball; and thoſe prongs were ſo conſtructed, that

that either of them could be made longer or ſhorter, juſt as the experiment required. On the end of one of the prongs was fixed a ball of braſs, three quarters of an inch in diameter, and on the other a ſharp ſteel point or needle. The ſhoulder of this fork ſcrewed into a ſmall plate of iron that was fixed on the inſide of a wooden veſſel, which contained the greateſt part of a cylindrical glaſs jar twelve inches and three quarters high, and about four inches in diameter. This glaſs was rather thick than otherwiſe, and the coating of it (which was tin-foil) meaſured nearly 144 ſquare inches on each ſurface. Beſides this coating, part of the inſide of the wooden veſſel was coated alſo with tin-foil, for the purpoſe of making a ſecure communication between the iron plate and the outward coating of the jar. Within the jar itſelf was fitted a cylinder of wood, that was covered with tin-foil alſo, to make a communication between the inſide coating of the glaſs and a braſs rod that was fixed upright in the center of the wooden cylinder. This upright rod having a ball of braſs at the end, three quarters of an inch in diameter, was bent towards the firſt part of the circuit: ſo that the two balls A and B in plate XVIII. fig. 2. being upon a level, looked towards each other, but were

placed

placed from time to time at different diſtances, as occaſion required, and thus anſwered the purpoſe of an electrometer.

Whilſt this inſtrument was making, Dr. LIND propoſed to Mr. CAVALLO and myſelf, that we ſhould ſee ſome experiments at his houſe which he had made in conſequence of thoſe we had before tried at Mr. CAVALLO's.

The apparatus he made uſe of was but ſmall (ſee fig. 1.); for the phial contained very little more than a pint, and the coating on its outward ſurface meaſured no more than thirty-nine ſquare inches.

The reſults of the ſeveral experiments we made are contained in the firſt of the following tables, from which it will appear, that in twenty-three experiments there was not any *one inſtance* where the ball was ſtruck at a greater diſtance than the point, nor even at the ſame diſtance. It is remarkable, that in two or three experiments where the point was farther off than the ball, both the point and the ball were ſtruck at the ſame time; which ſhews, that the influence of the point, although placed at a greater diſtance, was equal to the influence of the ſpherical termination placed conſiderably nearer.

When the forked inſtrument and electrometer were finiſhed, it was found, that a ſuperior force was neceſ-

ſary to charge the jar belonging to it (on account of its thickneſs) than what we had employed in our firſt trials.

Upon an application to Dr. HIGGINS he favoured me with the uſe of his machine; the cylinder of which, when excited with the aſſiſtance of his amalgama, acted ſo powerfully, that it charged the jar, accompanying the new inſtrument, very readily.

We began the experiments where the electrometer was ſtruck at the greateſt diſtance, and then adjuſted the diſtances of the ball and point from the copper ball accordingly; ſo that if the point was ſtruck (when they were adjuſted) the moving of the ball $\frac{1}{32}$d part of an inch would occaſion the ball to be ſtruck in preference to the point, and *vice verſâ*. Afterwards we leſſened the ſtriking diſtance of the electrometer in every experiment till we attained the leaſt diſtance.

Upon reverſing part of the apparatus, as in fig. 3. all thoſe experiments were repeated again; the copper ball being put neareſt to the glaſs in the place of the forked part, and the forked part in the place of the copper ball. This ſet of experiments being compleated, we made others, where the ball only was oppoſed; and after them, where the point only was oppoſed to the copper ball.

Having

Having gone through all theſe experiments as they are ſet down in the ſecond table, we then repeated the experiment with the chain, after Mr. HENLY's manner. The reſult of which, and with the apparatus reverſed, will appear in the third table.

The chain we employed upon this occaſion was of iron and very ruſty, no other being then at hand.

To avoid every objection it was reſolved upon, that all the experiments we had made at Dr. HIGGINS's ſhould be repeated, but with the two chains inſtead of the forked apparatus.

On the 23d of June, by the favour of Mr. PARTINGTON (Dr. HIGGINS having diſpoſed of his machine at that time) we went through the whole of the experiments thus circumſtanced. The chains employed were braſs, and a glaſs ſtand ſupported the ball and point. Mr. PARTINGTON's cylinder meaſured about thirteen inches in diameter: this glaſs, with the aſſiſtance of Dr. HIGGINS's amalgama, acted powerfully. All theſe experiments are contained in the fourth table.

Before this paper is concluded, it is neceſſary to caution thoſe who may be diſpoſed to repeat the experiments mentioned in the ſeveral preceding tables, that a ſtrict attention be had to every the leaſt circumſtance re-

lative to the making of the experiments; it being ſo difficult to preſerve the intended diſtances between the reſpective parts of an apparatus not perfectly executed (as is frequently the caſe with all new inſtruments) that we could not ſucceed in adjuſting the diſtances of the electrometer, ſo as to be exactly in an arithmetical progreſſion.

June 23, 1778. B. WILSON.

TABLE

TABLE I. FIG. I.

Experiments made at Dr. LIND's, June 18, 1778, with the Leyden phial.

Point and ball oppoſite the Leyden phial.				Ball only.	Point only.	Appar. reverſ.	Ball only.	Point only.
I.	Electrometer	68	– –	65	65	64		
	Ball –	18	Both ſtruck twice	59	–	18	Both ſtruck at	
	Point –	24	at the ſame time.	–	112	24	the ſame time.	
II.	Electrometer	64	– –	54	54	–	54	54
	Ball –	18	Both ſtruck twice	56	–	–	51	
	Point –	24	at the ſame time.	–	95	–	– –	66
III.	Electrometer	40	– –	–	–	40		
	Ball –	18	– –	–	–	18		
	Point –	25	– –	–	–	24		
(*b*) IV.	Electrometer	28	– –	28	28	–	28	28
	Ball –	34	ſtruck alternately.	26	–	–	23	
	Point –	73		–	68	–	– –	44
V.	Electrometer	24						
	Ball –	44						
	Point –	60						
VI.	Electrometer	22	– –	–	–	22		
	Ball –	18	– –	–	–	18		
	Point –	46	– –	–	–	26		
VII.	Electrometer	18	– –	–	–	18		
	Ball –	18	– –	–	–	18		
	Point –	48	– –	–	–	36		
VIII.	Electrometer	16	– –	–	–	16		
	Ball –	20	– –	–	–	15		
	Point –	56	– –	–	–	31		

N. B. Eighty of thoſe parts make one inch.

The number oppoſite the word electrometer denotes the diſtance between the balls which conſtituted the electrometer; and the numbers oppoſite to the words *ball* and *point* ſhew the ultimate diſtance at which they were reſpectively ſtruck.

(*b*) The point and ball in this experiment were not directed immediately towards the outſide coating of the jar, but towards the broad ſurface of a common tea canniſter, the oppoſite outſide of which was in contact with the coating of the jar.

JAMES LIND. TIBERIUS CAVALLO. B. WILSON.

TABLE II. FIG. II.

Experiments made at Dr. HIGGINS's, June 19, 1778, with the Leyden phial and Forked apparatus.

The meafures expreffed in the following tables were taken from a fcale containing 32 parts in one inch.

	Ball and point oppofite the Leyden phial.		Ball only.	Point only.	Apparatus reverfed.				Ball only.	Point only.
I.	Electrometer	32	32	32	32	–	–	–	32	32
	Ball –	34	48	–	34	–	–	–	36	–
	Point –	45	–	88	42	–	–	–	–	42
II.	Electrometer	28	28	28	28	–	–	–	28	28
	Ball –	30	43	–	36	–	–	–	33	–
	Point –	38	–	78	42	–	–	–	–	39
III.	Electrometer	25	26	26	25	–	–	–	26	26
	Ball –	28	36	–	31	–	–	–	32	–
	Point –	37	–	67	32	–	–	–	–	33
IV.	Electrometer	20	20	20	20	This exp. being repeated at intervals, other exp. intervening, gave the following refults:	20	20	20	20
	Ball –	28	29	–	29		28	30	25	–
	Point –	51	–	64	28		27	29	–	24
V.	Electrometer	16	16	16	16	–	–	–	16	16
	Ball –	22	20	–	22	–	–	–	23	–
	Point –	44	–	47	24	–	–	–	–	26
VI.	Electrometer	13	13	13	13	–	–	–	13	13
	Ball –	21	14	–	16	–	–	–	18	–
	Point –	38	–	36	22	–	–	–	–	22
VII.	Electrometer	10	10	10	10	–	–	–	10	10
	Ball –	12	10	–	13	–	–	–	12	–
	Point –	18	–	25	20	–	–	–	–	20

TABLE

TABLE III.

Experiments with the chain after Mr. HENLY's manner.

Point and ball oppoſite the Leyden phial.		Apparatus reverſed.		
Electrometer -	21	23	repeated at different times,	23
Ball - -	26	28		26
Point - -	24	26		30

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TABLE IV. FIG. III.

The experiments of the second and third table repeated at Mr. PARTINGTON's, June 23, 1778, a brass chain being used instead of the Forked apparatus.

	Ball and point opposite the Leyden phial.		Ball only.	Point only.	Apparatus reversed.			Ball only.	Point only.
I.	Electrometer	32	34	32	32	– –		32	32
	Ball –	40	39	–	30	– –		29	–
	Point –	76	–	71	38	– –		–	39
II.	Electrometer	28	28	28	28	– –		28	28
	Ball –	33	36	–	29	– –		28	–
	Point –	72	–	66	37	– –		–	38
III.	Electrometer	25	26	26	25	repeated	25	26	26
	Ball –	33	33	–	28		28	27	–
	Point –	46	–	64	35		37	–	37
IV.	Electrometer	20	20	20	20	– –		20	20
	Ball –	21	23	–	24	– –		24	–
	Point –	50	–	60	26	– –		–	27
V.	Electrometer	16	16	16	16	– –		16	16
	Ball –	21	15	–	19	alternately		19	–
	Point –	55	–	53	21			–	24
VI.	Electrometer	13	13	13	13	– –		13	13
	Ball –	16	11	–	14	– –		15	–
	Point –	44	–	42	19	– –		–	22
VII.	Electrometer	10	10	10	10	– –		10	10
	Ball –	11	9	–	11	alternately		12	–
	Point –	38	–	37	19			–	19

Because the electrometer in the experiments contained in the third table made at Dr. HIGGINS's with a rusty

iron chain ſtood at 21 and 23, we repeated the experiment at Mr. PARTINGTON's with a braſs chain, and the reſult was as follows:

Ball and point oppoſite the Leyden phial.		Apparatus reverſed.
Electrometer -	21	23
Ball - -	24	25
Point - -	64	30

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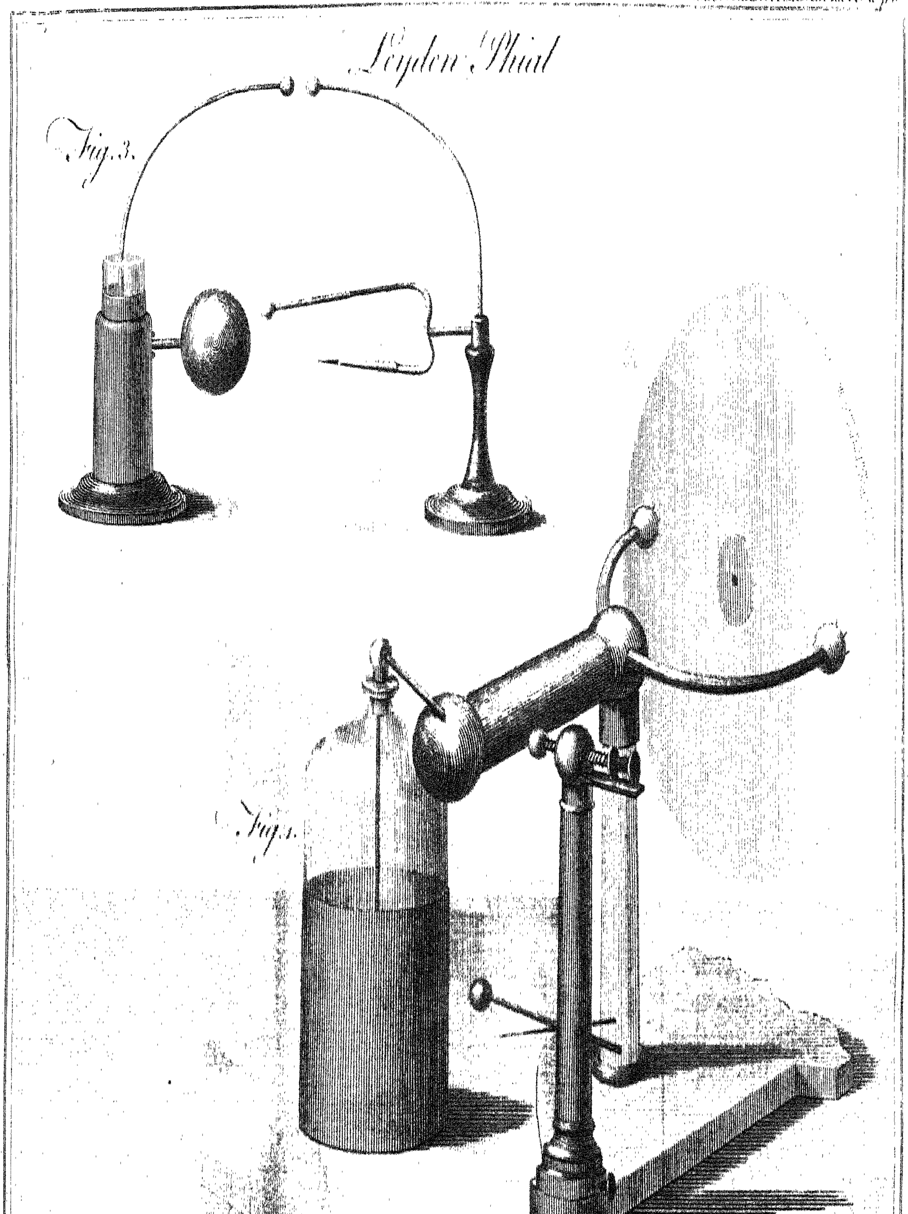
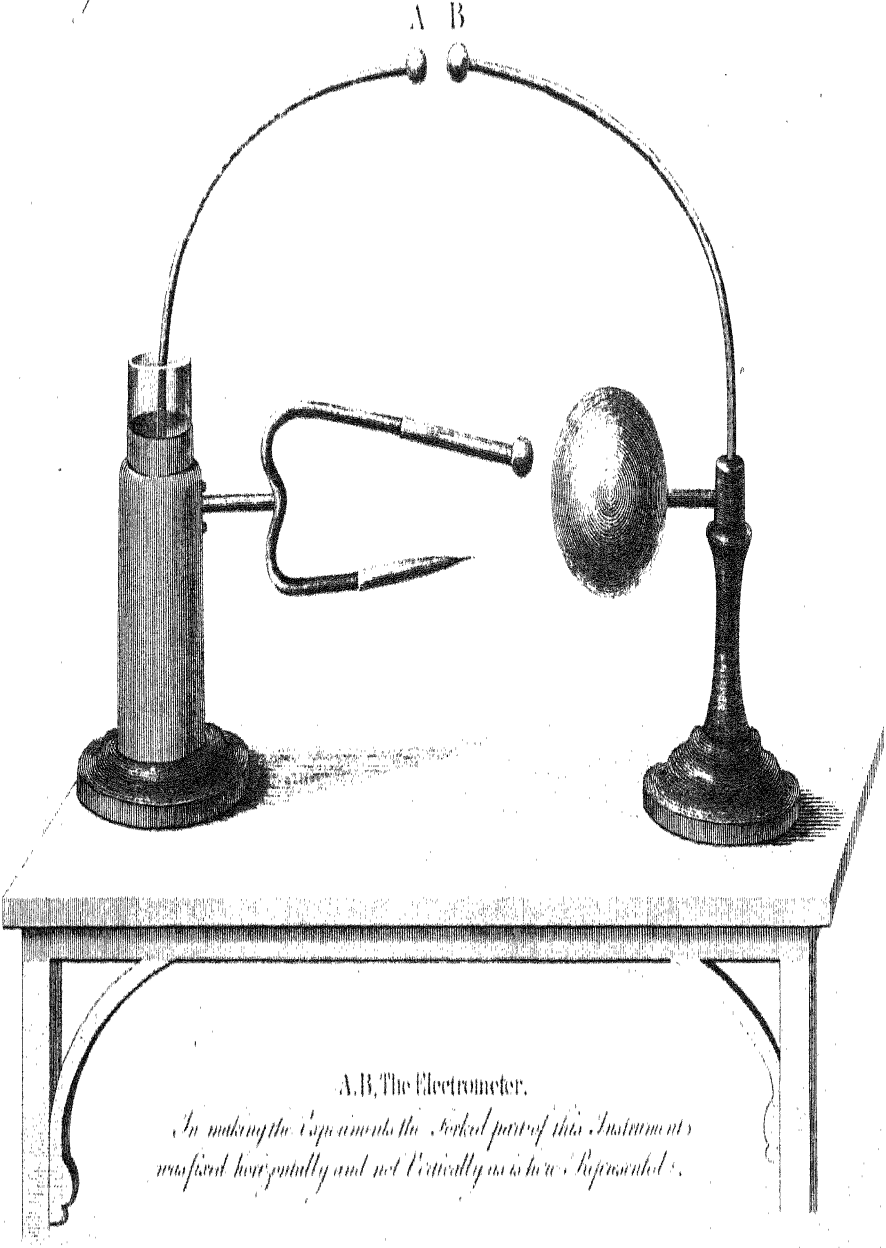
P. S. Having ſeen a method uſed by Mr. CAVALLO to repair broken Leyden phials, ſo as to render them again uſeful for experiments, I am glad of this opportunity to make it known, as it may be very acceptable to electricians. The method is as follows. When a coated phial is cracked, either by a ſpontaneous diſcharge or by any other accident, Mr. CAVALLO removes the outſide coating from the fractured part, and then makes it moderately hot, by holding it to the flame of a candle; and whilſt it remains hot, he applies burning ſealing wax to the part, ſo as to cover the fracture entirely; taking care that the thickneſs of the wax is rather more than the thickneſs of the glaſs. Laſtly, he covers all the ſealing wax, and alſo

part of the ſurface of the glaſs beyond it, with a compoſition made with four parts of bees wax, one of reſin, one of turpentine, and a very little oil of olives; which compoſition he ſpreads upon a piece of oiled ſilk, and applies it in the manner of a plaiſter. With this method I have ſeen ſeveral phials ſo effectually repaired, that, though after being frequently charged, they were at laſt broken by a ſpontaneous diſcharge, yet the fracture was in a different part of the glaſs.


Leyden Phial
Fig. 3.
Fig. 1.

Leyden Phial Fig. 2



A.B, The Electrometer.

In making the Experiments the Forked part of this Instrument was fixed horizontally and not Vertically as is here Represented.

XLV. *Obſervations on the Solar Eclipſe which happened June* 24, 1778. *By Mr.* William Wales, *F. R. S. and Maſter of the Royal Mathematical School in* Chriſt's Hoſpital.

Read July 9, 1778.

THE following obſervations of the ſolar eclipſe, which happened on the 24th inſtant, were made at the Royal Mathematical School in Chriſt's Hoſpital, where the latitude is 51° 30′ 55″ N. and the longitude not quite half a ſecond in time Weſt of the cupola of St. Paul's. I had my time by a moſt excellent watch made by Mr. LARCUM KENDALL, which goes while it is winding up, and has a proviſion for counteracting the effects of heat and cold. It was regulated by double altitudes of the Sun's lower limb, taken from a baſon of quickſilver with a HADLEY's quadrant of Mr. RAMSDEN's making; and the quickſilver was ſhaded from the wind by a roof, formed by two glaſſes whoſe planes had been ground perfectly parallel by the ſame ingenious artiſt, ſo that the time may, I think, be depended on within a ſecond, or two ſeconds at the moſt.

My teleſcope, which is of the Gregorian form, was made by the late Mr. SHORT; the focal length of the great ſpeculum being 18 inches, and the aperture $4\frac{1}{8}$ inches.

inches. I ufed a magnifying power of about 75 times for the beginning and end, and of about 50 or 55 times with the micrometer, in meafuring the Sun's diameter, and the diftances between the two cufps of the luminaries.

The micrometer, which is an exceeding good one, was made alfo by Mr. SHORT. The divided glafs is not achromatic, but only a fingle lens, whofe focal length is about 28 feet $5\frac{1}{2}$ inches; but as I have not had an opportunity of examining this point myfelf by adjufting the telefcope to parallel rays without the micrometer, and then putting it on, and meafuring the diftance at which objects are feen diftinctly, I have affumed the Sun's apogeal diameter to be $31' 28''$ as given by Mr. SHORT; and on that hypothefis the following reductions of the parts of the micrometer are made. Its error was determined immediately before the beginning of the eclipfe, by meafuring the angle fubtended by a fmall ball which is on the top of the fpire of St. Bride's Church, in Fleet Street, alternately before and after 0, or the beginning of the divifions of the fcale: thefe meafurements were as follow:

Off the scale, or before o.		On the scale, or after o.		
Inches.	Ver.	Inches.	Ver.	
0,05	4	0,05	11	
0,05	4	0,05	11	
0,05	$3\frac{1}{2}$	0,05	12	
0,05	$4\frac{1}{2}$	0,05	$12\frac{1}{2}$	
0,05	5	0,05	15	
0,05	4,3	0,05	12,3	Mean on the scale.
		0,05	4,3	Mean off the scale.
		0,00	8,0	Difference.

Half the above difference, or four divisions of the vernier, $= 4'',83$, is the error of the micrometer, to be subtracted from the measured distances of the cusps, and also from the diameters of the Sun, taken near the middle of the eclipse, in the same direction with the chords which were measured about the same time: and, this direction being nearly vertical, these measurements will, in some degree, be affected by refraction; but they may readily be corrected if the altitudes of the Sun be computed to the times when they were taken, and from thence the effect of the refractions.

1778.

1778	Times by the watch.	Double alt. of ☉'s L. L.	The Sun's diameter before obs. On.	The Sun's diameter before obs. Off.	The Sun's diameter after obs. On.	The Sun's diameter after obs. Off.	Apparent time.	Watch before apparent time.
	h ′ ″	° ″	′	′		′	h ′ ″	′ ″
♂ June 23	4 43 7	59 31¾	19	45⅔	18⅓	46	4 44 28,7	0 58,8
	4 44 0	59 13	18½	46	18½	46⅓		
	4 44 49	59 0	18½	46¼	18½	46⅓		
	4 45 52	58 39¾	18½	46¼	18½	46		
	4 47 1	58 19¾	18¼					
	4 47 50	58 3¾						
☿ 24	3 27 35	82 57	23	42	22¼	42¼	3 28 10,3	1 29,7
	3 28 40	82 36	23	41	22¾	42¼		
	3 29 47	82 16½	22½	41½	22¾	42¾		
	3 30 47	82 0	22¾	41¾				
	3 31 30	81 47¾		41¾				

Obser-

Observations on the eclipse.

1778	Time by the watch.	Apparent time.	Parts of the Micrometer. Inches.	Ver.	Reduced.	
	h ′ ″	h ′ ″			′ ″	
☿ June 24	3 41 17	3 39 47				Beginning very exact.
	4 2 3	4 0 32	2,15	15	21 51,8	Distance of the cusps.
	4 4 15	4 2 44	2,25	3	22 37,7	Ditto.
	4 5 24	4 3 53	2,30	2	23 6,7	Ditto.
	4 7 56	4 6 25	2,35	18	23 56,2	Ditto.
	4 8 41	4 7 13	2,40	4	24 9,5	Ditto.
	4 9 25	4 7 54	2,40	16	24 24,0	Ditto.
	4 11 5	4 9 34	2,45	19	24 57,8	Ditto.
	4 12 31	4 11 0	2,50	12	25 19,6	Ditto.
			3,10	18	31 29,2	Diameter of the Sun.
			3,10	17	31 28,0	Ditto.
			3,10	18	31 29,2	Ditto.
	4 21 52	4 20 21	2,65	17	26 56,2	Distance of the cusps.
	4 21 51	4 22 20	2,70	11½	27 19,8	Ditto.
	4 27 2	4 25 31	2,75	2	27 38,5	Ditto.
	4 29 36	4 28 5	2,75	4	27 40,9	Ditto.
	4 31 31	4 29 59½	2,75	8	27 45,7	Ditto.
	4 34 4	4 32 32½	2,75	16	27 55,4	Ditto.
	4 35 46	4 34 14½	2,75	18	27 57,8	Ditto.
	4 36 56	4 35 24½	2,75	15	27 54,2	Ditto.
	4 37 54	4 36 22½	2,75	15½	27 54,8	Ditto.
	4 38 38	4 37 6½	2,75	16½	27 56,0	Ditto.
	4 39 18	4 37 46½	2,75	16	27 55,4	Ditto.
	4 40 41	4 39 9½	2,75	14	27 53,0	Ditto.
	4 41 39	4 40 7½	2,75	13	27 51,8	Ditto.
	4 42 36	4 41 4½	2,75	4	27 40,9	Ditto.
	4 43 33	4 42 1½	2,70	21	27 31,2	Ditto.
	4 46 46	4 45 14½	2,70	14	27 22,8	Ditto.
			3,10	16	31 26,8	Diameter of the Sun.
			3,10	16	31 26,8	Ditto.
			3,10	16½	31 27,4	Ditto.
			3,10	15½	31 26,2	Ditto.
	4 52 40	4 51 8	2,60	11	26 18,8	Distance of the cusps.
	4 53 34	4 52 2	2,60	8	26 15,1	Ditto.
	4 54 20	4 52 48	2,60	3	26 9,1	Ditto.
	4 55 10	4 53 38	2,55	22	26 0,6	Ditto.
	5 26 34	5 25 1½				The end.

Time by the watch.	Double alt. of ☉'s L. L.	The Sun's diameter before obs. On.	Off.	after obs. On.	Off.	Apparent time.	Watch before apparent time.
h ′ ″	° ′	′	′	′	′	h ′ ″	′ ″
5 34 6	43 59¾	22	42	22	41½		
5 35 4	43 43	22	42	21¾	41½	5 34 31,2	1 32,8
5 35 40	43 29¾	22¼	41½	22	41½		
5 36 37	43 12						
5 37 5	43 2½						
5 37 53	42 51¾						

XLVI. *An Eclipse of the Sun June* 24, 1778, *observed at* Leicester. *By the Rev. Mr.* Ludlam, *Vicar of* Norton, *near* Leicester; *communicated by the* Astronomer Royal.

Read July 9, 1778.

THE beginning was observed at III^h 35′ 27″. The end at V^h 19′ 30″ or 34″, according to the time shown by the clock, the Sun being a little hazy at the end of the eclipse.

Zenith distances of the Sun's upper limb, taken with an eighteen inch quadrant made by BIRD, for determining the error of the clock.

Time by the Clock.	Z. Dist. ⊙ UL.
H M S	D M
IV 16 22	55 30
IV 29 35	57 30
IV 49 24½	60 30

 The

The barometer ſtood at 29,9; the thermometer at 71°. The error of the line of collimation was, in the ſummer of 1774, 13,8 ſeconds to be ſubtracted (ſee Phil. Tranſ. vol. LXV. part 2.). As the quadrant has not been altered, and indeed ſeldom uſed ſince, I ſuppoſe the error of the line of collimation to remain the ſame. From the firſt obſervation I make the clock to be 1′ 21″ faſter than ſolar time. From the ſecond, 1′ 22″. From the third, 1′ 23″. Taking the mean of theſe, *viz.* 1′ 22″, the beginning of the eclipſe at Leiceſter was at IIIh 34′ 5″; the end at v^{h} 18′ 8″ or 12″ by ſolar time.

The difference between the meridians of Greenwich and Leiceſter, from obſervations in the Philoſophical Tranſactions, computed by Mr. WALES, maſter of the royal mathematical ſchool in Chriſt's Hoſpital.

		M	S
From ſolar eclipſe June 3, 1769,	Beginning	4	24,5
	End	4	38,5
ζ Tauri, April 28, 1770,	Immerſion	4	27,8
Aldebaran, Nov. 18, 1774,	Emerſion	4	50,5
Solar eclipſe, June 24, 1778,	Beginning	4	23,2
	End	4	41,3

M. DU SEJOURS, in the Memoirs of the Academy of Sciences for 1771, makes the difference of the meridians of Paris and Leiceſter, from the end of the ſolar eclipſe of 1769, to be 13′ 59″; and the difference of the meridians

dians of Paris and Greenwich, both from the beginning and end of that eclipſe, 9′ 20″. Whence the difference of the meridians of Greenwich and Leiceſter 4′ 39″.

If we take the mean of all theſe computations, we ſhall have the difference between the meridian of the Obſervatory at Greenwich and St. Martin's Church in Leiceſter 4′ 35″ of time very nearly.

XLVII. *A ready Way of lighting a Candle, by a very moderate Electrical Spark.* *By* John Ingenhoufz, *M. D. F. R. S.*

Read July 9, 1778.

IT has been long known that an electrical fpark will kindle fpirit of wine, efpecially when previoufly warmed; and that vitriolic æther will be kindled by a very moderate fpark, even when cold. However, I never faw an electrician who made a common ufe of this experiment to light his candle when he had occafion for it. The reafon is, becaufe though it may be done without much danger of failing in the attempt, yet it requires fome trouble to prepare every thing neceffary for making the experiment anfwer with certainty. Befides, æther is very precious, and is eafily loft by evaporation before the electric power is excited, or before every thing is quite ready for performing the experiment.

I ufed to light my candle a good while ago by the explofion of a *fmall* jar (by fmall I underftand one which

has

has eight or ten inches of metallic coating, or even lefs) in the following manner. As I often amufe myfelf with electrical experiments, I have always an electrical machine, ready for action, fixed upon a table in my room. When I have occafion to light a candle, I charge a fmall coated phial, whofe knob is bent outwards, fo as to hang a little over the body of the phial; then I wrap fome loofe cotton over the extremity of a long brafs pin or a wire, fo as to ftick moderately faft to its fubftance. I next roll this extremity of the pin, wrapped up with cotton, in fome fine powder of refin (which I always keep in readinefs upon the table for this purpofe, either in a wide-mouthed phial or in a loofe paper); this being done, I apply the extremity of the pin or wire to the external coating of the charged phial, and bring, as quickly as poffible, the other extremity, wrapped round with cotton, to the knob: the powder of refin takes fire, and communicates its flame to the cotton, and both together burn long enough to light a candle. As I do not want more than half a minute to light my candle in this way, I find it a readier method than kindling it by flint and fteel, or calling a fervant.

I have found, that powder of white or yellow refin lights eafier than that of brown.

The

The *farina lycopodii* may be uſed for the ſame purpoſe, but it is not ſo good as the powder of reſin, becauſe it does not take fire quite ſo readily, requiring a ſtronger ſpark not to miſs; beſides, it is ſoon burnt away.

By dipping the cotton in oil of turpentine, the ſame effect may be as readily obtained, if you take a jar ſomewhat greater in ſize. This oil will inflame ſo much the readier if you ſtrew a few fine particles of braſs upon it. The pin duſt is the beſt for this purpoſe; but as this oil is ſcattered about by the exploſion, and, when kindled, fills the room with much more ſmoke than the powder of reſin, I prefer the laſt.

This experiment may be made uſe of for lighting a candle in the night as well as in the day. But for this purpoſe a charged phial ſhould always be kept in readineſs, and placed where it may be eaſily found in the dark. The jar for this purpoſe ſhould be furniſhed in the manner invented by Mr. CAVALLO, with a glaſs tube at the inſide, reaching from the mouth of the phial to the bottom, through which tube the wire which eſtabliſhes the communication with the inward coating paſſes, which, as ſoon as the phial is charged, is to be taken away, by holding it by the piece of ſealing wax, or glaſs rod covered with ſealing wax, faſtened to the knob of

 the

the wire, which wire is only to be put into the glaſs tube again when the phial is to be diſcharged.

Mr. CAVALLO finds, that this jar will keep its charge a month, if the glaſs tube, and likewiſe the jar where it is not coated, are carefully lined with ſealing wax both within and without.

A jar, containing ſix or eight pints, fitted up in this manner, may be kept as a magazine of electrical fire, and a little coated phial, juſt big enough to light a candle, may be occaſionally applied to it on purpoſe to light the powder of reſin. As ſoon as this little phial is charged, which is done in an inſtant, the wire muſt be taken out of the large jar or magazine, to keep the remainder of the charge, which may ſerve afterwards for ſeveral charges of the little phial.

I have often carried in my pocket ſuch a little jar a whole day, on purpoſe to fire a kind of piſtol loaded with inflammable air in the manner deſcribed by Mr. VOLTA of Como. A phial of about two ounces contained electrical fire enough to kindle ſuch a piſtol twenty times. In order to take out only as much of the electrical charge as was wanted for this purpoſe, I plunged into the glaſs tube of the charged phial a ſmall glaſs tube, four inches long, adapted as a Leyden phial, by ſticking

in it at the bottom, which is hermetically ſealed, a bit of tin foil, an inch long, coiled up, and paſting a ſimilar bit at the outſide: a thin wire paſſed through the tube from the inſide tin foil to the opening, which was ſhut by a ſmooth braſs ball ſtuck to it, and in contact with the ſaid wire. The outſide part of this tube, which was not coated with tin foil, was lined or varniſhed with ſealing wax.

XLVIII. *Electrical Experiments, to explain how far the Phenomena of the Electrophorus may be accounted for by Dr.* Franklin's *Theory of positive and negative Electricity; being the annual Lecture instituted by the Will of* Henry Baker, *Esq. F. R. S. By* John Ingenhousz, *M. D. F. R. S.*

Read June 4, 1778.

HAVING had the honour of being appointed by the President and Council of the Royal Society to read the annual dissertation on some philosophical subject, instituted by our worthy member the late Mr. BAKER, I have endeavoured to pursue some electrical experiments, to explain how far the *Electrophorus perpetuus* may be accounted for upon the almost generally received theory of Dr. FRANKLIN of positive and negative electricity.

THIS electrical instrument consists of two different pieces; *viz.* 1. a metallic body, in the form of a plate, or any other convenient figure, furnished with an insulating handle, to be used for lifting it up; and 2. a flat non-conducting substance, such as glass, resin, or some

other non-conducting matter, upon which the said metal plate is placed.

This machine, invented by Mr. VOLTA, a learned gentleman of Como, is certainly a valuable acquisition to the Electrical apparatus. Once excited, it is for a long while ready to afford electricity enough for all experiments which do not require a very great force; and it has the advantage of not being so much affected by damp weather as the common machines with glass globes, cylinders, disk, &c. It is very easily put in action by a slight friction with a dry hand, a piece of leather, a rough skin of a hare, a cat, or some other animal. It is as easy to excite with this machine a negative as a positive electricity. It has the advantage of being capable at almost all times of affording at pleasure such a force of electricity as is wanted, even to such a degree, that the metal plate is no longer able to contain all the electric fluid communicated to it; but throws it out every way, either upon the metal upon which the resinous cake is usually fixed, or into the air: and this increase of electrical power is obtained by the easiest means; for instance, by charging with the Electrophore a coated phial, and placing it afterwards upon the resinous cake itself, or upon the metal plate placed upon the resinous cake (provided the metal plate be less in circumference than the resinous

nous cake, and no metallic communication exiſt between the metal plate and that metal upon which the cake is faſtened). If the knob of the phial, thus placed, be touched by the finger, and then taken away, holding it by the knob, the force of the electrophore is found to be remarkably increaſed.

But a more pleaſing way of increaſing the electrical force of this inſtrument is by transferring alternately the metal plate from one reſinous cake to another, and touching it after it is placed upon the cakes. By this method both cakes acquire continually more and more electricity; ſo that in a ſhort time, by this alternate tranſlation, the metal plate returns from either cake quite overcharged; and thus Leyden phials may be charged by it very ſtrongly, and even ſo as to break them. It is very remarkable, that by this method the metal plate returns from one cake in a poſitive, and from the other in a negative ſtate.

This manner of increaſing the two electricities was found out by my learned friend Dr. KLINKOCH, profeſſor in the Univerſity of Prague, ſoon after I had given him a deſcription of this new inſtrument, which I had received from his Royal Highneſs the Arch-duke FERDINAND a very little while after Mr. VOLTA had invented it.

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It is true, that Father BECCARIA had a long while ago excited an electricity, almoſt perpetual, by two panes of glaſs, one placed upon the other, each having but one metal coating, and joined together ſo that no metal was placed between the two glaſſes. Theſe two glaſſes being applied to a prime conductor of an electrical machine, ſo as to receive a charge in the ſame way as one glaſs coated on both ſides is to be charged, afford numberleſs ſparks from both coatings, after the two glaſſes have been diſcharged in the common way, by making a communication between the two coatings.

In order to produce theſe ſparks, the two glaſſes muſt be ſeparated from one another, ſo as to avoid touching the coatings in the moment of ſeparation. Each of the coatings will give a ſpark, which may be repeated after having replaced the two glaſſes one upon another. After the two glaſſes have been thus joined again, and touched after their conjunction, another ſpark is obtained from both after their ſeparation; and theſe ſparks may be thus repeated almoſt *ad infinitum*; ſo that theſe two glaſſes, once excited, ſeem to be an unexhauſted ſource of electrical ſparks. Father BECCARIA calls this experiment *electricitas vindex*. Whether this denomination be a proper one to convey ſome idea of what is underſtood by it, I will not now undertake to determine.

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The ſame Father BECCARIA had alſo found, that the coating of a glaſs, after being diſcharged, was able to give new ſigns of electricity, when taken off by means of ſilk ſtrings.

Some other experiments were made many years ago by Mr. CIGNA of Turin, and by ſome other electricians, which have a great deal of ſimilarity with the electrophore.

But as the inventors of theſe experiments did not adapt them as an electrical machine, they do not diminiſh at all, in my opinion, the honour which Mr. VOLTA deſerves, for having enriched the electrical apparatus with a very ſimple and handy machine, continually ready to excite as ſtrong an electricity as is requiſite for the moſt ordinary purpoſe.

The novelty and ſimplicity of the machine could not but ſtrike every electrician; I cannot expreſs how much I was pleaſed with the firſt ſight of it, and with what eagerneſs I ſet about endeavouring to underſtand the nature of it. I analyzed it in various ways: I compared it with Father BECCARIA's *electricitas vindex*, with an ordinary coated glaſs, and coated reſinous electrics.

Some electricians, puzzled with the ſtrange phenomena which it affords, thought it over-turned entirely the almoſt univerſally received theory of Dr. FRANKLIN, and

and that it could not be underſtood but by eſtabliſhing new principles.

After conſidering the matter maturely I thought, that theſe phenomena, though at firſt ſight extraordinary, could be explained by the ſame principles which were already received by almoſt every philoſopher.

But before I proceed to my intended explanation of the moſt obvious phenomena of the electrophorus, I muſt beg leave to ſet down ſome conſtant laws, which nature obſerves in the various motions of the electric fluid, and to which electricians do not ſeem to give a ſufficient attention.

1. The electric fluid exiſts in all ſubſtances, in a certain quantity, which is natural to them.

2. The electric fluid is repulſive of itſelf, that is to ſay, each particle of electric fluid tends to recede as far from another particle of the ſame fluid as it can.

3. The ſtate of electricity of a body is that in which it has acquired more electrical fluid than the neighbouring bodies, or in which it has leſs of this fluid than the ſurrounding bodies.

4. In the firſt caſe the electrical fluid tends to expand itſelf through all bodies near it, which can by their nature receive it. In the ſecond caſe, the electrical fluid of all the ſurrounding bodies finding leſs reſiſtance towards a body

a body negatively electrified, or having loſt a part of its natural ſhare of electricity, ruſhes towards that body, and tends to diffuſe itſelf through it, and thus to diſpoſe itſelf into an equilibrium.

5. The reaſon why the electric fluid, exiſting every where, ſeems to remain inactive in the common ſtate of nature is, becauſe all other bodies having their ordinary ſhare of this fluid, an equal preſſure exiſts on all ſides. Thus, if all the bodies upon the earth were to acquire more or leſs of electric fluid at the ſame time, in equal proportions, no electrical phenomena would be the conſequence of ſuch a ſtate; becauſe the preſſure being every where equal, the repulſive force of the electrical particles would be every where balanced. Thus two bodies, both negatively or both poſitively electrified, will not give a ſpark to one another: they only recede more from one another, becauſe the other ſurrounding bodies are not in the ſame ſituation with them. This aſſertion ſeems to be illuſtrated by Father BECCARIA's electrical well *(puteus electricus)*, which is nothing but a metal veſſel electrified, in which two cork balls are ſuſpended by ſilk threads; the balls do not ſhow ſigns of electricity within the cavity of the veſſel, becauſe the electric fluid preſſes equally on every ſide.

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6. All non-conducting bodies may acquire on each part of their fubftance more or lefs of the electric fluid, as well as conducting bodies, at leaft to a certain proportion; but they do not allow it to pafs freely through their fubftance or over their furfaces.

7. All bodies whatfoever are fufceptible of electricity, pofitive and negative indifferently, either by exciting them by friction or any other way, or by bringing them within the fphere of action of a body already electrical; fo that even metals, the beft conductors, may be as eafily excited by friction, if infulated, as glafs or fealing wax. The only material difference between the conducting and non-conducting fubftances feems to be, that the electricity does not fpread itfelf fo eafily and fo rapidly through or upon thofe bodies which are non-conductors as upon thofe which are conductors. An electrical fpark thrown upon the furface of a piece of metal infulated, of whatever length it be, diffufes itfelf equally through the whole mafs, if this metal be left to itfelf out of the fphere of action of another body charged with electricity. The whole electricity communicated by this fpark is difcharged at once by touching any part of the fame metal. On the contrary, electricity feems rather to ftick to that part of a non-conducting body to which it is applied, fpreading but flowly and unequally over its furface, from

which it may be taken by degrees, by touching thoſe parts to which it was applied. There are ſome bodies which ſeem to be of a middle ſtate between theſe two, *viz.* through which the electric fluid propagates as through good conductors, but ſlowly, ſuch as common wood, moiſt air, and many other bodies. Electricity ſeems to diffuſe itſelf through theſe bodies almoſt as ſugar and ſalt diffuſe themſelves through water, ſpreading farther and farther through the liquid.

8. All thoſe bodies, which are non-conductors, ſeem to acquire a ſtate of electricity with ſome reluctance; and, after they have acquired it, to hold it more tenaciouſly, or to part with it with more difficulty, than conductors. One touch takes away all the electricity of a metallic body, but does not abſolutely convey away all the electricity of a piece of glaſs or another electric body, ſuch as ſealing wax, amber, &c. The metal plate of an electrophore takes almoſt no electricity at all from the reſinous cake, if it be lifted up without having been touched when it was upon the cake.

9. All reſinous bodies, ſilk and many others, retain more tenaciouſly their ſtate of electricity than glaſs, however dry. Thus a piece of glaſs excited is almoſt quite deprived of its electricity by a conducting ſubſtance

being applied to it; but a refinous body, though touched, retains ftill a great fhare of its electricity.

10. A conducting body infulated, being placed within the fphere of action of an excited non-conducting body, or even in contact with it, acquires at the fame time two contrary electricities; *viz.* the part in contact, or very near the non-conducting electrified body, acquires a contrary electricity to that of the non-conducting body, at the fame time that the oppofite or farthermoft extremity is poffeffed of the fame electricity with the conducting body.

11. A conducting body infulated, being in contact with another conducting body excited with either electricity, acquires the fame electricity throughout its whole extenfion, or divides with this body its electricity equally.

12. But an infulated conducting body, being only in the fphere of action of another electrified conducting body, acquires, as in the firft mentioned cafe, two different electricities at the fame time; *viz.* towards the electrified body it acquires a contrary electricity, and at the oppofite extremity it acquires the fame kind of electricity with the electrified body.

It feems therefore to be a law of nature, that the electric fluid which is accumulated upon a body, and finds an

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obſtruction in its free paſſage to another neighbouring body by the interpoſition of a non-conducting body (ſuch as dry air, glaſs, &c.) forces by its repulſive power the electric fluid naturally contained in all bodies to the furthermoſt extremity of the neighbouring body, ſo as to excite in its neareſt extremity a kind of defect of the electric fluid, or a kind of vacuum, till at laſt the accumulation of the electric fluid becomes ſo great upon the electrified body, that it overpowers the reſiſtance of the intermediate non-conducting ſubſtance, forces its way through it, and ruſhes in the form of a ſpark upon the neighbouring body.

If the electric fluid be thrown upon the ſurface of a pane of glaſs, coated on both ſides with a metallic ſubſtance, ſuch as tin-foil; the fluid, finding an obſtruction to its paſſage through the body, is crouded upon that ſurface of the glaſs which has received it; forces the electric fluid out of the other ſurface, if ſome conducting body is near it, or in contact with it, and can convey it away; till this fluid becomes ſo much crouded upon that ſurface as to overpower the reſiſtance of the glaſs, and to force its way through the ſubſtance of the glaſs, in order to diffuſe itſelf upon the other ſurface, upon which was produced a kind of vacuum. The glaſs being thus rent is no longer able to be what is called charged; but after the crouded

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electrical fluid of a prime conductor has in the same manner rent the plate of air (which obstructed to a certain degree its free passage between the prime conductor and the neighbouring body) by giving it a spark, the same spark may be repeated at pleasure, because the opening formed by the spark through the plate of air is immediately shut up again according to the nature of all fluids.

If an insulated conducting body be situated in the manner described, so as to possess at its different extremities a contrary electricity, it may impart to any other body brought in contact with it, or within its striking distance, a share of that electricity which it has acquired at its farthermost extremity. The former body, so touched, has effectually lost that part of electrical fluid which was in a certain manner crouded upon that extremity; and therefore being taken out of the sphere of action of the excited body, as, for instance, a prime conductor, after having thus lost a part of the electric fluid crouded upon its extremity, is found to possess a negative electricity if the excited body had a positive, and a positive if the excited body had a negative one.

Thus we see how far we must believe what is commonly affirmed as a fact, that a body, plunged in the atmosphere of an electrified body, acquires a state of electricity contrary to that of the electrified body. If the body plunged

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in this atmoſphere be of a ſmall extent, it will be found ſo to all appearance, becauſe the two extremities of a ſmall body cannot be ſeparately examined; whereas a body of a certain extent exhibits in a very perceptible manner the two diſtinct electricities. The reaſon of this wonderful phenomenon is to be underſtood by the principles adopted, and may without much attention be underſtood, if we ſuppoſe the excited body to be in a poſitive ſtate of electricity; for in this caſe the atmoſphere of electric fluid ſurrounding the excited body forces by its repulſive quality the electric fluid of the neighbouring body towards its farthermoſt extremity, and thus accumulates or crouds it upon that extremity, from which extremity it is therefore ready to fly off upon any other body, which is of a nature to receive it, being brought near enough.

If the excited body be in a ſtate of negative electricity, the explanation is not ſo obvious as in the poſitive caſe: it requires ſome more attention to conceive what paſſes. The excited body, having loſt a part of its natural ſhare of electric fluid, a kind of vacuum, if I may call it ſo, takes place upon this body. The electric fluid of any other body being in its natural ſtate, and therefore in a kind of inactivity, confined as it were within its limits by the electric fluid of all the ſurrounding bodies, is ſet at

at liberty, exerts its natural repulſive quality towards that body, upon which it does not find a ſimilar quantity of electric fluid reſiſting its ſpring, or its elaſtic and repulſive quality; it therefore ruſhes towards that kind of vacuum which exiſts upon a body negatively electriſied; and thus the electric fluid of this body, loſing its natural ſtate of equilibrium, and accumulating itſelf towards the vacuum, produces there a real poſitive electricity, at the ſame time that the oppoſite extremity has a negative one.

Before I go farther, I muſt ſpeak one word more of that particular quality of conducting bodies, by which they receive, with a kind of reluctance, either ſtate of electricity; and, after having received it, part with the ſame with as much ſeeming difficulty. This quality, not unknown to attentive electricians, who muſt have obſerved it, has commonly appeared ſomewhat extraordinary and difficult to be believed by many electricians, to whom I have happened to explain my theory of the electrophore. As this quality is the foundation of this theory, I conceive it will not be amiſs to demonſtrate it by facts.

The firſt part of this inherent quality of non-conducting bodies, receiving a ſtate of electricity with more difficulty than conducting bodies, is eaſily ſhewn by the following ſimple experiment: a piece of dry glaſs, held

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near a prime conductor, will receive no electricity, or almost none, at the ſame diſtance as that at which a piece of metal or another conducting ſubſtance will have received a conſiderable degree of electricity, or even a full ſpark.

The ſecond part of this inherent quality may be thus demonſtrated: a piece of metal inſulated, as, for inſtance, the metal plate of an electrophore, placed upon the cake of reſin excited with a conſiderable degree of electricity, will not receive any electricity at all, or only a faint one, when it is ſeparated from the cake without having been touched when it was in contact with the cake, or in the ſphere of action of the cake, though it was really in a ſtate of actual electricity all the time it was upon the plate. Now, if the cake of reſin did part as eaſily with its ſtate of electricity as the metal plate, it would leave a conſiderable degree of electricity upon the metal plate; the more as it is well known that the metal does not at all reſiſt the receiving of it.

Though it would be perhaps in vain to attempt a farther explanation of this inherent quality of non-conducting bodies, yet it will be eaſy to illuſtrate this law of nature by an example of another inherent quality in all matter, which Sir ISAAC NEWTON calls the *vis inertiæ*; and is a *vis inſita*, by which matter reſiſts being put in motion, and when it is once put in motion requires as much force

force to ſtop its motion as it required to be brought from a ſtate of reſt to that of motion.

Let us now conſider attentively the ſtate of a body ſituated, as I have before deſcribed, in the ſphere of action of an excited electric; as, for inſtance, a cake of reſin, a flat glaſs, or any other non-conducting ſubſtance; or, in other words, let us conſider the ſtate of the metal plate placed upon the reſinous cake of an electrophore, ſuppoſing this cake to be excited with a poſitive electricity; which electricity it acquires eaſily by ſliding the knob of a Leyden phial, charged in the common way, over its ſurface and by various other ways. The ſuper-abundant electric fluid of the cake repels the electric fluid of the metal plate to its farthermoſt extremity, and excites there an accumulation of that fluid; or, in other words, produces there a poſitive electricity, whilſt it produces a negative electricity at the ſurface in contact with the cake.

If in this condition a conducting body be brought in contact with the metal plate, or within its ſtriking diſtance, it receives a ſpark from it; which ſpark is the electric fluid of the metal plate crouded upon the extremity of the metal by the repulſive force of the ſuper-abundant electric fluid of the cake.

If the metal plate be touched at that ſide where it is really in a negative ſtate, it will, notwithſtanding, part with its accumulated poſitive electricity; becauſe the repulſive power of the atmoſphere of the cake will force this crouded electric fluid out of whatever part of the metal is touched, the electric fluid paſſing through metals very freely.

The metal plate, thus deprived of the electric fluid crouded upon it, becomes in a negative ſtate; but the repellent power of the electric fluid of the cake continuing to act upon the metal plate, forces what remains in it towards the farthermoſt extremity, ſo as to produce much the ſame ſtate as it had before it was put upon the cake; ſo that the negative ſtate, in which it is in reality, cannot appear but when this metal is taken out of the preſſing action of the atmoſphere of the cake; and therefore the metal plate being removed, by the inſulating handle, from the cake, gives evident ſigns of having loſt a part of its natural ſhare of electric fluid; or, in other words, of being electrified negatively, the reſinous cake being more tenacious of the ſtate of electricity, which it had acquired, than the metal plate.

If the reſinous cake be in a ſtate of negative electricity, (which it acquires by a friction either with a dry hand, a piece of leather, or a rough ſkin; or by ſliding the nega-

tive part of a charged phial upon it, or by many other ways) the contrary muſt happen, *viz.* the electric fluid of the metal plate, finding a kind of vacuum upon the reſin cake, ruſhes upon it, and thus leaves its oppoſite extremity in a negative ſtate.

A conducting body, having its natural quantity of electric fluid, being brought near this metal plate, gives it a ſpark, which ſpark the metal plate retains as an additional quantity. If the metal plate be afterwards ſeparated from the cake, it muſt retain this additional quantity which it has received from the approaching body; becauſe the reſinous cake being, from its nature, more tenacious of the ſtate of electricity acquired than the metal, remains thereabout in the ſame condition as it was before the metal plate was placed upon it; but the metal plate, having acquired an additional quantity in the time it was placed upon the cake, carries with it this quantity, and muſt therefore return from the cake in a poſitive ſtate.

This confirms what I ſaid before, that in the firſt caſe the cake of reſin does not quit readily the electric fluid which it had acquired; and, in the ſecond caſe, does not ſteal from the metal plate the electric fluid which it had loſt.

What happens to the metal plate placed upon the reſinous cake happens alſo to the metal upon which the reſinous cake is commonly fixed; but the reverſe muſt

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take place, that is to ſay, when the upper plate is taken off the cake in a poſitive ſtate, the metal under the cake muſt be found in a negative ſtate, if the electrophore be placed upon an electrical ſtand.

It may be aſked, what difference there is between an electrophore and a coated phial, or a flat glaſs coated on both ſides and charged? I anſwer, that there is none at all, if both or only one of the metallic coatings can be taken off by ſilk ſtrings, a piece of ſealing wax, or any other inſulating ſubſtance. The very ſame day I received the electrophore ſent to me by his Royal Highneſs the Archduke FERDINAND from Milan (which electrophore was a thin reſinous cake ſtuck upon a flat piece of metal, to which was adapted a metal plate furniſhed with a glaſs handle to lift it up) I produced the ſame appearance by a common pane of glaſs and the metal plate of the electrophore; but ſoon finding that glaſs, however dry, quickly loſes its electricity (probably from its eaſily attracting moiſture from the air) I tried to cover it with a reſinous ſubſtance, or to varniſh it over with a hard copal varniſh; by which means it was eaſily excited by friction, and retained a long while the electrical power, though not ſo long as the reſinous cake.

I will now explain the nature of an electrophore in a manner more familiar to electricians, who underſtand

the received theory, by taking inſtead of an electrophore only a common pane of glaſs, adapted as a magical picture, with this difference only, that both coatings may be taken off by ſilk ſtrings faſtened to them, or by pieces of ſealing wax. Having eſtabliſhed a free communication between the common ſtock and the under coating, apply the upper coating to the prime conductor of an ordinary electrical machine, the pane of glaſs is charged in the common way. The prime conductor has forced a ſuper-abundant quantity of electric fluid upon the ſurface next to it, by means of the coating, and as much is forced out of the oppoſite ſurface, and driven into the common ſtock. Open a metallic communication between the two coatings, and inſtantly the glaſs will be, as we call it, diſcharged: and, indeed, it is ſo to all appearance; but, if we examine more accurately what has happened, we ſhall find, that the upper metallic coating has parted, by the diſcharge, with all the electric fluid which the prime conductor had forced upon it, and even with that part of its own electric fluid which the repellent power of the ſuper-abundant electric fluid, communicated to that ſurface of the glaſs by the force of charging, had driven into it; and that the under coating has recovered as much as the glaſs had forced through it into the common ſtock, and has above that acquired or abſorbed that additional

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quantity which that ſurface of the glaſs, being brought into a negative ſtate, had drawn from the metal itſelf. And thus it will appear, that the glaſs had, in the diſcharging, by no means parted with that ſtate of electricity which it had acquired by the force of charging.

Now, glaſs and all electric ſubſtances receiving with more difficulty a ſtate of electricity, and parting with it with more reluctance, the conſequence muſt be, that, when the two coatings are ſeparated from the glaſs, ſo as not to be in the way of abſorbing or loſing the electric fluid by other conducting bodies being near them, the upper coating, which was poſitive when the glaſs was charged, and nearly in its natural ſtate, when after the diſcharge it remained in conjunction with the glaſs, muſt now give ſigns of a negative electricity, having loſt by the diſcharge a ſhare of even its natural electric fluid in the manner mentioned. The under coating, which was in a negative ſtate when the glaſs was charged, and (like the other coating) in a natural ſtate when after the diſcharge it remained in conjunction with the glaſs, muſt now, being ſeparated from the glaſs, be in a poſitive ſtate; becauſe it had abſorbed a quantity of electric fluid in the manner explained; which ſuper-abundant quantity it muſt take with it in the moment of ſeparation from the glaſs, becauſe the glaſs being unwilling eaſily to change

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its acquired ſtate of electricity leaves the metal without robbing it of that quantity of fluid which it had acquired.

If theſe two coatings, ſeparated from the glaſs, are brought near one another, they attract each other; a ſpark enſues, becauſe the coating, which has acquired a ſuper-abundant quantity of electric fluid, imparts it to the other, which had loſt as much; and thus a perfect equilibrium is reſtored between them.

If both theſe coatings are applied as before upon the ſame glaſs, a poſitive ſpark may be obtained from the uppermoſt coating, and a negative one from the other. If they are ſeparated again from the glaſs, as in the firſt caſe, the uppermoſt coating will afford a negative ſpark, and the undermoſt a poſitive; and theſe alternate ſparks may be continued a very long while.

This explanation or theory agrees perfectly with the experiments exhibited by our deceaſed member, the late Mr. CANTON, with elder pith balls hanging by linen threads from a wooden box, which balls are excited either negatively or poſitively by a piece of excited glaſs.

XLIX. *Obſervations and Experiments tending to confirm Dr.* Ingenhouſz's *Theory of the Electrophorus; and to ſhew the Impermeability of Glaſs to Electric Fluid. By* William Henly, *F. R. S.*

Read July 9, 1778.

DR. FRANKLIN has obſerved, " That " there is a great quantity of the elec-" trical fire in glaſs; that what it has it holds; and that " it has as much as it can hold: that what is already in " it refuſes, or ſtrongly repels any additional quantity: " that when an additional quantity is applied to one ſur-" face of a phial (for inſtance, by the atmoſphere of an " excited tube) a quantity is repelled or driven out of the " inner ſurface of that ſide into the veſſel, returning " again into its pores, when the excited tube with its at-" moſphere is withdrawn; and that the particles of that " atmoſphere do not themſelves paſs through the glaſs."

The following experiments, I think, remarkably il-luſtrate this, by ſhewing that bodies are very differently

affected by a fluid acting immediately upon them *through glafs*; or by acting upon them immediately *by the glafs*, as above mentioned.

EXPERIMENT.

A circular box, three or four inches in diameter, and a quarter of an inch deep, is furnifhed with a thin glafs for a top. In this box fcatter fome very fmall fteel filings, or fift them into it through a piece of writing paper, which has a number of holes pricked through it with a pin. Then apply one of the ends of a magnetic bar to the upper furface of the glafs; the filings will be inftantly attracted to the glafs, and remain there as long as the magnet is thus fufpended over them; but the moment it is removed, the filings fall to the bottom of the box, and there remain at reft. The glafs then being made perfectly clean and warm, let a fine piece of amber, fealing wax, &c. be ftrongly excited and applied to it as the magnet was in the former experiment; the filings will be inftantly in motion, and will continue fo for fome feconds. When their motion ceafes, withdraw the amber, &c. and the motion of the filings will be renewed, and continue as at firft; this fhews, I think, that in

both cafes, they really act as conductors of the electric fluid between the lower furface of the glafs and the bottom of the box, in order to reftore an equilibrium, as upon Dr. FRANKLIN's principles they ought to do; and that the electric fluid does not, like the magnetic, abfolutely permeate the glafs.

EXPERIMENT.

Take a clean, dry, thin phial, about four inches long, and one inch in diameter. In the cork of this phial fix a fmall loop of very fine iron wire. In the loop fufpend another wire, about two inches and an half in length, by a fimilar loop; and on the lower end hang a light round ball of the pith of elder or cork, and be careful to give the wire as free a motion as poffible. Let one of the ends of a fmall magnetic bar be brought near the fide of the phial, and the little ball will inftantly come to the glafs, and there remain as long as the magnet is held within the diftance of its influence. Remove the magnet, and the ball inftantly retires to, and remains in the center of the phial: then dry and warm the glafs, and let an electric ftrongly excited be applied to the fide of the phial, as the magnet was in the former experiment; the

ball inſtantly comes to the ſide of the glaſs, and there remains ſome ſeconds, and then returns to the center of the phial. Withdraw now the excited electric, and the ball inſtantly returns to the glaſs upon the principle before mentioned, which is more completely ſhewn by the filings in the little box.

EXPERIMENT.

Let a piece of thin glaſs be placed as a cover to a circular box, about ſix inches in diameter, and three quarters of an inch deep: put into the box twenty or thirty light balls of cork, or of the pith of elder; then, having made the glaſs very dry and warm, expoſe the ſurface of it to the electric matter iſſuing from the prime conductor to a good electrical machine, the balls will be inſtantly in motion, and will ſo continue for ſome time, the box being moved in ſuch manner that every part of the glaſs may be affected. Then remove the box, and the balls being at reſt, turn the glaſs, placing the upper ſurface downward; the balls will then inſtantly renew their motion. When this ſecond motion ceaſes, touch the ſurface of the glaſs near the center with a finger, or, which is better, with a round, ſmooth piece

of

of wood or metal, the balls will inftantly fly to either of thefe, and will frequently pile themfelves up between the glafs and the bottom of the box, eight or ten in a pile, and will remove themfelves, following the wood, &c. to different parts of the glafs, till the charge is exhaufted. Apply the glafs again to the conductor as before, and when the motion of the balls nearly ceafes, remove the glafs, and place on each furface a circular coating of metal, reaching within an inch of the edge of the glafs all round. Make a communication between thefe coatings, and the glafs will then fhew that it has been charged, and will give a very ftrong fhock: this proves, that the electric matter did not abfolutely pafs through the glafs, but only acted on the electricity inherent in it in the manner explained by Dr. FRANKLIN.

The direction of the electric matter, in the difcharge of the Leyden bottle, hath been fhewn in a variety of methods (fee Philofophical Tranfactions, vol. LXIV. and LXVII.); but I fhall here mention one which, I think, a very curious addition to the number. Mr. LULLIN, of Geneva, placed two wires, the one upon, the other under, a card, the ends of the wires, in contact with the card, being about an inch from each other. This apparatus

ratus being made a part of the circuit, a charged bottle of a proper fize was difcharged through it: when the charge paffed along the furface of the card from the end of that wire into which it was difcharged, till it came to the end of the other wire, and there pierced a hole through the card, paffing by that wire to the negative fide of the bottle; and this happened whether the bottle was charged pofitively or negatively. A learned and ingenious correfpondent of mine, the honourable FREDERIC CHRISTIAN MAHLING, counfellor of ftate at Copenhagen, has improved this experiment, by firft painting the card in a line about half an inch broad on each furface with vermillion. The charge paffing in this line (the card being previoufly well-dried) fhews its paffage by a black mark on the vermillion, the mark being on one fide of the card when the bottle is charged pofitively, and on the other fide of it when the bottle is charged negatively. To which I would add, that a line of light is feen upon one furface of the card through the whole fpace between the ends of the wires in one cafe, for inftance, when the bottle is charged pofitively. But no light is feen in the other cafe, that is, when the bottle is charged negatively, till the electricity burfts a hole through the card to get at the wire which is in contact

with

with the negative ſide of the bottle, as in this caſe the charge paſſes along the under ſurface of the card. If the card be placed vertically between two inſulated wires, as in the univerſal diſcharger, deſcribed in Mr. CAVALLO'S Treatiſe on Electricity, the experiment may be made with great facility and certainty. The card may be fixed on a bit of ſealing wax, or ſet in a piece of wood, ſawn to a proper depth with a fine tenon ſaw.

L. *Track of His Majesty's armed Brig* Lion *from* England *to* Davis's Streights *and* Labrador, *with Observations for determining the Longitude by Sun and Moon and Error of Common Reckoning; also the Variation of the Compass and Dip of the Needle, as observed during the said Voyage in* 1776. *By Lieutenant* Richard Pickersgill, *late Commander of the said Vessel; communicated by* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

N. B. The day is set down according to the log-book.

Days of the Month.	Time.	Latitude.	Long. by last observ.	Long. by ship's reck.	Therm. in the shade.	Error of common reck.	As read off quadrants: ☉'s alt.	As read off quadrants: ☽'s alt.	Distance observed.	Azimuths.	Variations.	N° of obs.	Errors of quadrant.	Winds.	Remarks, &c.
	h ′ ″	° ′	° ′	° ′	°	′ ″	° ′ ″	° ′ ″	° ′ ″	° ′	° ′				
		N	W	W			L.L.	L.L.		NW	W		☽ alt. + 1		
June 10	20 6 8	49 20	6 26	6 36	61	0 10	36 32 40	42 55 40	61 16 30	–	–	3	☉ – 1	S	Scilly isles about North of us.
	Noon.	49 23 A	7 5	7 15	61	0 10	–	–	–	–	–	–	–	S	Lizard East of us.
11	Noon.	49 20 A	8 36	9 3	63	0 27	–	–	–	–	–	–	–	NW	Squally with rain; a great swell.
12	7 P.M.	49 12	9 25	9 52	–	–	6 48 20	–	–	37 46	24 21	3	–	NW	An unsteady sea, though the compass stood pretty well.
	D° nearly.	49 12	9 26	9 53	–	–	4 25 0	–	–	34 37	24 39	6	–	–	
	Noon.	48 3 A	10 9	10 58	62	0 49	–	–	–	–	–	–	–	NW	Under courses.
13	Noon.	46 55 A	11 0	12 14	61	1 14	–	–	–	–	–	–	–	WSW	Much rain, &c.
14	Noon.	47 8 *	11 22	12 37	61	1 15	–	–	–	–	–	–	–	WSW	Strong gales with rain.
15	Noon.	47 59 *	12 12	13 43	60	1 31	–	–	–	–	–	–	–	SW	More moderate.
16	Noon.	48 22 *	13 36	15 33	62	1 57	–	–	–	–	–	–	–	WNW	Moderate and clear; a great swell.
17	Noon.	49 13 *	14 21	16 29	61	2 8	–	–	–	–	–	–	–	SW	Little winds and clear weather.
	7 P.M.	49 33	14 20	16 28	–	–	15 50 0	–	–	48 30	–	3	–	–	These I have not worked.
18	7 P.M.	49 33	14 20	16 28	–	–	15 35 0	–	–	55 40	–	3	–	NE	A fine pleasant day.
	Noon.	50 11 *	15 59	18 33	60	2 34	–	–	–	–	–	–	–	–	
19	Noon.	50 45 *	16 50	19 37	62	2 47	–	–	–	–	–	–	–	SE	Fine weather.
20	Noon.	52 2 A	19 5	22 31	59	3 26	–	–	–	–	–	–	–	SW	Hazy at times.
								L.L.							
21	7 31 0	52 30	18 42	22 27	–	3 45	5 51 50	32 6 50	54 1 50	–	–	6	☉ – 2 50	–	Considering the height of the Sun these not bad ones; air clear.
		52 30	19 10	22 27	–	3 17	4 13 0	30 10 10	54 1 30	–	–	6	☉ – 1 0	–	
	Noon.	53 21 *	19 16	22 50	57	3 34	–	–	–	–	–	–	–	NW	Coming on to blow hard.
								L.L.							
22	3 38 24	53 27	19 45	22 55	–	3 10	39 22 40	45 37 40	63 50 55	–	–	6	–	–	The log between these and the [illegible] pretty well.
	2 53 40	53 27	19 37	22 55	–	3 18	45 35 6	43 53 26	63 35 26	–	–	9	–	–	
	Noon.	53 45 A	18 16	22 8	58	3 42	–	–	–	–	–	–	–	WNW	Strong gales.
23	Noon.	54 53 A	16 53	21 8	59	4 5	–	–	–	–	–	–	–	WNW	[illegible] gales, vessel drifts very fast to the Eastward.
24	Noon.	55 36 *	14 30	19 47	61	5 17	–	–	–	–	–	–	–	NW	
								U.L.							
25	4 6 20	55 25	13 54	19 50	–	5 56	34 55 10	26 47 0	96 50 0	–	–	6	☽ + 8 30	–	Dip. Face E. mark up, 76 30; Face W. ditto, 75 0; Face W. mark down, 74 0; Face E. ditto, 74 30; 75
	4 14 8	55 25	14 6	19 50	–	5 44	33 50 30	27 27 0	96 53 25	–	–	6	☽ + 8 30	–	
	4 35 32	55 25	13 58	19 50	–	5 52	30 52 10	29 8 30	97 0 55	–	–	6	☽ + 8 30	–	
	Noon.	56 18 *	14 25	20 8	59	5 43	–	–	–	–	–	–	–	WNW	Fine Day.
26	Noon.	57 18 *	14 40	20 10	58	5 30	–	–	–	–	–	–	–	NNW	Moderate weather.
								U.L.					☽ + 1 30		
27	5 33 56	57 15	15 32	20 26	–	4 54	22 47 20	17 44 20	119 56 5	–	–	6	☉ – 1 0	E	A very good set.
	Noon.	57 4 *	16 31	21 7	58	4 36	–	–	–	–	–	–	–	NW	Foggy weather, like the banks of Newfoundland, water smooth.
28	Noon.	56 38 *	17 44	22 20	61	4 36	–	–	–	–	–	–	–	N	
29	3 P.M.	–	–	–	–	–	–	–	–	–	–	–	–	–	Calm, tried soundings, and got ground at 320 fathoms: drifted to the N.E. about two miles, and sounded again in 290 fathoms. fine white sand: at the same time saw a shag, gulls, and other signs of land not far from hence. By running about 19 miles NE by N. lost soundings, so bore away, calling it the Lion's Bank.
	Noon.	57 0	18 2	22 38	58	4 36	–	–	–	–	–	–	–	SW	Swell from the Southward.
30	Noon.	57 52 A	21 32	26 8	58	4 30	–	–	–	–	–	–	–	SE	Carried away the fore-yard.
July 1	Noon.	58 25 *	25 31	30 2	54	4 31	–	–	–	–	–	–	–	SSW	S.W. swell, lost all our birds.
2	P.M.	58 25	26 10	30 41	–	–	24 40 0	–	–	53 33	32 5	3			
	Noon.	58 20 *	29 35	34 6	54	4 31	–	–	–	–	–	–	–	SW	
3	P.M.	58 0	30 29	35 0	–	–	16 34 40	–	–	38 40	34 42	3	–	–	Very good observations and the vessel steady, when made.
	P.M.	58 0	30 29	35 0	–	–	15 35 30	–	–	36 30	35 16	6	–	–	
	P.M.	58 0	30 29	35 0	–	–	14 46 40	–	–	34 49	35 41	6	–	–	
	Noon.	58 3	30 56	35 27	57	4 31	–	–	–	–	–	–	–	NE	Cape Farewell's lat. 59° 38′ N. long. by obs. 42° 42″ W.
4	Noon.	58 38	34 24	30 55	48	4 31	–	–	–	–	–	–	–	NE	
5	Noon.	59 12	37 28	41 59	50	4 31	–	–	–	–	–	–	–	NE	
6	Noon.	59 11	41 16	45 47	48	4 31	–	–	–	–	–	–	–	ENE	The Eastermost part of it.
7	4 0 0	59 24	42 5	46 36	–	4 31	–	–	–	–	–	–	–	–	Saw Cape Farewell bearing N by W. by compass, distance about 7 or 8 leagues.

A continuation

A Continuation of the Track of His Majesty's armed Brig Lion, from Cape Farewell along the West Coast of Greenland, for the Determination of the Position of the Coast, as likewise the Dip, Variation, &c.

Day of the Month	Time.	Latitude.	Long. by last observ.	Long. by ship's reck.	Therm. in the shade	Error of common reckoning	Log time as read off quadrant, ☉'s alt.	☽'s alt.	Distance observed.	Azimuths.	Variations.	No of obs.	Error of quad.	Winds.	Remarks, &c.
	h ′ ″	° ′ ″	° ″	° ″	°	° ′ ″	° ′ ″	° ′ ″	′	° ′	° ′	°			
		N					L.L.	U.L.		NW					
July 6	18 52 22	59 46 0	46 47½	48 47	–	2 0 30	24 19 0 24 39 30 24 42 0 24 32 30	[illegible] 12 0 [illegible] 33 0 30 53 0 [illegible] 32 40	76 11 30 [illegible] 11 0 [illegible] 10 0 [illegible] 10 50	– –	– –	—	None.	N	Very good observation; water smooth; amongst a field of ice. Land from NE to SE, distance off 8 or 10 leagues, making in deep bays.
	Noon	59 44 0	46 42	49 10	40	2 28 0	– –	– –	– –	– –	– –	—	– –	SE	Clear weather; fast in the ice.
	6 0 0	59 53 0	46 19	48 47	–	– –	23 37 0 23 31 0 23 25 0 23 32 0	– –	– –	44 30 44 0 44 0 44 10	41 38	—	None.	– –	Water smooth; land at NE to SW 5 leagues. Dip, Face E mark end down, 76 5[illegible] Face W ditto, 77 [illegible] Poles changed, Face W ditto up, 76 15 Face E ditto, — 76 3[illegible]
9	Noon.	60 1 0	46 13	49 40	38	3 27 0	– –	– –	– –	– –	– –	—	– –	N	
10	Noon.	60 12 0	47 18	51 21	38	4 3 0	– –	– –	– –	– –	– –	—	– –	NNW	Hazy, no land in sight.
11	Noon.	60 30 0	47 38	51 41	36	4 3 0	– –	– –	– –	– –	– –	—	– –	Variab.	Much ice.
12	6 0 0	– –	– –	– –	–	– –	21 42 0 21 39 0 21 34 0 21 38 0			44 0 44 0 45 0 44 15	39 0	—	None.	– –	Cape Desolation E 4 leagues; soundings at 146 fathoms.
12	6 30 0	60 40 0	47 45	– –	–	– –	17 53 0 17 45 0 17 44 0 17 48 0	– –	– –	39 0 39 0 38 50 38 50	38 20	—	None.	– –	Very good.
							20 0 0 20 11 0 20 [illegible] 0 20 10 0			SE 58 0 58 0 57 50 57 50	41 0	—	None.	– –	These observations were made on [illegible] one of the ice glances, which [illegible] not [illegible] had any effect on the compass as some have [illegible] glances. Had very irregular soundings from 80 to 10[illegible] fathoms about 4 leagues off the land, which is high and [illegible]ged, being mostly covered with snow and ice.
	20 0 0	61 4 0	48 0	– –	–	– –	20 32 0 20 40 0 20 45 0 20 39 0	– –	– –	57 40 57 40 56 10 57 10	40 52	—	None.	– –	
	Noon.	61 28 0	48 9	52 12	40	4 2	– –	– –	– –	– –	– –	—	– –	E	[illegible]and from SSE to NE.
							15 28 0 15 33 0 15 39 0 15 33 0			55 0 54 0 54 0 53 4[illegible]	48 58	—	None.	– –	Dip of the needle, mean 80 50. The large ice glance NE about 5 or 6 leagues off.
13	19 0 0	62 4 0	48 0	– –	–	– –	18 4 0 18 11 0 18 15 0 18 10 0	– –	– –	52 0 52 30 52 30 52 20	49 46	–	None.	– –	Tried the dipping needle, the mean of the four sights gave 81 10.
							23 37 0 23 42 0 23 47 0 23 42 0			46 0 46 0 46 30 46 10	45 22	—	None.	– –	About 5 leagues off shore, the land high, and the sounds filled with frozen snow, even to the tops of the hills.
	Noon.	62 11 0	48 0	52 3	39	4 2	– –	– –	– –	– –	– –	—	– –	Calm.	Dip of the needle 81 30.
							25 4 0 25 0 0 24 58 0 25 0 0			NW 42 0 43 0 43 0 42 40	46 54	—	None.	– –	Many isles off this part of the coast, and soundings about 12 leagues off, though none close in.
14	7 0 0	62 12 0	48 15	– –	–	– –	17 56 0 17 46 0 17 40 0 17 47 0	– –	– –	28 40 27 40 28 30 28 3	48 17	—	None.	– –	Clear weather.
	Noon.	62 50 0	49 0	53 3	40	4 2	– –	– –	– –	– –	– –	—	– –	N	Land from N 30 E to S 2 E 3 leagues off.
15	Noon.	63 14 0	49 53	53 56	41	4 2	– –	– –	– –	– –	– –	—	– –	SW	Thick weather; land NE to SSE 4 leagues off.
16	Noon.	64 38 0	51 26	55 32	42	4 6	– –	– –	– –	– –	– –	—	– –	SW	Thick weather; fell in with a ridge of rocks, 28 miles E. from hence. This day's Southerly wind found the ship 17 miles to the N. of account.
17	Noon.	64 57 0	52 56½	– –	–	– –	– –	– –	– –	– –	– –	—	– –	– –	At anchor in an harbour, called Musketto Cove, until the 27th.
21	3 25 15	64 55 13	52 53½	– –	61	– –	Double being reflected hor: 68 48 0 68 18 0 68 0 0 67 40 0 67 18 0 67 10 0 67 52 20	–	Nearest L. 68 1 0 68 3 0 68 4 0 68 5 0 68 6 15 68 6 0 68 4 12	– –	– –	—	– –	– –	These observations were made on shore with a reflected horizon, and the latitudes settled by many good meridian altitudes; the weather fine and clear, so that I can answer for the observations, though not for the working.

A further

A further Continuation of the Track of His Majesty's armed Brig Lion, from Mosketto Cove, Greenland, to the End of Davis's Straits, &c.

Days of the Month.	Time.	Latitude.	Long. by last observ.	Long. by ship's reck.	Therm. in the shade.	Error of common reckoning	Log time as read off quadrants: ☉'s alt.	Log time as read off quadrants: ☽'s alt.	Distance observed.	Azimuths.	Variations.	No. of obs.	Error of quad.	Winds.	Remarks, &c.
	h ′ ″	° ′ ″	° ′	° ′	°	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′	° ′	°			
		N			61 in the day, & 54 in the night		L.L.	U.L.		NW					
July 21	3 43 4	64 55 13	53 0	— —		— —	reflected hor. 65 35 0 65 15 0 65 1 0 64 30 0 64 20 0 64 8 0 64 48 25	— — — — — — —	68 11 0 68 12 0 68 12 0 68 12 30 68 13 0 68 13 30 68 12 20	— —	— —	—	None.	- -	By these observations I deduce this Cove to lay in latitude 64 55 13 N. and longitude 52 55 30 W. Variation 50 56 0 W. Dip of the needle 81 22 30 Flow of the tide, spring, 14 f. — — — — neap, [illegible] f. High water full and change at 10h. 15″ A. M. Dip, Face E mark end down, 82 30 Face W ditto, 81 15 Poles changed, Face W ditto, 80 30 Face E. — — 80 15
21	4 0 0	64 55 13	52 56½	— —	61	— —	62 49 0	— —	— —	60 0	50 48	—	None.	- -	
21	4 0 0	64 55 13	52 56½	— —	61	— —	66 2 30	— —	— —	66 2½	50 24	—	None.	- -	
27	Noon.	64 49 0	54 25½	58 31	43	4 6	— —	— —	— —	— —	— —	—	- -	N	
28	Noon.	64 40 0	55 29	59 35	39	4 6	— —	— —	— —	— —	— —	—	- -	NNW	No soundings, clear weather.
29	Noon.	65 14 0	58 10	62 16	37	4 6	— —	— —	— —	— —	— —	—	- -	ENE	Little or no ice, and steady weather.
30	Noon.	65 38 0	59 30	63 36	44	4 6	— —	— —	— —	— —	— —	—	- -	- -	Close to a very extensive field of ice, and no appearance of land at the back; the dip of the needle as underneath: Mark end up, face E. 86 [illegible] — — — face W. [illegible] 40 Poles changed, face W. [illegible] — — — face W. 85 30
										SE					
31	19 0 0	65 48 0	59 10	— —	—	— —	15 50 0 16 2 0 16 29 0 16 7 0	— —	— —	45 0 43 40 43 0 43 50	54 0	—	- -	- -	
31	Noon.	65 49 0	59 8	63 14	29½	4 6	— —	— —	— —	— —	— —	—	- -	- -	Sailing along the field of ice to the N. E. rigging froze
Aug. 1	Noon.	65 46 0	58 19	62 25	40	4 6	— —	— —	— —	— —	— —	—	- -	- -	Lost sight of the ice.
2	Noon.	65 36 0	58 19	62 25	42	4 6	— —	— —	— —	— —	— —	—	- -	- -	Saw another large field of ice to the Eastward.
3	Noon.	66 53 0	58 8	62 14	40	4 6	— —	— —	— —	— —	— —	—	- -	S	These four days sometimes thick fog, and others clear weather, a heavy swell from the South.
4	Noon	67 43 0	58 50	62 56	36	4 6	— —	— —	— —	— —	— —	—	- -	- -	At 12 this night was in latitude 68 14, and nearly the same longitude as at noon.
5	Noon.	68 10 0	56 46	60 52	38	4 6	— —	— —	— —	— —	— —	—	- -	- -	Vast quantity of very large ice islands, and a great sea, with thick weather.
6	Noon.	67 32 0	58 41	62 47	40	4 6	— —	— —	— —	— —	— —	—	- -	- -	Fresh gales, thick weather, and ice islands.
7	Noon.	67 20 0	56 17	60 23	42	4 6	— —	— —	— —	— —	— —	—	- -	- -	Saw high land bearing N. E. distance 20 leagues, a great swell, and little wind.
8	Noon.	67 10 0	55 11	59 17	41	4 6	— —	— —	— —	— —	— —	—	- -	- -	Light airs all this day; the land appearing high, and in many places covered with snow.
9	Noon.	66 40 0	55 1	59 7	43	4 6	— —	— —	— —	— —	— —	—	- -	- -	Little winds from the S. W. about four leagues from the land soundings from [illegible] fathoms, sandy ground.
10	Noon.	66 40 0	57 15	61 21	40	4 6	— —	— —	— —	— —	— —	—	- -	- -	Standing in and out to get to the southward, wind S. W. blows strong.
										NW					
11	7 0 0	66 38 0	56 48	— —	—	— —	15 20 0 15 17 0 15 12 0 15 5 0 15 1 0 14 57 0 15 10 30	— —	— —	39 0 37 0 39 0 38 30 39 0 40 0 38 45	not worked.	—	- -	- -	When these observations were taken Mount Cunningham bore SSE. distance about six leagues, depth of water 65 fathoms, sandy ground.
11	Noon.	66 30 0	56 31	60 37	46	4 6	— —	— —	— —	— —	— —	—	- -	- -	Land from SE to ENE. seven leagues off no soundings.
12	7 30 0	66 26 0	56 15	— —	—	— —	12 4 0 11 47 0 11 37 0 11 26 0 11 19 0 11 14 0 11 34 30	— —	— —	23 30 24 30 22 30 22 20 21 0 21 50 22 32	56 54	—	None.	— —	When these observations were taken there was little ice in sight, the weather fine, sea smooth, the land from E. to SSW. which was high and about 12 leagues off, being that part named Christian Sound, and where the Danes for some years worked a silver mine, and as this coast has in many places strong appearances of iron ore, this may be one, and in all probability may occasion this high variation.
12	7 45 0	66 27 0	56 15	— —	—	— —	4 37 0 4 28 0 4 25 0 4 21 0 4 15 0 4 12 0 4 8 0 4 5 6 4 19 0	— —	— —	5 0 5 45 6 0 6 40 4 30 3 50 5 0 4 40 5 11	55 50	—	None.	— —	
12	Noon.	66 2 0	56 45	60 51	44	4 6	— —	— —	— —	— —	— —	—	- -	SW	Land in sight.
13	8 0 0	65 58 0	55 49	— —	—	— —	7 25 0 7 16 0 7 7 0 6 57 0 6 44 0 6 37 0 7 0 40	— —	— —	23 10 23 45 21 40 22 45 22 5 20 30 22 19	47 23	—	None.	— —	The extremes of the land from NE. to SW. distance six leagues.
13	Noon.	66 7 0	54 43	58 49	53	4 6	— —	— —	— —	— —	— —	—	- -	— —	Within one mile of the mouth of a rocky river, with a rapid tide
14	Noon.	65 12 0	55 52	59 58	50	4 6	— —	— —	— —	— —	— —	—	- -	SBE	Land from E. to ENE. very high.
15	Noon.	65 22 0	56 12	60 18	40	4 6	— —	— —	— —	— —	— —	—	- -	— —	Hard gales with much rain, and a great sea, wind SWBS, weather thick.
16	Noon.	— —	56 50	60 56	42	4 6	— —	— —	— —	— —	— —	—	- -	WSW	Thick close weather, with little wind.
17	Noon.	65 22 0	55 29	59 35	44	4 6	— —	— —	— —	— —	— —	—	- -	SSW	Foggy weather.
18	Noon.	65 3 0	54 2	58 58	—	4 6	— —	— —	— —	— —	— —	—	- -	— —	The land like Islands. Caught many large hollibutts.
19	4 0 0	64 32 0	53 50	— —	—	— —	— —	— —	— —	— —	— —	—	- -	— —	Dip, mark end up, face E. 83 30 — — — face W. 84 40 Poles changed, face W. 85 0 — — — — face E. 85 0

A Track of His Majeſty's armed Brig Lion, from Davis's Straits to the Coaſt of Labradore.

Days of the Month.	Time.	Latitude.	Long. by laſt obſerv.	Long. by ſhip's reck.	Therm. in the ſhade.	Error of common reck.	As read off quadrants ☉'s alt. L.L.	As read off quadrants ☽'s alt. U.L.	Diſtance obſerved.	Azimuths.	Variations.	No. of obſ.	Errors of quadrant.	Winds.	Remarks, &c.
	h ′ ″	° ′	° ′	° ′	°	′ ″	° ′ ″	° ′ ″	° ′	° ′	° ′				
Aug. 19	Noon.	64 43 0	53 25	58 31	41	4 6	– –	– –	– –	– –	– –	—	– –	– –	The land near Balls River in ſight, making in Iſlands.
							33 12 0	16 33 0	60 0 30						
							33 5 0	16 39 0	60 1 30						
							33 0 0	16 42 0	60 2 0						
							32 57 0	16 45 0	60 4 0						
19	2 16 0	64 34 0	52 30	– –	–	– –	32 54 0	16 47 0	60 4 0	– –	– –	—	– –	– –	
							32 50 0	16 51 0	60 5 0						
							32 49 40	16 42 50	60 2 50				None.	– –	Theſe obſervations are very good ones, yet as they give too little, and diſagree with the reſt, I did not pay much regard to them, nor alter the track, as I cannot anſwer for the working of them.
							32 42 0	16 59 0	60 5 30						
							32 36 0	17 3 0	60 6 0						
							32 31 0	17 7 0	60 7 0						
							32 27 0	17 10 0	60 8 0						
19	2 22 0	64 34 0	52 50	– –	–	– –	32 22 0	17 12 0	60 9 0	– –	– –	—	– –	– –	
							32 20 0	17 14 0	60 10 0						
							32 29 40	17 7 30	60 7 35						
20	Noon.	64 33 0	54 35	58 41	–	4 6	– –	– –	– –	– –	– –	—	– –	– –	A very heavy gale of wind, which obliged us to throw overboard the guns we had on deck.
21	Noon.	63 56 0	54 15	58 21	36	4 6	– –	– –	– –	– –	– –	—	– –	– –	This night, for the firſt time (the gale being over and fine weather), ſaw the aurora borealis, which began in the SE. quarter, but was neither ſo ſtrong nor ſo beautiful as in the ſouthern hemiſphere.
							5 50 0	10 40 0	84 9 0						
							5 19 0	10 38 0	84 11 0						
							5 6 0	10 36 0	84 11 0						
							4 54 0	10 32 0	84 12 0						
21	A.M.	63 36 0	53 20	– –	–	– –	4 42 0	10 25 0	84 11 0	– –	– –	—	– –	– –	By the obſervations on the 19th, I find the error of the common reckoning to have increaſed 55 miles, and by theſe to have increaſed 50 miles; therefore ſhall alter the track to the obſervations, as I think them a ſufficient confirmation of the truth.
							4 28 0	10 23 0	84 13 0						
							4 59 50	10 32 20	84 11 10						
22	Noon.	62 19 0	53 4	58 3	44	4 59	– –	– –	– –	– –	– –	—	– –	– –	High land EbS diſtance 12 leagues, which was the laſt we ſaw.
23	Noon.	60 58 0	53 14	58 13	–	4 59	– –	– –	– –	– –	– –	—	– –	– –	Moderate breezes, and no ice to be ſeen.
24	Noon.	60 2 0	52 28	57 27	46	4 59	– –	– –	– –	– –	– –	—	– –	ESE	Ditto, with miſts.
25	Noon.	58 4 0	52 46	57 45	52	4 59	– –	– –	– –	– –	– –	—	– –	E	Freſh gales.
26	Noon.	56 17 0	53 38	58 27	54	4 59	– –	– –	– –	– –	– –	—	– –	NE	Freſh gales.
27	Noon.	54 54 0	54 10	59 9	56	4 59	– –	– –	– –	– –	– –	—	– –	NE	Cloudy weather.
28	Noon.	53 4 0	53 15	58 14	56	4 59	– –	– –	– –	– –	– –	—	– –	– –	Saw the firſt ice Iſlands on the Labradore.
29	Noon.	53 7 0	52 6	57 5	60	4 59	– –	– –	– –	– –	– –	—	– –	WNW	Much ice.
30	Noon.	53 33 0	53 9	58 8	60	4 59	– –	– –	– –	– –	– –	—	– –	SW	Much ice.
31	Noon.	53 25 0	53 58	58 57	–	4 59	– –	– –	– –	– –	– –	—	– –	– –	Round Hill Iſland, bore NW. ſix or ſeven leagues, which being the eaſtern part of America, I make it to be in latitude 53 32 N. longitude 54 29 W. of Greenwich.

We anchored in Porcupine Harbour, where we ſtaid until the 26th of September, and from thence proceeded to England; which being a common rout, I ſhall conclude with a few general obſervations on this part of the world, ſo little known and ſo terribly repreſented by people who, in order to raiſe their own merit, make dangers and difficulties of common occurrences; merely becauſe the places are unknown, and there is little or no probability of their ever being contradicted. I do not mean this as a perſonal reflexion; but having diſcourſed with many of the maſters of Greenland veſſels, as well as their employers, and heard ſuch dreadful ſtories of thoſe countries, I cannot help remarking it as a circumſtance equally fooliſh and ridiculous, tending to miſlead thoſe who from a laudable principle would be benefactors to their country, but are deterred from it by ſuch repreſentations; and I appeal to thoſe ſeries of facts for the truth of my aſſertion. The weather in Davis's Straits is, in the ſpring and autumn, boiſterous; the ſeas run irregular, like the Gulph of Lions, and other places I could inſtance (that is ſhort and high); occaſioned (I imagine) by the narrowneſs of the Straits, the many impediments it meets with from the ice, and its being open to the ſouthern parts of the Atlantic Ocean. As the ſouth winds are always the ſtrongeſt, bring thick weather, and the greateſt ſea, ſo the northern ones bring fine clear weather, and are ſeldom ſtrong. I ſhall here finiſh theſe obſervations, with the particulars of them, &c. and ſhall communicate obſervations on the ice, the atmoſphere, the land of Forbiſher, and the probability of a north weſt paſſage, in a ſhort time.

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AN INDEX TO THE SIXTY-EIGHTH VOLUME OF THE PHILOSOPHICAL TRANSACTIONS.

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E R R A T A.

Page 50, line 5, *after* Mr. Charles Hutton, *insert* F. R. S.

136, line 5, *for* seventh, *read* eighth

146, line 6 from the bottom, *for* unhealty, *read* unhealthy

160, line 2, *after* Edward King, Esq. *insert* F. R. S.

162, line 7, *for* Sago tree *read* Sago tree

206, line 15, *for* was *read* were

242, line 12, 13, 14, *for* enclosing a letter—conclusion, *read* enclosing an Account of Mr. WILSON's Experiments on the nature and use of Conductors, addressed to HIS MAJESTY.

285, line 7 from the bottom, *for* maing, *read* maining

327, line 1, *for* coucluded, *read* concluded.

554, line 1, *for* Abtract, *read* Abstract

567, line 1, *for* Extract *read* Extract.

571, line 4, *after* George Lloyd, *insert* Esq. F. R. S.

574, 575, 576, line 2, *for* 1778, *read* 1777

577, Rain in February 18, *for* 1.079, *read* 0.079

587, ——— July 21, *for* 1.194, *read* 0.194

593, ——— October 18, *for* 0.902, *read* 0.092

598, ——— January *for* 1.029, *read* 1.039

——— ——— July, *for* 5.697, *read* 4.697

——— ——— whole year, *for* 25.371, read 24.381

815, line 5, *for* bing, *read* being

867, line 13, *for* these *read* their